VGA
Institution

JOURNAL

OF THE

INSTITUTION OF

ELECTRICAL ENGINEERS,

INCLUDING

ORIGINAL COMMUNICATIONS ON TELEGRAPHY AND ELECTRICAL SCIENCE.

PUBLISHED UNDER THE SUPERVISION OF THE EDITING COMMITTEE,

AND EDITED BY

W. G. McMILLAN, SECRETARY.

VOL. XXVII.—1898.

London:

E. AND F. N. SPON, LIMITED, 125, STRAND, W.C.

New York:

SPON AND CHAMBERLAIN, 12, CORTLANDT STREET.

1899.

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VOL. XXVII.

JOURNAL

OF THE

Institution of Electrical Engineers.

Founded 1871. *Incorporated* 1883.

VOL. XXVII. 1898. No. 132.

The Three Hundred and Eighth Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, Great George Street, Westminster, on Thursday evening, January 13th, 1898—Sir HENRY MANCE, C.I.E., late President, in the Chair.

The minutes of the Ordinary General Meeting held on Friday, December 17th, 1897, were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council:—

From the class of Associates to that of Members—

Max Binswanger-Byng.
Arthur L. Dearlove.
Alfred Herbert Dykes.
Alfred Horswill Gibbings.
Ernest Matthew Lacey.
Victor A. H. McCowen.
Arthur Barnes Mountain.
Charles Bottomley Smith.
Arthur Annesley Voysey.

From the class of Students to that of Associates—

Alfred B. Blakey.
A. D. Constable.
Ernest Rider Cook.
Walter H. de Winton.
Trevor Duesbury.
Charles Walter Fourniss.
Henry William K. Irvine.
C. H. McCarthy Jones.
Anthony Clark McWhirter.
Albert P. Pyne.
Arthur B. Rayner.
H. Skipwith.
George H. Starr.
Robert N. Tweedy.
Frank Twyman.
Robert Tervet.

Mr. R. H. Mance and Mr. A. Jacob were appointed scrutineers of the ballot for new members.

A donation to the Library was announced as having been received since the last meeting from Mr. Kapp, to whom the thanks of the meeting were duly accorded.

Mr. LATIMER CLARK: I desire to offer to the Institution of Electrical Engineers a series of six volumes of papers by our late lamented Honorary Member, Mr. Jacob Brett. I think you will find them of interest, because they prove, without possibility of doubt, that the English nation was the first to introduce submarine telegraphy to the world, and their value will certainly increase as time goes on. I am very happy to have this opportunity of presenting them while Sir Henry Mance still occupies the chair. I think his first act as President—and a very sad one—was to attend the funeral of our departed friend, and nearly his last act in the chair this evening will be to accept these volumes at my hands. I am one of the older members, and, being about to throw off harness, I feel it a duty to collect these unique and invaluable records, and to place them in hands where they will remain permanently secure.

These papers are not arranged chronologically, or indexed, but I have indicated at the commencement of each volume a few of the subjects of interest. Among other things they include a pretty complete history of many of Messrs. Brett's original companies, which no longer exist, with a completeness which could scarcely now be obtained elsewhere, and they must always remain among the important historical records of that period. Perhaps the most interesting among these is the lithographed fac-simile in vol. i. of the receipt for the original registration of the "General "Oceanic Telegraphic Company." It was registered by Mr. Jacob Brett in his own name on the 16th June, 1845. In addition to its very early date (being the first electrical company ever registered), the point of interest about it is that he actually registered it in the first instance as the "General Atlantic Telegraphic Company," and described it as intended "to form a connecting mode of "communication by telegraphic means from the British Islands

"across the Atlantic Ocean to Nova Scotia, the Canadas, the "Colonies, and Continental Kingdoms." One stands amazed at the boldness of such a conception in 1845, when no other telegraphic company had come into existence, and when the idea of an electric telegraph had scarcely penetrated the public mind. The original and first electric telegraphic company was not incorporated till 1846, and only transmitted its first messages by land in January, 1848; yet here we have the earlier idea of a full-blown Atlantic telegraph in 1845, and Messrs. Brett actually lived to carry it out. The name, however, was not allowed to remain. He appears to have obtained permission, probably on the day of registration, to cross his pen through the word "Atlantic" and to write over it the word "Oceanic." Anyone can satisfy himself on this point by referring to the original registry at Somerset House, or to the copies in vol. iv., pages 81 to 83.

As we have important business before us, I will only add a few remarks on Mr. John Watkins Brett's book, "On the Origin and "Progress of the Oceanic Electric Telegraph," published in 1858. It is now, I believe, somewhat scarce. After his death Mr. Jacob Brett published a new edition, and considerably extended its size by adding largely to the newspaper extracts and other public notices of their work, and several photo-lithographed copies of the original registration papers above alluded to, the Certificate signed by the French officials at the time of the completion of their first cable in 1850, and other interesting documents. This intended extension is alluded to in the first edition. There is no date to this second edition, but I find that Mr. Jacob Brett presented several copies to his friends from time to time, some as early as 1885. It is, consequently, much more scarce than the original volume, and appears never to have been published, for I found the larger portion of the sheets unbound after his death, when they came into my possession. I presented them to his former landlady—Mrs. Vokes, of 10, Sevington Street, Maida Vale—so that in the event of any jubilee demand they may be obtainable. I retained a few copies for myself, which I shall be happy to present to any technical institution or library which may be interested in the early history of submarine telegraphy. I am desirous of

distributing them impartially among foreign and English librarians.

The CHAIRMAN (Sir Henry Mance): The communication which has just been made must, I am sure, be interesting to all of us, and especially so to old telegraph members. I have had the pleasure of looking at some of these papers, and I can fully appreciate their value, and the interest with which they will be regarded in future years by members of this Institution. Did time permit, I could say a good deal with reference to the subject-matter of the volumes which are now so kindly presented to the Institution. It is, indeed, fortunate for us that these papers have fallen into the hands of one of our oldest members, who has so much the interest of the Institution at heart. I have now only to ask you to pass a hearty vote of thanks to Mr. Latimer Clark for his valuable donation to the Library.

The resolution was carried by acclamation.

The CHAIRMAN: My last duty, before vacating the chair, is a very agreeable one. I have now the pleasure of presenting the premiums which have been awarded during the past year.

Sir Henry Mance then presented the following premiums:—

The "Institution Premium," value £10, to Mr. W. M. Mordey, Member.

The "Paris Electrical Exhibition Premium," value £5, to Mr. John Gavey, Member.

The "Fahie Premium," value £5, to Mr. H. Benest, Associate.

Extra Premium, value £5, to Mr. Alexander P. Trotter, Member.

The Students' Premium, value £3 3s., to Mr. P. S. Sheardown.

Extra Premium, value £2 2s., to Mr. Frank Johnston.

Extra Students' Premium, value £2 2s., to Messrs. R. M. Sayers and S. S. Grant.

The Salomons Scholarship of £50, to Mr. Edward Ernest Tasker, a Student of the Technical College, Finsbury.

The "Willans Premium," value £25, to Mr. Mark Robinson.

In presenting this latter premium, the CHAIRMAN said: This is the first time this premium has been awarded. I congratulate

you, Mr. Robinson, on being the first recipient. I can assure you that it is especially gratifying to the Council that the author of the paper which they found to be the most worthy of this premium is one who was so closely associated with the late Mr. Willans, in memory of whose services to engineering and electrical science the premium was founded.

The CHAIRMAN: Gentlemen,—In vacating the chair, I feel I should be ungracious if I did not express my cordial thanks for the loyal support which I have received from the Council during my year of office. I beg, therefore, to thank them most heartily, collectively and individually. Some may imagine that a President resigns the chair with a certain amount of pleasure; but were I to use this commonplace expression, it would, I feel, be no compliment to you, neither would it in my case be true. I feel too highly the honour which the occupancy of this chair confers on the individual, but on this occasion I may admit feeling a certain amount of satisfaction that in vacating the chair it is my privilege formally to introduce to you a gentleman whose name is as familiar in our mouths as household words, and who has done more for the cause of electric lighting than any man living. I have much pleasure in introducing to you your new President, and to ask him to take the chair and favour us with his Inaugural Address.

Mr. Joseph W. Swan, F.R.S., then took the chair vacated by Sir Henry Mance.

Professor AYRTON: A most pleasurable duty devolves upon me this evening, of proposing that a cordial vote of thanks be given to Sir Henry Mance for the way in which he has fulfilled the duty that you committed to his charge a year ago. The Presidential year was ushered in telegraphically, and telegraphic has been its character. You know that the submarine cable has uncoiled its history several times during the past twelve months, and it has been patent how the ether waves—the Italian and English ether waves—have telegraphically collided and bounded away, due to their mutual repulsion, as smoke-rings are wont to do. Those who, like myself, have had the pleasure of serving on the Council with our late President must have been struck with the very earnest

way in which he has attended every committee, attended to every duty, as well as with the energy with which he has forwarded every suggestion to benefit the interests of this Society. All the well-known Mance's methods tend to brevity, and therefore you will appreciate why to-night I am brief. We have been most accustomed to connect these methods with testing cables; but during the last twelve months we have been testing Sir Henry Mance himself, and I think we may say that of the various Mance's methods the one that has struck us as being superior to all others is the Mance's method of filling the Presidential chair. I therefore cordially propose—"That the best thanks of the "members of this Institution are due to Sir Henry Mance for the "able manner in which he has filled the office of President during "the past year, and for his indefatigable attention to the duties "attached thereto."

Mr. LATIMER CLARK: I have great pleasure in seconding that resolution, for Sir Henry Mance is, like myself, an old telegraphist. On this point I cannot help observing that some of our more recent members, who have probably never given any thought to the subject of telegraphy, are inclined to regard it with a sort of half impatience, and to assume that it has become a matter of ancient history. This feeling has doubtless been much engendered by the circumstance that telegraphy had such importance that it soon became a monopoly. They direct their thoughts with exclusive interest to the apparently boundless prospects of discovery and invention which electric science is daily opening out before them on all sides. This is perfectly legitimate and natural, but at the same time I do not fully agree with it, or consider it just.

We are no doubt moving very rapidly, but, whatever the future may have in store for us, we have certainly not at present advanced far enough upon the new path to be in a position lightly to undervalue the work of our predecessors. There is still a great deal more to come out of the telegraph. It should be remembered that the Telegraphists first harnessed electricity and made it subservient to the practical purposes of man. They founded this powerful Institution. The advances made by them in

1837 and 1845 will always be historical, and loom large in the history of the world. By creating a new means of rapid communication between nations and countries, and providing new facilities for commercial intercourse, they have entirely changed the history of civilisation and the conditions under which we live, so that up to the present time telegraphy has contributed far more powerfully to the progress of mankind, and produced results immeasurably greater than those of all other electrical inventions whatsoever.

The Telegraphists not only gained for this country the credit due to their energy and inventive genius, but they did their work so well that we who come 50 years after them find this little island still a great telegraphic centre, and we still possess or control much the larger share in the submarine telegraphs of the world. It is the proper province of an Institution such as this to honour the memory of such men, and to preserve the records of their work.

The PRESIDENT: It was not necessary, Sir Henry, except as a matter of form, that I should announce the passing of this resolution. You heard yourself how readily the members responded to the proposition. They appreciate the altogether admirable way in which you have discharged the Presidential duties during your year of office, not sparing the time or thought their efficient discharge required. It seems to me that the members, in passing this resolution, acknowledge a debt and make a promise to pay—for certainly the obligation under which such services as you have rendered to the Institution have put them cannot be discharged by merely passing a resolution. I hope we may assume that the members who have so heartily and unanimously supported this resolution have the full intention of following your example and of giving without stint whatever time and thought is necessary to advance the interests of the Institution.

Sir HENRY MANCE: I can assure you I deeply feel the kind expressions which have fallen from the lips of the proposer and seconder; and still more do I appreciate, Mr. President, your kindly endorsement of the same. It only remains for me to thank you, gentlemen, for the cordial manner in which you have

signified your approval of what I have been able to do during my year of office.

The PRESIDENT then proceeded, amid great applause, to read his Inaugural Address.

INAUGURAL ADDRESS.

By J. W. SWAN, F.R.S., President.

Sixteen years ago, I had the honour of bringing under the notice of the Society of Telegraph Engineers the question of a new mode of electric illumination by means of incandescent lamps. Mr. Preece was in the chair. I am sorry he is not with us to-night, and especially for the cause of his absence; may he soon return with renewed health!

There was placed outside this building, at the back, a portable engine, of the farmyard type, and a Gramme dynamo, built—as all the dynamos of that time were—for lighting one or two arc lamps. This apparatus was managed by Mr. Radcliff Ward, who had the not easy task, marvellously well accomplished, of running the dynamo at the exact speed required for the lighting up of a number of incandescent lamps attached to a kind of fitting that has since become generally known as an "electrolier." This, the first installation of incandescent lamps in London, was carried out by Mr. Fleetwood. When the gas was lowered, and the current was turned on, there was an audible expression of surprise as the lamps lighted up; and when, after a breathing space, it was realised that the room was for the first time entirely lighted by incandescent lamps, the manifestation of satisfaction was, I remember, very strongly pronounced.

That occasion marked, in an emphatic manner, the beginning, or almost the beginning, of a movement that has gone on with increasing activity, until now it may be truly said that a great revolution in the means of producing artificial light for common use has been accomplished, a new and profitable industry has been created, and, incidentally, an impulse and inducement given to the larger and more general utilisation of electricity. We are confronted at almost every turn with evidences of beneficial

change, flowing, in some measure, from this source. Such change, for example, as that from the depressing darkness of the streets of London at night, in the pre-electric light time, to the almost daylight brilliance of many of them now—a change, I admit, not wholly effected without the aid of the arc light, nor even of the gas lamp; but, that the gas lamp shines with an *unwonted brightness*, and that the arc lamps are there at all, are facts not distantly connected with the very general use of these unobtrusive little bulbs, which were thought so wonderful at the time to which I am looking back, and are such universally familiar objects now. Sixteen years is not a long time in the history of industrial evolution, and yet what changes have occurred in the last 16 years! The entire space is crowded with electrical invention and electrical work, not confined to electric lighting, but extending over the wide and varied fields of electrical power transmission, electric traction, and electro-chemistry. The successful introduction of electric lighting, and the great incidental improvements made in the machinery for transforming dynamic energy into electric energy, gave the impulse required to produce the immense activity that we witness to-day.

The domain of electrical engineering has broadened. When this Institution was founded, telegraph engineering was its principal feature; later, there grew up the new branches of electric lighting, electric traction, and the electrical transmission of power. These have so flourished that, if they have not overshadowed the older branch, they have at least sheltered and supported it; and there is another branch vigorously growing and giving promise of immense enlargement—that of electro-chemistry.

This brings me to a subject of great interest to the electrical engineer, and especially to the young electrical engineer—"the "world is all before him, where to choose;" and, in my belief, a moderate proportion of those who are aspiring to make their mark as electrical engineers would choose wisely in making a very special study of that portion of the field within which lies the application of electricity to chemical manufactures.

The field is a wide one, and so far only a small corner of it has

been cultivated, but that portion is already yielding rich harvests. There are now three or four flourishing electro-chemical industries of capital importance—the electrolytic refining of copper, the electrolytic extraction of aluminium, the electrolytic recovery of gold, and the electrolytic production of chlorine and of soda. Besides these, there are *other* successful chemical manufactures which rest on an electrical basis. Their importance is great even now, and is increasing. They afford opportunities for the advantageous exercise of special knowledge and skill on the part of the electrical engineer, who may be called upon to design suitable apparatus for carrying out known processes, or to invent new or improved means of effecting some unattained but desirable end.

Considering the importance of this branch of electrical engineering, it seems to me—and I hope you may take the same view—that the time custom places at my disposal to-night will not be ill spent in a general review of *the rise and progress of electro-chemical industries.*

EARLY WORK IN ELECTRO-CHEMISTRY.

Two years hence there should be celebrated, in the city of Como, the centenary of Volta's great discovery, to which we owe the origin of electro-chemistry. Electrical phenomena had been diligently studied long before his time. But, if we except the action of the electric spark utilised by Cavendish to induce the combination of gases having an affinity for each other, no marked electro-chemical effect had been observed up to Volta's time. There was, in fact, no knowledge of phenomena due to the sustained operation of an *electric current,* as distinct from those due to *intermittent discharges.*

Closely following upon the announcement of the discovery of the voltaic pile, its analytical power was made known through the electrolysis of water by Carlisle and Nicholson. But it was Davy who first fully realised and demonstrated the transcendent power of the voltaic current to effect chemical decomposition. Davy made for ever memorable the year 1806 by the electrolytic extraction of potassium from potash. Distinctly prophetic as this

was of other far-reaching kindred discoveries, I suppose that not even the imaginative mind of Davy ever entertained the idea that out of this embryo would grow any of these great manufacturing processes that are to-day shaking the fonndations of some of the oldest and most important of our chemical industries.

The method used by Davy in this historic experiment has so close a bearing on my subject, and is so intensely interesting in itself, that I think you will not grudge the moment it will take to read an account of it in Davy's own words. He says: "A small "piece of pure potash which had been exposed for a few seconds "to the atmosphere, so as to give conducting power to the surface, "was placed upon an insulated disc of platinum, connected with "the negative side of the battery of the power of 250 of 6 by 4, "in a state of intense activity; and a platina wire communicating "with the positive side was brought in contact with the upper "surface of the alkali. . . :"

"Under these circumstances a vivid action was soon observed "to take place. The potash began to fuse at both its points of "electrization; there was a violent effervescence at the upper "surface; at the lower, or negative, surface there was no liberation "of elastic fluid; but small globules having a high metallic lustre, "and being precisely similar in visible character to quicksilver, "appeared, some of which burnt with explosion and bright flame, "as soon as they were formed, and others remained, and were "merely tarnished, and finally covered by a white film which "formed on their surfaces."

Davy, fortunate in almost everything, was supremely fortunate in his *assistant, Faraday*. Never, surely, in the history of experimental science did the mantle of genius fall on worthier shoulders than when Faraday became the successor of Davy, and the inheritor of his methods and of his work. Great, immensely great, as is the debt owed by electrolytic chemistry to Davy, the debt is doubly great to Faraday. To Faraday we owe the discovery of the law of electrolytic conduction, without which knowledge industrial progress in the field of electro-chemistry would have been impossible; and, above all, it is to Faraday that we owe the first principles of the dynamo—principles applied to

practical electrolytic work much earlier than is commonly supposed.

Even as early as 1842 there were at work in Birmingham, for the electrolytic deposition of silver and gold, power-driven electric current generators, based on the dynamo-magneto-electric principle discovered by Faraday. One of these machines I saw not long ago, still doing duty at Messrs. Elkington's factory. These ancient machines were not *called dynamos*; the term "dynamo" had not issued from that mint which, by the coinage of a word, seems to create the thing signified. But these so-called magneto-electric machines were, to all intents and purposes, dynamos; they transformed mechanical power into electrical power through the medium of magnetism, and the dynamo of to-day is their direct descendant.

During the 30 years following Faraday's discovery of magneto-electric currents, and its primitive application to electro-plating, I cannot recall in this connection any of those striking events which make a moment memorable, but the tools were being fashioned wherewith the way was to be cleared and the work of progress carried on.

Towards the end of that quiescent period, Wilde was building powerful machines for the electro-deposition of copper; and those great incentives to electrical engineering enterprise and progress, the telegraph and electric lighting, were already beginning to quicken the pace along the collateral lines of scientific and industrial advancement. To speak only of electric lighting, it should be noted that, in the fifties, Holmes and De Meritens had designed efficient, if costly, magneto-electric apparatus for lighthouse illumination.

The principle of magnetic self-excitation in an electro-magnetic generator was made known in 1867, and four years later the first really practical continuous-current machine was constructed by Gramme.

The two succeeding decades saw the evolution of the modern dynamo; and at the end of this period the critical point was reached when there was demonstrated, with sufficient clearness to captivate the commercial mind, the fact that for *lighting*, for

transmission of power, and for effecting several important chemical operations, *electricity*, as produced through the dynamo, by the steam engine or by water power, was a thing of *utility*, and could be turned in all these ways to commercial advantage.

These great uses of electricity have been for several years established on the secure basis of commercial success. This result has been reached through the co-operation of many minds, and especially by the union of the *skill* of the *mechanical engineer* with the *specialised knowledge* of the *electrician* and the *chemist*.

Out of this combination and concurrence of forces electrical engineering has grown, and, by making new demands, has reacted beneficially on purely mechanical engineering.

The requirements of electric lighting have largely contributed to those great improvements and economies, in electric power-producing machinery, and in the steam engine itself, which have materially assisted in bringing about the degree of success which has now been reached in electro-chemical industries.

Copper-Refining.

At the outset, 60 years ago, the only electrolytic industry then in existence was comprised in that small and closely related group, electro-plating, electro-gilding, and electrotyping. Since then, and comparatively in recent years, the principle of electrotyping has been applied to copper-refining. This has developed to such an extent that now one-third of all the refined copper required in the world is produced electrolytically. In 1896 the production was 137,000 tons. The product of one works alone—the Anaconda Works—was over 30,000 tons. One great advantage of electrolytic copper-refining over the old method is the saving of the gold and silver from the unrefined copper. But there is a further advantage, and one that electrical engineers especially appreciate, viz., the *higher conductivity* of electrolytic copper.

The first Atlantic cable was made with copper that had a conductivity only 40 per cent. of that of pure copper. At that time it was difficult to buy copper free from arsenic. So completely

Fig. 1.—General View of one Hall of the Anaconda Copper-Refining Plant

has the electrolytic method of refining copper altered the old state of things, that lately, when I wanted a small quantity of arsenical sheet copper, I had much trouble in procuring it.

The process of electrolytic copper-refining is, as you know, simply electrotyping on a grand scale. An impure copper anode is dissolved, and pure copper is deposited upon a cathode in an electrolytic bath of acid sulphate of copper solution.

The amount of power expended in copper-refining relatively to the product is small. In this respect it is greatly different from that other electro-chemical industry—perhaps next in importance to copper-refining—the extraction of aluminium. In electrolytic copper-refining almost the whole of the electric energy is expended in overcoming ohmic resistance, therefore the power required for a given output may be reduced to a small amount by increase of the size of the apparatus. Hence the location of copper-refining works is not greatly influenced by the consideration of the cost of power; other considerations generally prevail in the choice of locality.

The range of current-density within which reguline copper can be deposited is extremely wide. The size of the apparatus—regulated by a law analogous to Kelvin's law of balance, of the *cost of capital* against the *cost of power*—is, for a given output, usually large. It is found most economical to use a low current-density; the greater purity of the copper deposited under those conditions is an additional consideration determining that practice.

In electrotyping, where *power* is a less important consideration than *time*, current-density is usually much higher.

In this connection, I may mention experiments I made to ascertain how far it is possible to go in the direction of increase of current-density without detriment to the physical properties of the metal deposited.

I found that under proper conditions it was possible to obtain tough copper with a current-density ranging from 1 ampere to 1,000 amperes per square foot of cathode surface. The conditions necessary to be observed were, to adapt the strength of the solution to the strength of the current, using, of course, the

strongest solution with the largest current; and, when the current-density was high, to take suitable means to obtain extremely rapid circulation of the electrolyte. I found that regularity and smoothness of deposit were almost entirely dependent on the absence of solid particles held in suspension in the electrolyte, and that nodular excrescences could be entirely avoided by taking care that the electrolyte was free from solid floating particles. I found also, that an exceedingly rapid flow of the electrolyte over the cathode surface tended to the suppression of a crystalline condition of the deposit. This effect was most strikingly shown when the electrolyte was projected against the cathode surface with considerable force from a submerged jet. In the *Philosophical Magazine*, 1881, vol. xii., p. 300, Tribe published an exceedingly interesting series of observations on the distribution of the lines of conduction in a liquid undergoing electrolysis; these showed me the causes of the wasteful growths round the edges of electrotypes. By applying remedies suggested by Tribe's results, I was able almost wholly to prevent this waste, to obtain nearly complete uniformity in the thickness of deposits, and entirely to prevent excrescent marginal growths. The general principle followed was the restriction of the sectional area of the electrolytic bath to, as nearly as possible, that of the plate intersecting it, so as to prevent curvature of the lines of flow.

In considering this branch of the subject, the question occurs whether it is economically possible to take advantage of the greater purity and higher conductivity of electrolytic copper that has not undergone fusion after electro-deposition. The common practice is to fuse electrolytic copper and cast it into ingots, and then proceed to roll and draw the ingots into the various sizes of bars and wire required in electrical work. This treatment results in a slight loss of conductivity. Some years ago I worked out a process in which a copper wire stretched in an electrolytic bath was, whilst receiving a deposit of copper, continually subjected to the action of wire draw-plates. This resulted in unlimited extension of the wire without increase of its thickness: all the deposit went to increase the length; and this might go on to an indefinite extent. The original wire formed a core, which, as the

process proceeded, dwindled towards nothing. There are on the table some pieces of wire made in this way, in the different stages of its growth. I ascertained the possibility of producing wire in this manner; but even with a rapid rate of deposit, such as I was able to use, I found the apparatus would be excessively costly, relatively to the output; and, being allowed, by the kindness of Messrs. Bolton, to witness the method of wire-drawing employed at their works, I was so impressed by the rapidity and simplicity of their process as to feel that, looking at the matter from a non-scientific point of view, unless there was something much more to be gained than 1 or 2 per cent. extra conductivity, the play was not worth the candle. I do not know whether, by the method, proposed by Mr. Elmore, of cutting a spiral from an electrolytically deposited copper cylinder, a sufficient degree of economy of production can be obtained; but, so far, the ordinary process has not been interfered with by direct electrolytic methods of producing wire. Nevertheless, the greater purity and slightly higher conductivity of electrolytic copper that has not been subjected to the fusion treatment common in commercial practice, give to those attempts to produce wire from electrolytic copper that has not undergone fusion, at least a scientific interest and value.

Before I leave this subject of copper deposition, I must draw your attention to this mirror—one of the latest results of industrially applied electrotyping. It is made by depositing a thick backing of copper on a silvered glass matrix, and, after separation, coating the surface with palladium. This kind of mirror is intended as a substitute for the glass mirrors hitherto used in light projection. It has the great advantage over a glass mirror that a shot would not destroy, though it might damage it. The process has been worked out and patented by Mr. Cowper-Coles.

Electro-Deposition of Zinc.

Similar methods to those used in copper refining are now being followed in electro-zincing. The words "galvanising" and "galvanised" are much abused in their application to that useful

material *galvanised iron*. The coating of iron with zinc in the ordinary process of galvanising is, as we all know, not effected by electrolytic action, but there are, in practical operation, true galvanising processes, by which a hard and very adherent coating of zinc may be obtained without impairing the strength of the iron. The process is largely in use for galvanising boiler tubes. In one of several works where it is in operation, over 500 tons of tubes were galvanised in this way last year.

Direct Electrolytic Treatment of Ores.

A characteristic feature of electrolytic *copper-refining* is that the *anode* is formed of the *same kind of metal* as that *deposited*, and *dissolves* to keep up the supply of metal in the electrolyte. There is an equal and opposite action going on at the cathode and anode. But there is another class of electrolytic operations of perhaps even greater interest to the electrical engineer, and certainly of great economic importance, namely, that class in which the *ore*, and not the already reduced *metal*, furnishes the metallic supply to the electrolyte. This opens a very large subject, since there is included in it not only the extraction of copper, nickel, zinc, gold, aluminium, and sodium, but also the great question of the electrolytic production of caustic soda and chlorine, and other substances hitherto produced by purely chemical operations.

There have been many attempts to utilise the fact that copper matte or sulphide can be *cast* in the form of *plates* or *slabs*, and that such plates have a sufficient degree of conductivity to allow of their being used as anodes in an electrolytic bath. These attempts have not always been successful, but there is an interesting exception in the case of the copper-nickel mattes worked by the Canadian Copper Company, who refine copper and nickel electrolytically, and use the matte as anodes. The mattes contain about 40 per cent. each of copper and nickel, and 14 per cent. of sulphur, together with small quantities of silver, gold, and platinum. The power used in the production of 1 lb. of nickel is nearly 1 electrical horse-power-hour.

Gold Extraction.

Before the introduction of the cyanide process for the treatment of gold ore, various electrolytic methods, chiefly based upon the solvent action of electrolytic *chlorine*, had been proposed and worked; but the purely chemical cyanide process has largely, if not wholly, superseded these electrolytic methods for the primary treatment of gold ore. Messrs. Siemens & Halske, however, have patented and successfully introduced a method for treating the cyanide liquors from the tailings, or waste sludges, produced in cyanide gold extraction, and containing a very small amount of gold. An extremely dilute solution of cyanide is employed to dissolve the gold. This is afterwards subjected to electrolysis, with iron anodes and thin lead cathodes, with a current-density of one or two tenths of an ampére per square foot. This results in an almost complete recovery of the gold in an adherent form upon the lead cathode. When the required amount of gold has been deposited, the cathodes are removed, and the gold separated by cupellation. This process appears to be exactly suited to the clean quartzite ore of the Transvaal. Over 1,000,000 tons a year of tailings, such as were formerly discarded as useless, are now profitably treated by this process.

Zinc Extraction.

The extraction of zinc from its ores by electrolysis is a problem on the solution of which much ingenuity and a considerable amount of money have been expended. It is a tempting problem, inasmuch as the method in common use, of reducing the native sulphide or the carbonate of zinc to the state of oxide by calcination, mixing this with non-bituminous coal, and distilling in clay retorts at an extremely high temperature, is absolutely barbaric in its primitiveness and its wastefulness. The quantity of coal consumed in the smelting of zinc is more than five times the weight of the metal produced. The cost for smelting involved in the production of a single ton of zinc, including coal, labour, pottery, and stores, is generally not less than 80s.; and, as about 16 per cent. of the metal contained in the ore is unextracted, the loss of this at the present value of zinc ore makes an addition

of 56s. to the cost of production, and a total of about £7 16s. for smelting one ton of zinc. These figures are, of course, variable with the market value of zinc ore, also with the locality of the smelting works, and are based on the present value of zinc ore and the present cost of material and labour in England. Here would seem to be a great opportunity for the introduction of an improved method, and it is well worth considering again and again, notwithstanding past failures—and they have been many—whether this is not a case in which amelioration may be obtained by electrolytic methods.

A step has been recently taken towards this object by means of a process (the invention of Dr. Hoepfner) at present being worked by Messrs. Brunner & Mond. In this process zinc chloride is electrolysed; the products are chlorine and zinc. Such zinc is purer than ordinary commercial zinc, and will no doubt be welcomed by the users of zinc in primary batteries. A specimen ingot is lying on the table.

For the electrolytic treatment of that hitherto intractable class of ore such as the Broken Hill Mine produces, the mixed sulphides of lead and zinc, two processes deserve mention—one, the Ashcroft process, because very extensive preparations have been made for carrying it out on a large scale; and the other, that of Cowper-Coles. This process is in actual operation on a small scale, and we have here some of the results. An interesting feature of these deposits (there is an example on the table) is, that they have been made on aluminium cathodes and stripped off, the film of oxide preventing adhesion.

These bold attempts in new directions deserve success.

Aluminium Extraction.

One of the largest, the most important, and in many respects the most interesting, of the electro-chemical industries is that of the production of aluminium. In 1855 the price of aluminium was not far from the price of *silver*, when silver was twice its present value; now, bulk for bulk, it is the price of *copper*. It is no longer bought by the *ounce*, but by the *pound* or *ton*. The full measure of change has been brought about by the employ-

ment of electrolytic extraction. The purely chemical method had been highly elaborated before the electrolytic process was brought into competition with it, but the lowest price of the chemical product was three or four times the present price of the metal. It is an instance of an electrolytic method displacing a thoroughly elaborated and established chemical method, and of the enormous increase in demand that has followed a reduction of price. The production at the present time is, I estimate, not less than 2,000 tons a year. At least 10,000 horse-power is absorbed in this industry alone, and power to double that amount is about to be applied to it. There is every probability that new uses will be found for the metal, and that the manufacture will become a much larger one than it is at present. Professor Richards, well known as the author of the most complete work on aluminium, says, in a letter I recently received from him: "The end of the century sees "another metal added to the list of *common* metals; a metal "whose ore is as plentiful as that of iron, whose cost of production "is steadily decreasing, and whose uses are just as steadily "increasing. It is bound to stand next to iron in its production "and in its usefulness to mankind."

Although English enterprise was prompt to adopt and improve the original chemical process, the production of aluminium had wholly departed from us until the British Aluminium Company recommenced the manufacture in the electrolytic form, across the Border, attracted there by the advantage of cheap water power.

(Mr. Swan here called attention to a carbon anode and a number of most interesting aluminium products illustrating the manufacture, kindly supplied for the occasion by the British Aluminium Company, through the managing director, Mr. Ristori, to whom he tenders his thanks.)

The process by which aluminium is extracted, in *America*, on the *Continent*, and in *Scotland* to-day, is in principle exactly similar to that by which Davy extracted potassium from potash 92 *years ago*. The electrolyte is kept in a state of fusion by the electrically generated heat. There are nominally two processes in use, but the difference is extremely small—chiefly a slight difference in the composition of the electrolyte. That known as

Hall's process has as its distinctive feature an electrolytic bath composed of potassium fluoride in which alumina prepared from bauxite is continuously dissolved; while in Héroult's process the solvent of the alumina consists of cryolite, the double fluoride of

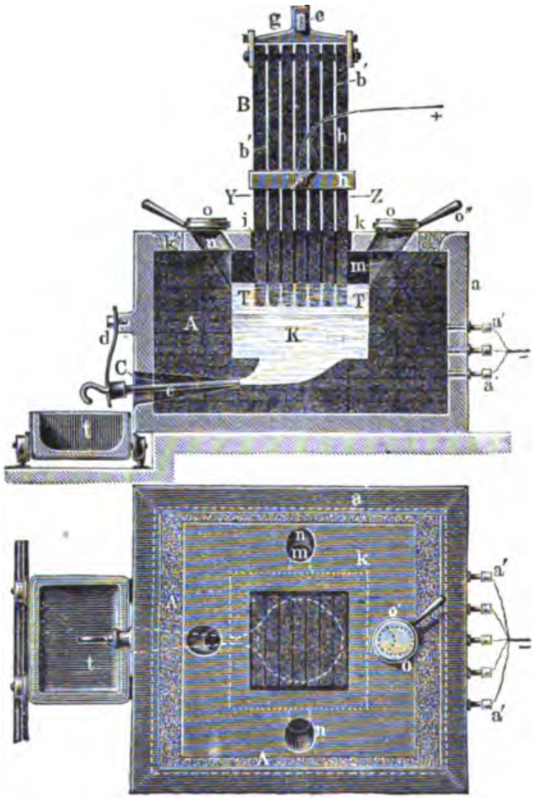

FIG. 2.—Héroult Aluminium-Reduction Furnace (in Section, and in Plan).

A, carbon lining to furnace, serving as cathode.
B, carbon anode, formed of parallel carbon blocks.
C, tap-hole for conveying the fused aluminium from the furnace to the truck, *t*.
K, the fused electrolyte.
m, *n*, channels for the introduction of alumina to replenish the bath; with covers, *o*, *o*.
a, conducting shell of furnace, with electrical connections, *a'*, *a'*.

aluminium and sodium. The electrolytic furnace consists of a carbon-lined iron box connected with the negative pole of a dynamo; this contains the electrolytic bath. Massive blocks of

carbon are connected with the other terminal of the dynamo, and form the positive pole. These are immersed in the bath of fused material, and nearly reach the bottom.

The carbon used in the manufacture of the anodes, and for lining the furnace, is required to be of great purity and hardness. The current-density employed is very large—about 700 amperes per square foot of cathode surface, about 8,000 amperes per cell.

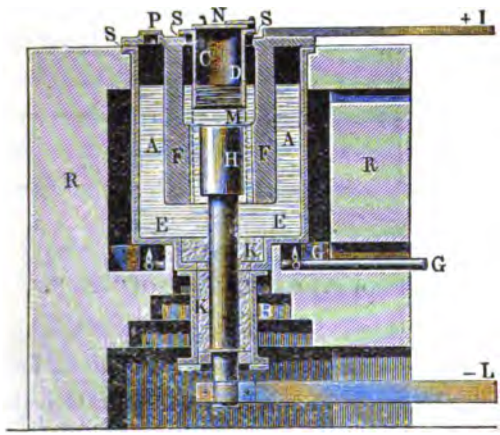

Fig. 3.—One Form of Castner's Sodium-Reduction Furnaces.

A, iron melting-pot.
G, ring of gas-burners.
E, bath of melted alkali.
F, F, anodes.
H, cathode.
D, layer of electrolytically reduced sodium floating melted on bath.
C, collecting chamber for sodium.
N, lid, through which sodium is removed by perforated ladle at intervals. Fits loosely, to allow escape of hydrogen.
P, escape-vent for oxygen evolved at anode.
R, masonry wall of heating chamber.
M, ring of wire gauze separating anode from cathode.

A difference of potential of 5 volts is maintained between the electrodes. In practice, 14 electrical horse-power-hours are expended in the production of 1 lb. of aluminium. If a mean pressure of 5 volts is assumed, the theoretical yield should be nearly one-third of a pound more; there is, therefore, some secondary and wasteful action as well as true electrolytic action, going on, and room for further economy.

Sodium Extraction.

The experiment by which Davy set free the few minute globules of metallic potassium in the little pool of fused potash has to-day its fruition in the electrolytic process of Castner for the extraction of *sodium*. In the Castner sodium process an electrolyte of fused caustic soda is employed, with an anode of iron and a cathode of copper. The sodium is reduced at a comparatively low temperature, and while in a fused state is run off into moulds. By this process there is produced in one works 260 tons of sodium a year.

Sodium is also extracted electrolytically in Germany, and, I believe, in America also. The electrolytic process of sodium extraction is so much more economical than the chemical process as to have almost completely displaced it.

Electrolytic Alkali Production.

I now come to perhaps the most important of all the applications of electro-chemistry at present engaging the attention of chemical and electrical engineers, namely, its application to the alkali manufacture.

The manufacture of alkali has undergone a revolutionary change during the last 25 years; the Le Blanc process, which produces carbonate of soda and hydrochloric acid, having been largely superseded by the ammonia-soda process of Hemming and Solvay—a process identified in this country, in its most highly developed form, with the names of Brunner and Mond. The ammonia-soda process yields no hydrochloric acid, and, therefore, does not lend itself as easily to the production of chlorine for the purpose of making bleaching powder as does the process of Le Blanc. Devices to meet the want of hydrochloric acid in the ammonia-soda process have been many; most of these have been based on ordinary chemical reactions, but some have been electrolytic. I have already mentioned one of these—the chloride of zinc process of Hoepfner. But there are schemes afoot for the production of alkali and chlorine by the electrolysis of alkaline chlorides which aim at the accomplishment of another revolution in this great industry.

There are now several processes in commercial operation for the production of caustic alkali and chlorine from brine. (Specimens of the products of some of these are on the table.)

In the process of Holland and Richardson brine is electrolysed in a tank divided into anode and cathode compartments by impermeable partitions reaching nearly down to the bottom of the tank. The anode compartment is enclosed, and provided with a flue for conducting the chlorine to bleaching powder chambers. Carbon anodes and iron cathodes are used. During electrolysis the caustic alkali formed at the cathode dissolves, sinks down to the bottom of the tank, and is drawn off; this alkaline solution is subsequently evaporated and fused.

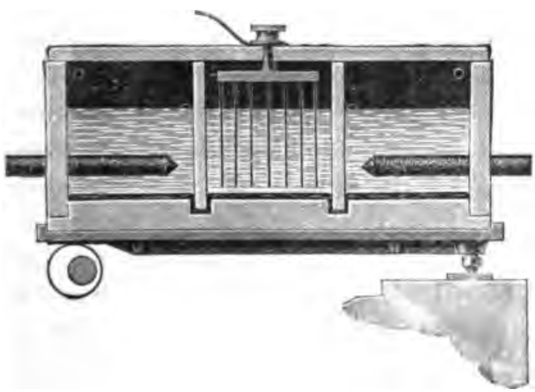

FIG. 4.—Castner's Electrolytic Cell for Alkali Manufacture.

Showing anodes in two end chambers; mercury on bottom, forming a "seal" to prevent electrolyte passing from chamber to chamber; central cathode compartment, with cathode of parallel plates; partitions reaching to cover and nearly to bottom of transverse grooves; and eccentric rocking arrangement below.

In the process of Hulin—in which brine is electrolysed for the production of soda and chlorine—the anode and cathode are both of carbon, but the carbon cathode is in the form of a thin porous partition. The peculiarity of the process is the percolation through the cathode partition of the stratum of the electrolyte in contact with it. This portion of the electrolyte is most strongly charged with alkali, and is forced slowly through the diaphragm by slight pressure on the surface of the bath, caused by restraining the escape of chlorine.

A somewhat similar process has been introduced by Messrs. Hargreaves and Bird for the manufacture of bleaching powder and alkaline carbonates.

In the processes described, considerable loss and many disadvantages arise from imperfect separation of the products of the electrolytic action at the anode and cathode. There have been a number of inventions with a view to avoid this defect. The apparatus of Castner and Kellner is one that grapples with the difficulty in a most ingenious and effective manner, and it is especially entitled to notice because it is already in extensive commercial use. 10,000 tons of caustic soda and over 20,000 tons of bleaching powder will be produced by it this year.

The elementary apparatus consists of a shallow rectangular slate trough, divided into three compartments by two partitions. These cross the trough from side to side, but do not quite reach the bottom, which is grooved to form a shallow gutter under each partition. The partitions dip into the gutters sufficiently deeply to ensure complete isolation of the three compartments when the gutters are filled to the level of the bottom of the trough with mercury. During operation the mercury not only fills the gutters, but extends in a thin stratum over the bottom of the trough. The trough is so mounted that a slow and extremely slight oscillatory movement is given to it. This results, when one end is tilted up, in the stratum of mercury on the bottom running out of the upper end compartment into the middle compartment. The alternate rise and fall of the ends of the trough is so small that the movement is almost imperceptible, but it is sufficient to cause the mercury in the compartment at the raised end to run into the middle compartment, and that from the middle compartment into the lower end compartment; that is to say, there is an alternate flow of mercury from end to end, which alternately leaves the raised end compartments denuded of mercury, but the floor of the middle compartment and of one of the end compartments are always covered with mercury. The grooves into which the partitions dip always contain mercury, and completely prevent the mixing of the electrolyte in the three compartments. The two end compart-

ments contain brine and carbon anodes, and the centre compartment an iron cathode and water. The anode compartments are covered with glass, and provided with pipes for the conveyance away of chlorine to bleaching powder chambers. During the working of the process sodium is deposited upon the mercury, with which it instantly amalgamates; the tank is then tilted until the mercury in an anode compartment runs into the cathode compartment, where the sodium is oxidised and dissolved by the water. The current generated by the oxidation and solution of the sodium helps to reduce the power required for electrolysis; for it will be seen that the sheet of mercury, lying on the floor of the trough and divided by the partition, is always negative in the end compartments, and positive in the middle compartment, relatively to the opposed electrodes.

The chlorine evolved at the anodes is, so far, entirely used for the manufacture of bleaching powder. The caustic soda produced by this process is of great purity.

Closely resembling the Castner-Kellner apparatus is that lately invented by Rhodin, in which the mercury-sealed anode compartments are capable of being rotated, and the construction is such that external heating may be applied, a higher current-density employed, and such temperature conditions maintained as are necessary for obtaining the best result.

Electrolytic chlorine is also extensively applied to the production of chlorate of potash. The manufacture of chlorate of potash, by electrolysis, is performed in a tank divided by a porous partition, with very thin iridio-platinum anodes and iron cathodes. The electrolyte in the anode compartment is usually a solution of chloride of potassium maintained at a temperature of 45° to 50° C. The solution from the cathode compartment containing caustic potash is continuously supplied to the anode compartment, where the potash absorbs the chlorine, with the production of hypochlorite, which is almost immediately decomposed, with the formation of chloride and chlorate of potassium. The chlorate is removed from the electrolyte in crystals. The yield of chlorate of potash is about 1 lb. per 5 electrical horse-power-hours—nearly 45 per cent. of the theoretical amount. In Switzerland and in Sweden chlorate

of potash is now largely produced electrolytically by water power. I am informed on very good authority that preparations are in progress for a large increase of production, and that there is no question as to the electrolytic method entirely superseding the purely chemical method.

ELECTRO-THERMAL PROCESSES.

The electro-chemical processes I have so far described or referred to are all of them of the electrolytic kind. There are other electro-chemical processes which are not electrolytic, but which are important, and deserve mention. I refer, in the first place, to a group of processes and effects which depend on the principle of dissociation and combination at extremely high temperatures, and which involve the employment of the electric furnace, first suggested and applied experimentally by Sir William Siemens.

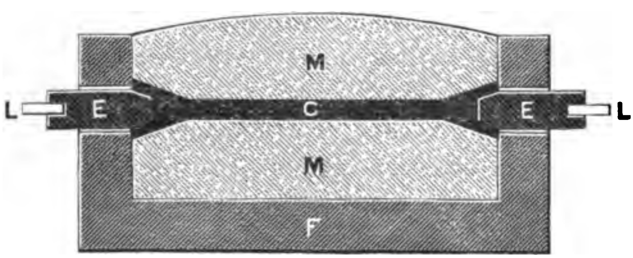

FIG. 5.—Diagram of Carborundum Furnace, in Section.

F, the furnace wall.
M, M, mixture of sand, carbon, and salt.
C, core of granular carbon enlarged to a cone at each end, to surround
E, E, carbon rods.
L, L, copper rods electrically connecting E, E, with electricity mains.

In this class is included the electro-thermal manufacture of phosphorus; also that most useful and interesting polishing and cutting material next in hardness to the diamond, carborundum—the invention of Mr. Acheson, to whom I am indebted for these most beautiful specimens lying on the table.

Mr. Acheson has developed the size of the electric furnace to enormous proportions, and made it yield results of great industrial

value. Amongst these I must mention incidentally—for it is not a chemical, but a physical action—the complete transformation of amorphous carbon into graphitic carbon. It is not new to produce this transformation on a small scale, but completely to convert large masses of carbon into graphite is both new and of great importance.

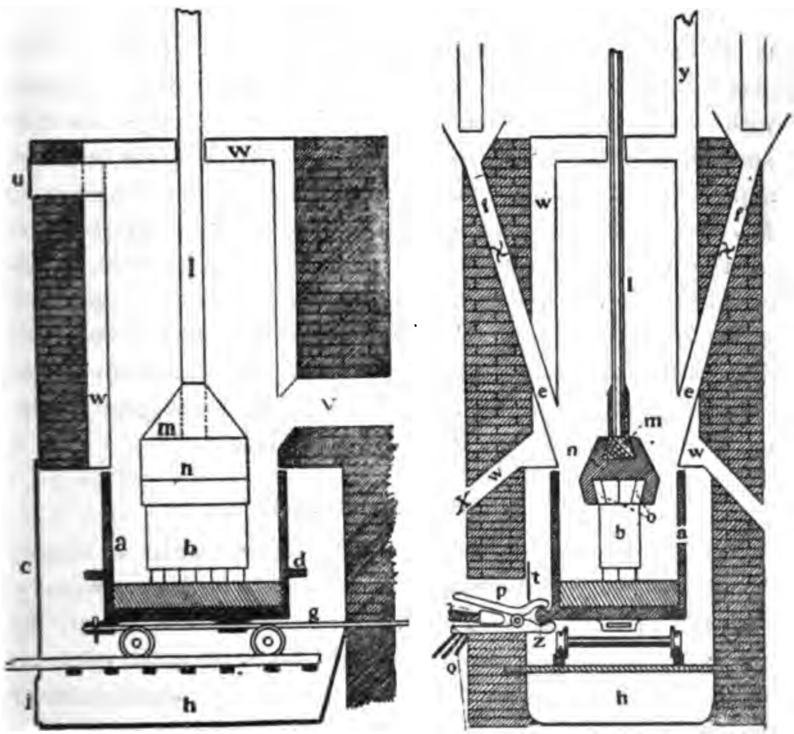

Fig. 6 and Fig. 7.—Calcium Carbide Furnace.

a, movable bottom, or hearth, forming lower electrode.
b, upper electrode of carbon.
g, bar for imparting shaking motion to hearth.
p z, clamp for connecting hearth to electricity mains.
e, charging channel with rotating blades, *f*, to convey solid charge into hearth.
w, Air cooling chamber for upper part of furnace.

It is well known that blocks of carbon as ordinarily manufactured, when used as anodes in an electrolytic cell, rapidly disintegrate; and until now this has been a serious difficulty in

the construction of electrolytic apparatus like that of Castner-Kellner. This difficulty is completely met by the use of graphite anodes, into which ordinary amorphous carbon anodes are now being transformed by the electric furnace.

Some idea of the scale of these electric-furnace operations may be formed when it is realised that 1,000 electrical horse-power for 36 hours is expended in one heating.

To the same class of electro-thermal products belong carbide of calcium and a great number of analogous products, first obtained by M. Moissan by means of the electric furnace,* employed with the most admirable skill, guided by thorough scientific knowledge, and the exercise of that kind of imagination which apprehends and realises far-off possibilities. I am informed by Mr. Worth, of the Acetylene Company—to whom I am indebted for the specimen of carbide of calcium on the table—that carbide of calcium is now being manufactured at the rate of probably 20,000 tons per annum. Considering the value of this substance as a means of easily generating the highly illuminating gas, acetylene, and other products, there appears to be great probability of this manufacture becoming much larger.

Ozone Manufacture.

I must not omit to mention a quite different order of electro-chemical effects in which alternating or intermittent currents of high tension are employed to induce the formation of ozone By means of ozone secondary chemical effects of great value are obtained; among these I may mention the manufacture of vanillin and heliotropine, now established manufactures.

Ozone has also been applied to wax-bleaching, and to the thickening and bleaching of oils, and to a number of other important uses.

Here is a model of an ozone-generating apparatus, kindly lent by Mr. Andreoli, to whom I am also indebted for the specimens showing the effect of ozone on wax and oil. There are also on the table specimens of vanillin and heliotropine, perhaps at

* Mr. T. L. Willson produced calcium carbide by means of the electric arc in the early part of 1892.

present the most important of the ozone products. For these, and for much information on the subject, I am much obliged to Mr. Salamon.

PROSPECTS OF THE ELECTRO-CHEMICAL INDUSTRY.

Although I have but touched the fringe of this matter, I will not weary you with further examples of the value and extent of the applications of electricity to chemical manufactures. I have shown that already there is a large amount of valuable electro-chemical work being done, and that there is a limitless prospect of expansion.

Looking at the immediate future, many interesting questions present themselves, which must be considered, even though we may not be able completely to answer them. Amongst these are the questions, To what extent, and in what cases, are electro-chemical methods likely to supplant old-established chemical methods? And it is not too soon to ask, Where, and by what means, will the new electrolytic manufactures be ultimately carried on?

Will the introduction of electro-chemical methods of manufacture uproot the old manufactures from their ancient habitat?

Already there are ominous signs.

The aluminium manufacture,
The carborundum manufacture,
The calcium carbide manufacture,

are entirely located where there is cheap water power. But these are all *new* industries, and of the kind in which the power element of cost is large, and the value of the product is also large.

In these respects they differ widely from such industries as the alkali and the bleaching powder manufactures, and from electrolytic copper-refining.

In these manufactures, carried on in the electrolytic manner, the cost of power is comparatively very small; and nearness to the market, cost of the carriage of raw material and of product, are even more important factors. Consistently with this, we find that caustic soda and bleaching powder are being electrolytically produced on a large scale in Lancashire by means of *steam* power.

It remains to be seen how far, in the long run, the most

economically produced steam power, as the basis of electrolytic manufacture, can hold its own against water power.

It seems to me probable that in a number of instances steam power *can* hold its own.

There is no uniformity in the cost either of water power or of steam power.

In one place water power will be less costly, and in another place steam power.

Speaking generally, electro-chemical manufactures demand *cheap* electricity; not all of them with equal imperativeness demand the *cheapest*, but some of them absolutely depend on electric energy developed at its cheapest rate.

It is perhaps not entirely superfluous to ask the question whether there is any ground of hope that electric energy may be economically generated by other means than by the transformation of the energy of motive power.

It would be rash to say that it is not possible, but it is certain that there is no better way at present discernible.

Any hope once entertained of the possibly economical direct conversion of *heat* energy into electric energy was crushed by the result of the investigation Lord Rayleigh communicated to the British Association meeting of 1885.

The projects for obtaining voltaic effects by means of carbon as the positive element in a cell have never approached within measurable distance of practicability, and the prospect of their ever coming within that range is all but hopeless. It is quite hopeless so long as the general lines of voltaic cell construction are followed, and so long as it is contemplated to employ the positive carbon in the expensive manufactured forms in which hitherto it has been proposed to use it in carbon-consuming cells.

It seems to me that if ever the voltaic cell is brought into serious competition with the dynamo its form and character must be such that there will be no occasion for renewal either of the electrodes or the electrolyte, but that it must in these respects approximate, in the conditions of its working, to the gas battery.

But when it is remembered how small a steam or water engine and dynamo will develop a hundred or a thousand electrical horse-power, and what a small amount of attendance such apparatus

requires; and when this is compared and contrasted with the much greater amount of labour involved in the maintenance of any equivalent voltaic combination of the ordinary type, it will at once be seen that, to supersede the dynamo, something radically different from and superior to even the most perfect voltaic combination now known would be necessary.

Any such development as this, is at the present moment entirely out of sight; meanwhile, in contemplating the prospective changes which electrolytic processes of manufacture must bring about, we have to count upon the dynamo and motive power as the agency by means of which, in the immediate future, such manufacture will be carried out.

The conditions under which steam power is used in electro-chemical manufacture are extremely favourable to economy, where, as would generally be the case, coal is cheap, the unit of power large, and the power is used *continuously* and *uniformly.*

It seems to me that there is still plenty of room for the steam engine in connection with electro-chemistry, and that, though there are certain electro-chemical industries which can be most economically carried on by means of cheap and not too distant water power, there are other industries—and these may grow to be very large—which may with great advantage be carried on in the "Black Country," or wherever coal is cheap, and the market and the raw material are near at hand.

In the time that has passed, Britain has enjoyed, in chemical manufactures, a great advantage in the possession of an abundance of coal.

We are about, in some measure, to lose the benefit of this advantage through the innovations of electro-chemistry.

Whether we profit or lose by the change, largely depends on our readiness or unreadiness to adapt ourselves to the new order of things.

Whatever happens, nothing can be more certain than this—that the electrical engineer who adds to the ordinary knowledge of his profession a competent knowledge of the principles of electro-chemical practice in manufacturing operations, is thereby making broader and surer his path to success.

ADDENDUM.

Ratio of Cost of Power to Production in Electrolytic Manufactures.

—	Electrical Horse-Power-Hours consumed in the Production of 1 Lb.	Cost of Power to produce 1 Lb. with 1 E.H.P. at £5 and £10 a Year.	
		At £5.	At £10.
		Pence.	Pence.
Aluminium	14	1·75	3·5
Nickel	1	0·13	0·26
Sodium	3·33	0·41	0·82
Caustic soda + $2\frac{1}{3}$ lbs. bleaching powder ...	2·7	0·33	0·66
Chlorate of potash	5	0·62	1·24
Zinc extraction	1	0·13	0·26
Copper „	0·5	0·065	0·13
Copper-refining	0·25	0·032	0·064

Note.—I am indebted to the *Electrician* for Fig. 1 (taken originally from the *American Engineering and Mining Journal*); to *Industries and Iron* for Fig. 5; and to Messrs. C. Griffin & Co. for the remaining figures, which are taken from their translation of Borchers' "Elektro-Metallurgie."

Mr. A. Siemens: Mr. President, ladies and gentlemen,—To me falls the task, which your applause really makes superfluous, of moving a vote of thanks to Mr. Swan for his very interesting Address. I will be as brief as possible, and will only allude to the points which you may think out for yourselves in appropriate language. The first is the one thought which has run through the Address—that the cheapening of a process leads to the establishment of a new industry, to more employment, to a greater activity in all sorts of walks of life, and, generally speaking, to the advancement of the great cause of civilisation. The

lesson we may take from this part of the Address is that we should all endeavour to increase the output per man as much as possible, and to avoid all the restrictions either of mental or physical labour. I believe a good many of you will share my regret that Mr. Swan has not spoken a little more about what he has done—how the invention of this incandescent lamp came about, 16 years ago, and how he did it. It would have interested us very much. However, I believe that Mr Swan, in not doing that, but drawing our attention to new fields, has really followed his own nature, which, in his words, is "full "of the imagination which apprehends and realises far-off "possibilities." It is quite in keeping with this spirit that he, the pioneer of the one branch of electric development, should exhort our young members to try and be pioneers also. I have great pleasure in moving—"That a cordial vote "of thanks of the Institution is due to the President for the "very valuable and highly interesting Address delivered by him "this evening; and that, with his permission, it be published in "the Institution's Journal of Proceedings."

Mr. A. A. Campbell Swinton: I have much pleasure in seconding that resolution. At this late hour I will not occupy time, except to point out what a marvellous instance we have had this evening of our new President's remarkable versatility. Mr. Swan, we all know, has done great things for electric lighting, more especially in connection with the incandescent lamp; but perhaps everybody here is not aware how much he has also done in other directions,—how very important has been his work in bringing photography to its present state of perfection. We now have a further illustration of Mr. Swan's powers, and I am sure that everybody present will join heartily in passing the resolution that Mr. Siemens has read.

The resolution was heartily and unanimously carried.

The President: Mr. Siemens, Mr. Swinton, gentlemen,—I thank you very heartily for the way in which you have received my Address, and I take the opportunity also of acknowledging the honour you have done me in making me President of this Institution. I esteem the honour very highly; at the same

time I am not without apprehension of the weight of responsibility that attaches to it, especially when I remember the eminence and ability of your past Presidents, and the obligation that devolves upon any new President to maintain the high prestige that the Institution has acquired.

I am greatly encouraged by remembering that I am associated with gentlemen on the Council who have large experience and ability, and that I shall have—for a short time at least—the assistance of the retiring Secretary, Mr. Webb, and after that the support of Mr. McMillan. May I beg you, gentlemen, in conclusion, to remember the suggestion put before you by Mr. Siemens that each member should do all he can in the way of contributions to the proceedings of the Institution during the session which opens to-night. Gentlemen, I thank you very much.

The scrutineers report that, as the result of the ballot, the following gentlemen have been duly elected :—

Foreign Members:

Roderick B. Bumiller.
Frederik Michael Nicolai Dresing.
Don Pedro Lopez.
Iwasaburo Nakahara.
Don Alfredo Zinder.

Members:

Capt. D. Brady, R.E.
Walter Dixon.

Associates:

Edwin John Brothers.
Philip Henry Cole.
Edgar Cooke Cox-Walker.
W. M. L'Estrange.
David Origen Evans.
Gilbert Holt Green.
Edward Nicholas Gulich.
Francis Harrison.
Charles Francis Higgins.
Robert Harold Houghton, B.Sc.
Francis Alfred Jackson.
Arthur Alan Jenkins.
Julius Pierpoint Lawrence.
Gilbert James Lloyd.
William Manson.
William Phillips.
Charles Douglas Schofield.
Walter Vernon Scott.
John McFall Smyth.
Henry John Spencer.
Harry E. Stobie.
Samuel Graham Willmot.
Henry Hodgson Wright.

Students:

William Stuart Boyd.
Dugald Alexander Brown.
John George Bruce.
Sebastian Lewis Cazeaux.
Herbert Hugh Clements.
William M. Cobeldick.
Arthur John Cridge.
John Denham.
Alfred Eddington.
Arthur P. M. Fleming.
Alfred A. Godfrey.
James Colin Guthrie.
R. P. Howgrave-Graham.
Hammond C. Hastings.
Joseph William Johnston.
Percy Arthur Jones.
James William Keefe.
Frederic Charles Kidman.
George Reginald Madge.
John Fairles Magoris.
Alexios C. Manuel.
Charles Basil Nixon.
Douglas Ockenden.
Edward Henry Partridge.
Alfred Edward Payne.
Frederick V. Pipe.
Albert Richard Powell.
Edmund Lewis Robinson.
William Graham Royal-Dawson.
J. A. Seager.
Edmund Ramsay Spence.
Oscar Alfred Tuxen.
Frank Percy Whitaker.
Lionel Wood.

The Three Hundred and Ninth Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, January 27th, 1898—JOSEPH W. SWAN, Esq., F.R.S., President, in the Chair.

The minutes of the Ordinary General Meeting held on January 13th, 1898, were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council:—

From the class of Associates to that of Members—

Robert Jamieson Browne.	Hugo Hirst.

From the class of Students to that of Associates—

Harry Jeffery Bellow.	John Dennis Coales.
C. D. Braddon.	Herbert Tyndale Haws.
W. H. Chapman.	R. B. Roberts.
Norman Clough.	Frederick Stephens.

Mr. W. Howard Tasker and Mr. W. J. Grey were appointed scrutineers of the ballot for new members.

The following paper was then read:—

21

NOTES ON THE ELECTRO-CHEMICAL TREATMENT OF ORES CONTAINING THE PRECIOUS METALS.

By Major-General C. E. WEBBER, C.B., (Ret.) R.E., M. Inst. C.E., Past-President.

Maj.-Gen. Webber. The precipitation of gold and silver with the aid of an electric current has a history which may help my audience—many of

whom, I think, are more or less acquainted with the subject—to appreciate the present situation of a question which cannot be devoid of interest to our Institution. Maj.-Gen. Webber.

If we go far enough back in the subject, we shall find that in 1835 to 1840 Becquerel used a saturated solution of common salt for dissolving compounds of silver and lead, and subjecting the solution to the electric current, both to hasten the reactions of the process, and better to utilise the precipitating agent.*

In 1843 Prince Pierre Bagration described in the *Bulletin de l'Academie des Sciences de St. Petersbourg* some experiments with finely divided gold dissolved in an aqueous solution of potassium cyanide under the influence of the galvanic current, by which means he precipitated the precious metal on a copper cathode.

In 1867 Julio H. Rae proposed, in the United States, a method of treating ores containing gold and silver mixed with a suitable solution, such as one of cyanide of potassium in water, by the action or aid of a current of electricity; suggesting at the same time, in addition, the agitation of the solution.

Although Rae's proposals are said to have never gone beyond an experimental stage,† in his description is found the combination

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* Professor Silvanus Thompson has sent me the following extract from "The Memorials of Andrew Crosse," published in 1857:—In 1837, "I took a "piece of quartzy gold ore from California, which weighed 4,306 grains, and "reduced it to coarse powder in an iron mortar," "roasted," and "repowdered" it. "Of this powder I took 1,000 grains and put them into a Wedgwood mortar, "having first thrown into it 200 grains of pure mercury. I then partly filled the "mortar with extremely dilute carbonate of ammonia, and connected the mercury "with the negative pole of a very weak voltaic battery of 12 pairs of cylinders, "keeping up the action for five hours. . . . I next weighed the mercury, "having carefully dried it. Its weight was 205 grains, which, when evaporated in "a black-lead crucible, yielded *eight* grains of gold. . . . I tested the residue, " . . . so that the above electrical process had only left, after five hours' action, "one-seventeenth part of the gold untouched."

† Rae (Fig. 1) describes the employment of a jar or vat, A, of an insulating material, which contains a stirrer in the form of a cage, B, carried by a vertical shaft, C, which, resting on the bottom of the jar, is made to revolve on its axis by means of a pulley, D. This stirrer, combined with the plate, P, on which the axis of the stirrer rests, and on which the pulverised ore is deposited,

Maj.-Gen. Webber.

of (1) a circular vessel to contain the ore; (2) a solvent in solution; (3) a stirrer, or agitator, on a vertical shaft, working with a rotatory motion within the vessel, the stirrer being connected with one electrode, and a conducting metal plate which supports the charge under treatment within the vessel being connected with the other electrode, of a source of electricity.

In December, 1882, Messrs. Breakell and Haycraft patented, in South Australia, a means of treatment of slimes by agitation

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constitutes one (the positive) electrode; the other electrode is connected with a metal plate, E, suspended in the vessel above the stirrer.

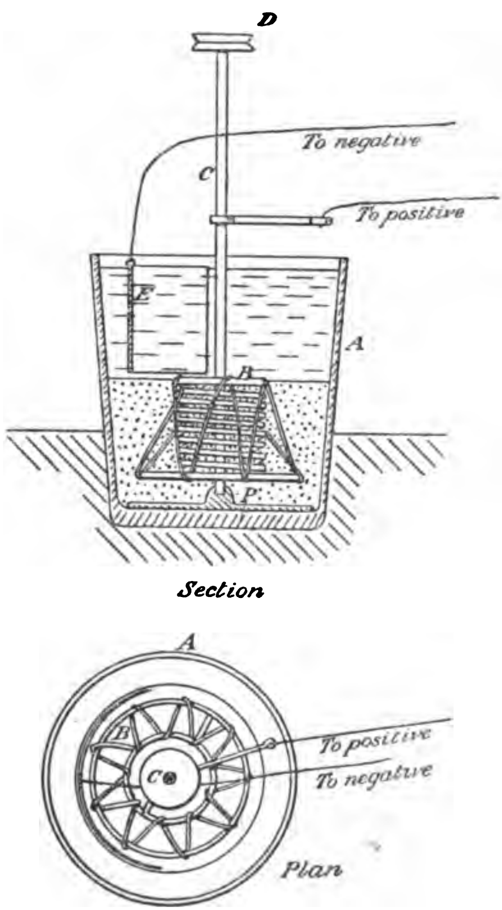

FIG. 1. *Julio Rae*, 1867.

combined with an electrical current and amalgamation. In nearly all respects they copy Rae, except that they use no solvent of the precious metals present in the mixture under treatment.* Maj.-Gen. Webber.

In passing, I must mention Barker's apparatus of 1882, for extracting gold and silver from "sands" containing them, in which we find the use of a mercury cathode, combined with stirrers on horizontal shafts which however did not constitute the anode.

Also Body's process of 1883, which combined a drum and ball crusher of ore containing precious metals. These had been previously ground and dissolved in a solution containing ferric salt. The axle of the revolving drum carries carbon anodes, and the walls of the vessel and the balls serve as cathodes, on which

* In their apparatus (Fig. 2), A is the vessel, or "pan," in which the operation is performed. B is a revolving shaft, carrying arms, or rakes, C, C, which is connected by a contact, preferably of mercury, on the top of the shaft, with the positive pole of a battery. The bearing on which the axis revolves is insulated

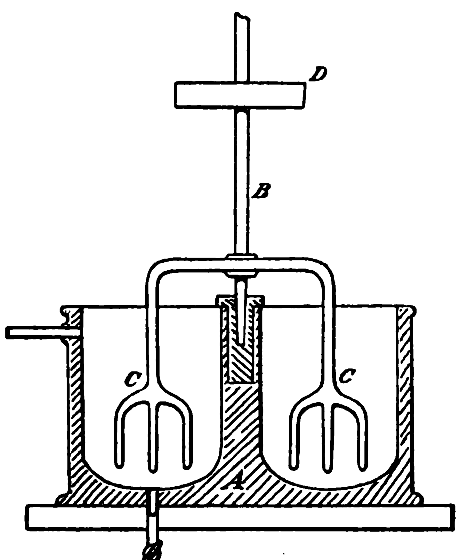

FIG. 2. *Breakell and Haycraft*, 1882.

from the vessel. D shows the position of a pulley on the shaft by means of which it is revolved. The negative pole of the battery is connected with the vessel, which is of iron, or such suitable material; so that the vessel itself, as well as the mercury lying at the bottom of it, constitutes the cathode.

Maj.-Gen. Webber.

the noble metals after solution are deposited electrolytically. Mercury is used, but only for the purpose of collecting the free gold and silver.

I propose to avoid as far as possible allusion to the large number of inventions which deal chiefly with electrical amalgamation, but it is difficult not to refer to some of those which use electrical deposition. Thus, when, in 1888, MacArthur and Forrest investigated the chemistry of, and patented their world-renowned process, they seem to have neglected, or, having tried it, discarded, the assistance of electricity in combination with a weak solution of potassium cyanide, because of a greater expenditure of chemicals which (they alleged) was the result, and also because they believed it encouraged the solution of any baser metals together with the gold and silver which might be present in the ore, and involved the extra expense of their separation.

I need not here refer to the reasons for MacArthur and Forrest's rejection of electricity, nor to the well-known Siemens & Halske process,* in which "circulation" of the solution by gravity is an essential feature, as I wish to direct your attention more particularly to those processes in which the combination of similar conditions with "agitation" are included.

Following on Rae and Breakell, in 1891 Hannay described and patented in the United Kingdom a process and an apparatus for "extracting gold from minerals containing it, by subjecting the "finely pulverised mineral mixed with a solution of cyanide to "electric action and agitation in the presence of mercury." In this process—which it is not understood was ever put to work on a practical scale, and which it is believed, in common with his predecessors', is not described so that the ordinary intelligent engineering mind could erect the apparatus and work it successfully—there is found the combination of (1) a circular vessel or tank; (2) an electrolyte consisting of a solution of cyanide of potassium, mixed with pulverised ore; (3) a stirrer, or agitator, on a vertical shaft, standing in the middle of the vessel, and acting by rotation on its axis; (4) one electrode—the anode, which in

* In 1896 added to, if improved on, by Stuart Croasdale. See *Engineering and Mining Journal* of 12th December, 1896.

this case is a cylindrical block of carbon—being suspended or fixed in the middle part of the vessel. Two examples of the apparatus were published in December, 1891, and in one of them (the later one) we find another condition in combination with the above, which, so far as I can discover, but for the proposal of Breakell and Haycraft in 1882, would be the first example in which the bottom of the vessel described as of "basin shape, containing mercury," as in the case of the ordinary amalgamating pan, is also the cathode. Examination of the description of this cathode will, I think, not satisfy electricians of its practical value without important modifications. The use of solvents or oxidising agents other than potassium cyanide is not proposed, and the strength of the cyanide solution, current, &c., is not given.* Maj.-Gen. Webber.

* There are two forms of this apparatus (see Figs. 3 and 4). In each there is a cylindrical vessel, A, with a revolving vertical shaft, B, turned by a pulley, P. The shaft carries what is called a "propeller," C, which serves only for the purpose of stirring the liquid contents of the vessel. In each section a hollow cup-shaped

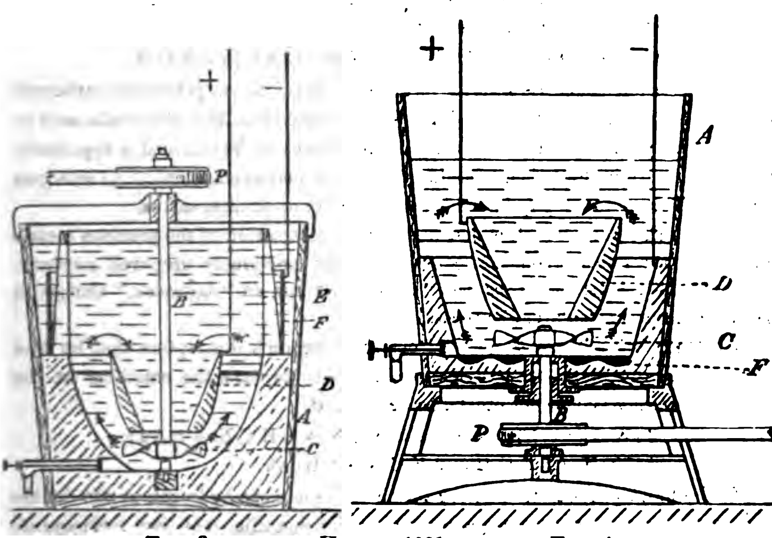

FIG. 3. *Hannay*, 1891. FIG. 4.

block, D, of carbon, is shown, which constitutes the anode, and is connected with the positive conductor from a dynamo. In the example No. 3 the cathode—which is formed of carbon plates, E, within an annular frame, F—is fixed above the propeller. In No. 4 the cathode is a body of mercury which lies in the "basin-shaped" bottom of the vessel, F.

Maj.-Gen. Webber.

There exists an interesting description of a process patented in 1893 by Mr. Molloy, M.P., who, between 1884 and 1887, had dealt with the subject in a different manner. Although it does not include all the features of treatment to which I wish to confine myself, its chemical reactions are not without interest to our subject. It involves at first sight two stages, but really it has four, namely :—

First, the solution of the gold contained in a pulverised ore by means of bromine, chlorine, cyanogen, or other compounds; secondly, the charging a mercury cathode, which forms the bottom of a treatment tank, electrolytically, with potassium or other alkaline metal, free potassium being taken up by the mercury; thirdly, the introduction to the tank of the solution, and its treatment therein; and, fourthly, the recovery of the solvent by regeneration.*

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* In the first stage, although it is not so described, Molloy must subject the pulverised ore to a process of leaching to obtain his solution, say Au Cn.

The following equation expresses it:—

$$Au_2 + H_2O + O + 4\,KCN = 2\,[K\,Au\,(CN)_2] + 2\,KOH.$$

In the second stage, his alkali metal is an alkali salt, *i.e.*, potassium carbonate (K_2CO_3) or a sodium carbonate, &c. No reference is made to the anode used in electrolytically charging the cathode, but the object to be attained is apparently that the mercury in the cathode should take up free potassium, forming an amalgam of these. This addition may be made mechanically as an alternative.

In the third stage, which is that in which the extraction of the precious metals takes place, the solution comes in contact with the previously prepared mercury. It is said that it "rests or passes over" it; so it is not an "agitation," though it may be called a "circulation" process.

Although it is not easy to gather from the inventor's description that he uses electricity in this stage, it is evidently implied, because the reactions in the following equations could not otherwise be effected.

$$2\,[K\,Au\,(CN)_2] + H_2 = 2\,KCN + 2\,HCN + 2\,Au;$$
$$2\,CNH + 2\,KOH = 2\,KCN + 2\,H_2O.$$

In the first equation we have the action of the nascent hydrogen on the double cyanide of gold and potassium cyanide, producing metallic gold and hydrocyanic acid. The gold is precipitated and absorbed by the mercury, from which it is afterwards released in the usual way.

In the second equation we find that the hydrocyanic acid unites with caustic potash formed from the alkali-metal in the mercury, and re-forms potassium cyanide, which is one of the important features of the invention. The recovery of the cyanide will constitute the fourth stage.

Maj.-Gen. Webber.

The entire absence in the specification of the position of the other electrode, and the description of his apparatus being confined to merely the words "many forms of construction," seem to exclude claims to originality in that respect; there being no attempt to describe something that the ordinary engineering mind can construct or put to work.

In January, 1893, E. D. Kendall applied for, and in August, 1894, was granted, a patent in the United States for a method which claims to be "a method" of treating pulverised gold and silver ores with sodium dioxide and a "suitable cyanide" in a water solution. The quantities of chemicals to the ton of ore, are 2 lbs. of sodium dioxide (Na_2O_2) and 7 lbs. of potassium cyanide (KCN), but these may vary. The treatment may be by lixiviation, "with or without" agitation. And the precious metals may be separated from the lixivium by "electrolysis or "other suitable means." No apparatus or special means are described.

Also, in December, 1893, Carl Pielsticker obtained a patent in New Zealand for the extraction of gold and silver from ores, both in the form of sulphide, and in ores in which the precious metals exist in a state of "extremely fine division."

The use of this process was chiefly the cause of the well-known litigation between the Cassel Gold Extracting Company, the owners of the MacArthur-Forrest process, and the Cyanide Gold Recovery Syndicate, by which an attempt to monopolise the use of potassium cyanide in all processes of gold extraction throughout the world, broke down. It is for this reason, and as helping to explain the difference between "agitation" and "circulation" in a cyanide solution, that my notes include its description.

Its essential features may be briefly described as "a process "of separating gold and silver from their ores," which consists in treating the powdered ores with a solution of cyanide of potassium, in conjunction with an electric current of low tension used for the purpose of the deposition of the precious metals. In order that the process may go on continuously, he produces a circulation of the liquid through the space between electrodes,

Maj.-Gen. Webber.

which are fixed respectively, the anode at the bottom, and the cathode at the top of a tank.*

It is claimed for the use of a current of higher potential specified to be used in the ore tank that "the precious metals "are attacked more energetically by a cyanide solution in con-"nection with a current of electricity than without." We are also told that "free gold is certainly more quickly dissolved by "cyanide of potassium in conjunction with an electrical current "than without one."

To recapitulate: This process has, in common with Rae and his successors, the combination of a solution of potassium cyanide with an electric current, by which it assists solution and effects precipitation on a cathode; and, in common with Molloy, it effects the treatment in more than one stage, and by means of "circulation." In this latter respect Molloy and Pielsticker are

* A is an ore tank; P is the anode, made of perforated iron or carbon; N, the cathode, also a perforated plate. The circulation of the liquid is through two other tanks, B and C, besides the ore tank, by means of pipes connecting the whole, and the movement of the liquid through the system is by power working a pump, K. The second tank, B, is used to arrest suspended matter in solution; and the third, C, is a precipitating tank, in which electrodes are placed vertically, the anode, M, being of carbon, so as to resist the dissolving action of potassium cyanide. The precipitation is on to the

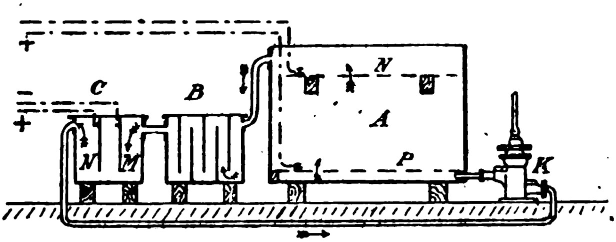

FIG. 5. *Carl Pielsticker*, 1893.

cathode, N, in the third vessel of the set. The same solution is circulated round and round, until all it can dissolve in the ore tank has been extracted, and all that the electricity can separate from it in the third vessel has been precipitated. The electricity is taken from the generator through two circuits, and in each a separate duty is performed. In the first tank the sludge, as it may be called, is subject to an electric current, which we are told may be of higher potential than that in the third tank, where it is preferably limited to 1 volt and 10 amperes per square metre of surface of cathode.

at one, in contradistinction to the processes in which "agitation" is essential. With reference to my observations further on as to "agitation," it may be here observed that in any such processes the density of the sludge at various points in the system of circulation must depend on its rate of motion. Maj.-Gen. Webber.

Edwards, in April, 1894, described, in the United States, an apparatus very similar to Hannay's, except that the position of the carbon anodes is altered, and they are apparently placed so as to line the sides of the treatment vessel.

In 1894 an invention was produced in the United States, and subsequently patented there and in other countries by Messrs. Pelatan and Clerici, for an electrolytic process and apparatus for extracting gold and silver from their ores and other compounds.

The process has been described as a single continuous one—an essential feature—because as such it has proved itself equal to effect in one operation all that can be expected of it. In other words, it receives the pulverised ore in a wet condition, and, after being stirred up, and water, if necessary, added, so as to be in a condition of fluidity to allow of suitable agitation of the particular kind of ore under treatment, and so that the chemicals used in the treatment are properly mixed with the solution, it is introduced into the treatment vat or vats, in which the operation, which I shall again refer to, is carried out until nearly all the precious metals are extracted and absorbed by the amalgam at the bottom of the vessel or vessels. Although a preliminary mixing of the solution in a separate tank may be thus implied, there is nothing to prevent the mixing being carried out in the treatment tank itself.

In this we have, I believe, for the first time, a process and apparatus which effectively combine, in a way that can be constructed and worked by a workman of average intelligence, the following:—

(1) A vat made of a material, dielectric in its nature; (2) an agitating apparatus of various specific forms, each form calculated to carry out one and the same process—being the result of considerable experience—having an agitator, part of which constitutes the anode in an electrolytic circuit, which is carried so

Maj.-Gen. Webber.

that it can by no means make contact with the bottom or sides of the vat; (3) the presence of a cathode, which covers the whole of the bottom of the vat, made of a metal (preferably copper) plate or sheet, and suitably contained and fixed so as to carry on it a layer of mercury; (4) the use of a graduated current from an electrical generator, having large quantity and low potential; (5) the mixture or sludge under treatment being composed of water in given proportions, ore finely pulverised, potassium cyanide or other solvent of gold and silver, and common salt, with the addition as required during the process of alkalies or organic acids as may be required.

These are the chief features, together with important details of construction to which I shall refer further on, that constitute the Pelatan-Clerici process.

In the latter half of 1894 J. H. Haycraft, of Adelaide, in West Australia (whose invention jointly with Breakell has been already mentioned), described an improved process for the treatment of auriferous and argentiferous ores. He expressly disclaims originality for the apparatus, and for any separate part of his process, or for any two or more parts together, but considers that his invention consists in the entire and particular combination planned by him.

In this process we again find the following conditions:—One circular vessel for the whole process of treatment, having a revolving stirrer with projecting arms connected with the positive pole of a dynamo. The charge under treatment is a mixture of pulverised ore and water, and the precipitation is obtained by the use of a solvent in conjunction with the electric current, the precious metal being absorbed (and afterwards recovered) by amalgamation in mercury. But, in addition, the process presents the following features:—

First, the vessel is heated by a furnace or by a steam chamber underneath, and being therefore of metal, the whole of it forms the cathode.

The stirrer, which is the anode, can doubtless be insulated from the vessel without much difficulty, and its arms are clad with carbon electrodes at their extremities, which in revolving are

separated from the cathode by a space of about a quarter of an inch. Maj.-Gen Webber.

To the charge of ore is added about 5 per cent. of its weight of mercury, with about 1 per cent. of chloride of sodium or any other salt capable of yielding chlorine by electrolysis; but these proportions the inventor varies according to the class of ore under treatment.*

Practical experience in working an apparatus of the description given by Haycraft might be expected to result in failure, owing to three only out of many difficulties that the construction presents, namely: (1) in keeping the density of the solution uniform throughout; (2) in bringing it under the influence of the current within the ¼ inch space between the anodes which lie nearest to the cathode, where the path of the greater part of the current would lie; and (3) the difficulty of maintaining surface parallelism between the electrodes.†

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* The charge having been introduced into the vessel, the current—about 200 amperes by 3 volts to each ton of ore under treatment—having been started, and the temperature raised to and maintained at a little under the boiling point of water, the process is continued for about one hour, the stirrer revolving slowly all the time. The bullion is afterwards recovered from the amalgam in the usual way, but in the first instance the amalgam must be separated from the pulp with which it has been mixed during the treatment.

The peculiarities of this process which may occur to the electrical engineer are, that the bottom of the vessel slopes towards the centre, and that, using the words of a description by Professor Wiedemann, "of the University of Munich," "a large quantity of quicksilver placed in the centre of the pan gives the cathode "or negative electrode; thus the pans remain the only suitable leading link "between the cathode and dynamo; he (the Professor) attaches great importance to "the free movement of the quicksilver cathode, as at boiling water temperature the "quicksilver spreads through the whole pan, thus coming into contact with the "coarser particles of gold, which settle down at the bottom of the pan by their own "specific gravity." And then he adds—what is still less easy to understand, seeing that the space between the anode and the cathode is only about ¼ of an inch—"The "spreading of the quicksilver brings into action the most distant carbon shoes, "otherwise the electric current would pass to the iron pan electrode, and by its "action would prove of no value, as there would be a continuous deposition and "reunion."

Again he says: "The electric current by its passage through the liquor (salt "water and the ore in suspension) decomposes," &c.

† I regret to have failed in my efforts to obtain a diagram of Haycraft's apparatus.

Maj.-Gen. Webber.

In September, 1894, P. Danckwardt patented in the United States an improved apparatus for, and process of, extracting gold and silver from ores.

In this we find again the treatment of finely pulverised ore in a solution containing potassium cyanide—in this case about 10 lbs. of cyanide to the ton of ore—and agitation. Besides, the inventor adds to the solution 2 to 3 lbs. of ammonium-, "or another "alkali-metal-," sulphide, the object of which is said to be to reduce the consumption of cyanide to a minimum, as a means of preventing "the formation of soluble combinations between any "of the raw metal combinations and part of the cyanide of "potassium."*

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* The agitation is effected by the rotation of a cylindrical drum, A, on its axis, and two ways of doing this are described. In one case (Fig. 6) the rotating

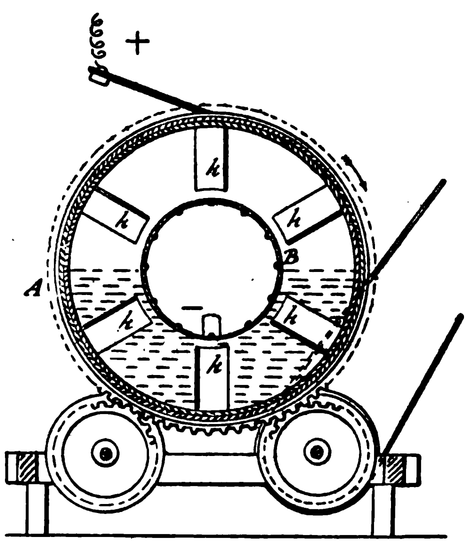

FIG. 6. *P. Danckwardt*, 1894.

drum is external to the fixed one, and the conditions are the other way in the second example (Fig. 7). In each case the cylinder in motion, B, constitutes the anode, the stationary vessel the cathode. In the first case the outer revolving drum carries internally blades, *k*, *k*, by which the solution is stirred and guided on to the amalgamated surface of the inner fixed cylinder, B, which is made of copper.

The second example (Fig. 7) of effecting agitation in this way is an auxiliary

In spite of the low tension of the current employed, the difficulty of insulation between the cylinders at their axial bearings must be considerable. I can find no record of this ingenious combination having ever been used on a practical scale, and I do not think that the description given is such as to enable an apparatus to be constructed that would produce practical results on a working scale. The actual subjection to electrolysis of the mixture described was, of course, not novel in 1894. Maj.-Gen. Webber

In November, 1894, Edward W. Clark filed an application in the United States, describing a process and apparatus, which he calls an "electric chlorinator," for extracting ores by electrolysis.

He passes an electric current through a solution containing the crushed ore to be treated and chloride of sodium. He uses an agitator of a particular form, and mercury as his cathode.*

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one, and only resorted to in case the extraction by means of the first form of apparatus is imperfect, when the solution taken from the first, having been filtered, is passed through one or more of the agitating vessels; and in this latter case the

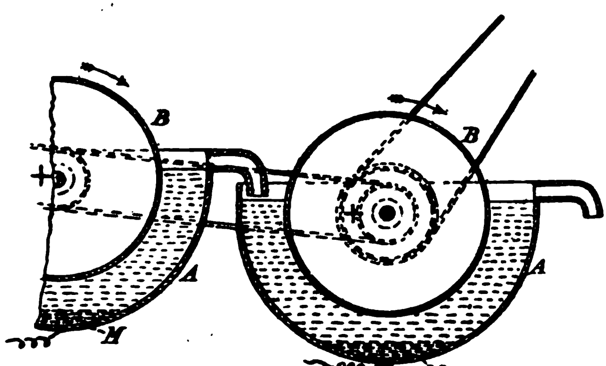

FIG. 7. *P. Danckwardt*, 1894.

outer and stationary tanks, A, A, besides being amalgamated on their inner side, contain a little mercury, M, M.

* The vessel in which the agitation takes place is a horizontally fixed cylinder, A, with a shaft, B, carrying spirally fixed stirring arms, C, which revolve inside on its axis. The bearings are gas- and water-tight.

The anode is a fixed carbon lying on the bottom of the cylinder. The mercury cathode lies at the bottom of small boxes, D, D, attached in pairs at intervals on each side of the cylinder. The communication between the cylinder and the boxes is by means of openings, E, E, at the lower sides of the former,

Maj.-Gen. Webber.

With the exception of that part of the combination which is affected by the special form of apparatus, and which is said to prevent the combination of the liberated hydrogen, oxygen, and chlorine, there is no reason for claiming novelty for the invention.

At the end of 1896 Dr. Keith, who read a paper on "The "Electrolysis of Gold," in March, 1895, before this Institution, took out a patent in Canada, in which he makes, amongst others, a claim for a "process of extracting gold and silver from "auriferous and argentiferous materials, rocks, or ores, which "consists in submitting them to the solvent action of a solution "of cyanide of potassium, containing a solution either of cyanide

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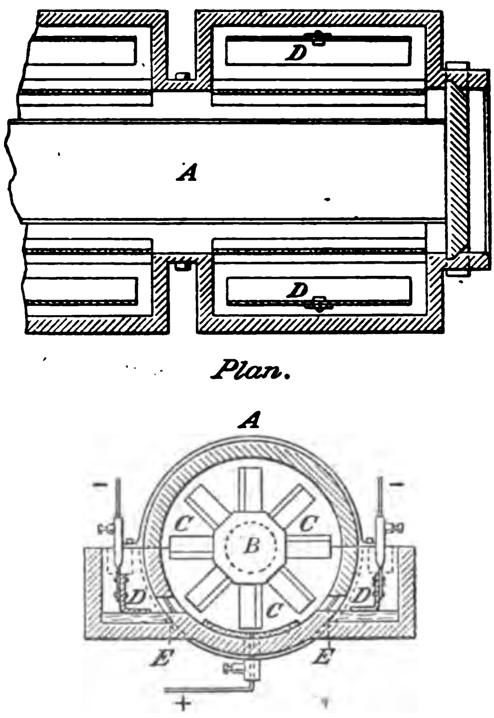
which are covered with a canvas screen or filter. The current has to pass between the electrodes through these screens, in company with the solution on its way



Section.

FIG. 8. *Edward Clark*, 1894.

for the chloride of gold to deposit the gold in the above-named boxes or amalgamating chambers.

"or bromide of mercury, or both, and then depositing the gold, "or the silver, or both, and the mercury from the solution so "obtained, by means of electricity upon a cathode or an amalgam." Maj.-Gen. Webber.

I understand that this was reported on by Professor Silvanus Thompson, but the only encouragement I can find in the published part of his report is, that the "process hastens the "solution of the gold, in comparison with the use of potassium "cyanide only." *

I now propose to go more carefully into the process I have described as that of Messrs. Pelatan-Clerici, which I have had under my observation for more than two years.

It claims as its object the treatment of ores containing gold or silver, or both, so as to obtain the precious metal therefrom in a manner more complete, simple, satisfactory, and with greater economy of the agents, than hitherto.

As an example, I shall refer to the simplest form—of which an illustration is given—namely, the circular vat, with the shaft of the anode in a vertical position. (See Fig. 9 on page 55.)

The improvements are—

First, that the space (unlike some previous proposals) between the revolving anode and cathode is free from all obstructions, the disadvantages of which are that they tend to cause the ore under treatment to accumulate upon the cathode, and prevent perfect parallelism between the surface of the mercury cathode and the effective under surface of the anode—a condition the necessity for which is obvious.

Second, that the sludge is constantly and gently swept by the current from over the cathode so as to have no tendency to settle on it, and also so that that portion of the sludge which is above the anode should not acquire such a continuous rotatory motion as to cause the heavier particles to be carried outwards by the centrifugal action, and thus to travel round in the same plane,

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* Similar means to some of those I have mentioned for doing the same work have been patented by De Neufville, September, 1895; Hinman, September, 1896; Becker, August, 1897; but, with the exception of the combination of very minor details, these present no features of originality.

Maj.-Gen. Webber. instead of in their turn being subject to the combined action of the electric current and the chemical agents employed.

This leads me to draw attention to the necessity that efficient agitation should, in the first place, maintain the sludge perfectly homogeneous throughout its mass, and, in the second place, should not be such as to disturb or break up the mercury cathode. In the example before us this is secured by regulating the speed at which the anode is driven, according to the size of the vat and the number and length of arms that are attached to the shaft, either for the support of the anode plates, or to act only as stirrers. For instance, subject to the density of the sludge, it is found that, if the speed of the agitator has to be lowered 30 per cent., the number of arms should be doubled.

As the diameter of a vat is increased, in order to maintain homogeneity in the mixture the rate of motion at the periphery of the agitator would have to be increased, but to the detriment of the surface of the mercury cathode, namely, above a rate of about 8 to 10 feet a second. To avoid this the number of arms is increased. Provision is also made, to meet special circumstances, to add arms which vary in length, which may form part of the anode or not.

The proportion of water to ore in the sludge is a condition that also affects this department of the treatment, and it is found that between equal weights and proportions the best results are obtained when the water weighs three-fifths of the ore.*

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* The revolving anode is mounted so as to be suspended from above in the vat at the lower end of a vertical shaft, S, braced and supported in the manner shown in the drawings (Fig. 9).

The lower surfaces of the iron plates, P, P, &c., attached to the under sides of the lowest tier of projecting arms are in the same horizontal plane, and are carried at a distance of between 4 and 6 inches from the mercury cathode, M, which is a matter of regulation according to conditions which are well understood.

The positive connection from the dynamo is by way of the vertical shaft and a rubbing contact connected with the conductor.

The connecting cable from the negative pole of the dynamo is led to the copper plate of the cathode, M, with which it may make connection at one or more points.

In the space between the anode and the cathode the actions and reactions described further on take place, and the metal which is absorbed or taken up by

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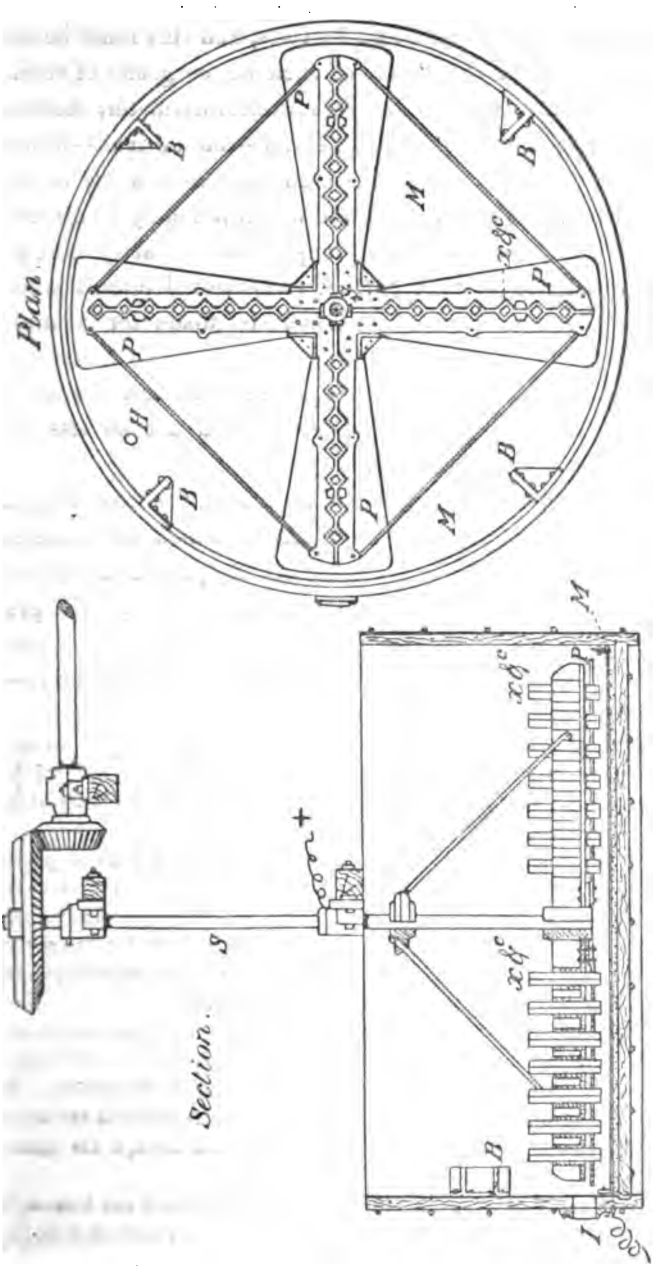

Fig. 9. *Pelatan-Clerici,* 1894 *and* 1897.

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As regards the current, the potential should be capable of being governed between 5 and 14 volts, and this must be done by resistances fixed in the circuit of each vat or group of vats. For instance, when using vats of 9 feet interior diameter, each capable of treating 2½ tons of ore, in two shifts—total, 5 tons—in a day of 24 hours, should a plant to treat 200 tons of ore a day be worked, 40 vats will be required, and these would probably be governed in eight groups of five vats in a group. The quantity is regulated by the resistance in that part of the external circuit which lies between the electrodes. Provision is made for a minimum quantity of 1½ amperes per square foot, being half the sum of the cathode and anode together in square feet: thus, if that area is equal to 52 square feet, the current should not be less, and not much more, than 39 amperes.

The inventors claim the use of several other forms of apparatus to effect the same objects, the details of which are more or less original, and are wholly original as forming parts of combinations; and they also describe conditions of variations of temperature during the progress of the treatment, as well as the addition of specific oxidising agents and compounds, and organic acids—all of

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the amalgam is there allowed to collect through the continuance of the treatment of successive charges until, when it is saturated, a "clean-up" takes place. According to the richness of the ore, a "clean-up" may be necessary at intervals of, say, 2 to 15 days.

In order that the agitation of the sludge may be such as to prevent the accumulation of heavy material on the surface of the mercury, the revolving arms are provided with pins, *x*, *x*, *x*, &c., say of wood or other non-conducting material, placed vertically, and projecting downwards to within a short distance, say an inch, of the cathode. These pins, as shown on the drawing, also project upwards, so as to help to maintain agitation throughout the liquid mass.

In order to correct the tendency to continued centrifugal movement of the liquid, projections, or bafflers, B, B, &c., are fixed at intervals to the inner side of the vat above the arms, either vertically or at an angle to the vertical. By their influence the revolving current is broken up, and guided inwards and downwards so as to oblige every portion to pass in rapid succession through the space below the anode.

The pipe opening at the side of the vat and a little above the bottom, I, is for drawing off a charge at the expiration of its treatment; and the hole, H, in the bottom is provided for the purpose of drawing off the mercury at the time of periodical "clean-up."

which are matters appertaining to the discoveries they have made in experimentally treating samples on a working scale of ores from over 200 mines.* Maj.-Gen. Webber.

In an investigation of the electro-chemistry of the process I shall confine myself to the simplest (or circular) form of apparatus.

But before doing so it is well to remind any mining engineers who may be present that the degree of fineness of the pulverisation employed is obviously a varying factor in considering this part of my subject, both as regards the chemical and electrical, and their combined, effects, especially when dealing with refractory ores.†

* Results of some tests on a full-size scale, made at Denver with low-grade ores from various mines, pulverised to 40 mesh only:—

Name of Mine.	Dwts. to the Ton. Assay Gold Value.	Precipitated direct.	Precipitated from the Solution.	Total Gold saved. Per Cent.
Rose	10·0	40	50	90
De la Mar	13·0	46	31	77
"	4·8	55	25	80
Oaxaca	8·0	45	30	75
Bassick tailings	6·8	23	53	76
Baby	13·2	64	21	85
Phœnix	2·4	75	17	92
Leroi	8·4	57	19	76
Alma	18·4	80	9	89
Miller	7·8	60	25	85

Some of the above samples were pulverised also to 60 mesh, and gave a result from 6 to 10 per cent. better.

† The advocates of treatment by simple leaching with chemical solutions dislike fine crushing, say finer than will pass a mesh of 20 to 30 to the lineal inch, because it encourages the formation of "slimes," in the presence of which systems of treatment that act by means of percolation become less effective.

In those systems of treatment in which extreme fineness is not detrimental—rather otherwise—crushing so that the powdered ore will pass a mesh of, say, 60 to 100 to the lineal inch is more or less easily practicable, and makers of milling machinery have no hesitation in facing the problem.

Even then the way in which the work is done depends on the nature of the ore and the means employed. For instance, with some kinds of gangue, and of milling machinery, the particles in which the precious metal is intimately attached

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To enable you to follow the course of what happens in the treatment tank, the process will be divided into more than one stage, although the actual order in which they follow one another, and the time occupied by each, may vary with the ore under treatment, and it is not always the case that it is worth while to separate them even for a short time.

During the whole time of treatment, "agitation," as distinguished from "circulation" or "percolation," is going on. The stages may be with or without the accompaniment of an electric current during portions of the time, but in any case the current is used during more or less of the time.

The liquid may contain sodium chloride (Na Cl) or potassium cyanide (K C N) alone, or both; but, as the sodium chloride is directly and in the first instance used to "reduce the resistance" or "increase the conductivity" of the solution, there is no stage during which—except for the purpose of mixing—it is used alone, without being accompanied by the current.

The expressions I have used—namely, "reduction of resistance" and "increase of conductivity"—are useful because they are easily understood; but, be it remembered, they are not correct as applied to the solvent, *i.e.*, water. Indeed, these descriptions of the effect of the mixture of sodium chloride in a solution, and then placing that solution between an anode and a cathode, is scientifically inaccurate. What happens to the sodium chloride in solution in the water is, that it is disintegrated and re-formed, and thus it becomes the intermediary by which, at the instant that chlorine gas and sodium are set free, the current is enabled to "communicate" between the electrodes.

During the time of electrolysis, when the liquid contains only sodium chloride (Na Cl), the primary decomposition will be sodium (Na) and chlorine (Cl). In practice the quantity by

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to a baser metal--say in iron pyrites—instead of the result rendering them more pervious to chemicals, they may be found to be pressed out in solid, rounded, and smooth atoms, and in the worst form for separation and subsequent stages of the process.

These conditions would govern the net results, subject to the cost of fine crushing.

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weight of Na Cl may be between 0·2 and 1 per cent. of the weight of the ore in the sludge, but this depends upon the nature of the gangue. In any case, the presence and addition as required of sodium chloride is a regulator of the electrical resistance of the solution which at any given moment is situated between the cathode and the anode, and, as the resistance in the conductors counts for very little, it practically affords means of governing the current in the external circuit of the electrical generator or dynamo. Of the several compounds which might be used for the same purpose, common salt is doubtless much the cheapest.

Now to consider the action of these products of electrolysis. As the sodium is liberated, in contact with the mercury cathode, a small proportion will no doubt be dissolved in the mercury as an amalgam. If the current ceases, this is converted into sodium hydrate (Na H O), with liberation of hydrogen.

But the greater part of the sodium liberated will react at once with the water in the solution, giving Na H O and H, the former to be used as described further on. Most of the chlorine liberated will at first be dissolved in the solution, but, as the liberation will take place at the surface of the anode, a small quantity may be used up in attacking the metal of which the anode is made.

While this is going on, the liquid immediately in contact with the cathode will become rich in sodium hydrate (Na H O), and the agitation will cause it to come in contact with the chlorine liberated at the anode, with some of it to re-form sodium chloride (Na Cl), and with another portion sodium hypochlorite (Na Cl O) and water (H_2 O).*

This formation of hypochlorite in solution will, subject to the adverse conditions caused by agitation, be directly in proportion to the quantity of Na Cl used to regulate the resistance, and with the relations of the areas of the anode and cathode respectively.

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* $2\,NaHO + Cl_2 = NaCl + NaClO + H_2O.$

At a higher temperature, say 130° Fahr., the Na Cl O would become sodium chlorate, according to the following equation:—

$$3\,NaClO = NaClO_3 + 2\,NaCl.$$

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But this formation of sodium hypochlorite will be limited by the extent to which the sodium hydrate above referred to, when it is liberated at the cathode, rises to lay hold of the chlorine which is rich around the anode. This we may regard as taking place in spite of the agitation of the stirrers, which probably tends to cause temporarily a rapid diffusion of the chlorine and sodium hydrate separately in the solution, although eventually they must come together.

Although, when not in excess, it is a useful oxidiser, this formation of sodium hypochlorite has little advantage; and when potassium cyanide is added to the solution, it is, when in excess, even when alkaline, likely to oxidise some of it into potassium cyanate—a decided disadvantage, as it is a salt which is easily decomposed, and much less useful.

Let us now consider what may be effected by the chlorine liberated, but not engaged, as above described. In the first place, one might expect the nascent chlorine to attack some of the baser metallic ores present in a finely divided state. For instance, sulphides, selenides, arsenides, &c., often attached to particles of gold or silver, would be so attacked; the chlorine uniting with the metallic base iron (Fe) or copper (Cu), and the (S) sulphur, (Se) selenium, or (As) arsenic being oxidised by the combined action of the chlorine and sodium hypochlorite above alluded to, into sulphuric, selenic, or arsenic acids.

These changes would be marked by the formation of sodium sulphate, sodium selenate, &c., and the consequent formation of free acids, including hydrochloric acid.*

This probably represents what actually is the cause of disintegration of some of the metallic compounds which are present in refractory ores, as the chlorine attacks these first in preference to the gold. At the same time, it is no doubt the case that, as the process continues, some of the gold is brought into solution by the chlorine diffused through it, as chloride of

* The reaction in this process may be represented in two stages, as follows:—

$$Fe\,S + Cl_2 = Fe\,Cl_2 + S,$$

$$\text{or } Fe\,S_2 + Cl_2 = Fe\,Cl_2 + S_2 \text{ (iron pyrites);}$$

$$\text{and } S + Na\,Cl\,O + 4\,Cl + 3\,H_2O = Na\,H\,S\,O_4 + 5\,H\,Cl.$$

gold ($Au\ Cl_3$), the metal from which is readily deposited by electrolysis on the mercury. Maj.-Gen. Webber.

We all know that the solution of gold by chlorine is in common use, but I think that in the example given it is effected under novel conditions, the chlorine being added electrolytically instead of mechanically.

During the first two or three hours of treatment by agitation with the electric current, there is no doubt that the free gold which is not dissolved will be precipitated, the heavy particles at a very early stage of the process.

In all such mixtures there is also more or less of what is called "float" gold, which in the processes using "percolation," and even "circulation," is lost in the "slimes." These particles, if they escape solution, will be so well mixed up in, and diffused through, the solution by agitation, that they should all in their turn be brought into close neighbourhood of the mercury surface of the cathode.

The advantage of mechanically adding small quantities of sodium to mercury employed in gold extraction to assist amalgamation has long been recognised, but in this case the sodium so used is provided electrolytically. There is little doubt that the heavier particles of gold and silver which reach the bottom by subsidence will, owing to the surface being strongly polarised both by the current and by the slight amount of sodium amalgam formed by the current, be more readily amalgamated, and the same conditions also will promote the seizure by the mercury of the minute particles of float gold and silver when they approach its surface. The reason for this is probably, not merely that it is thereby "kept clean"—*i.e.*, free from oxide in the well-known sense—but that the difference of electrical potential set up between the mercury and the liquid alters the surface tension at the liquid junction, and helps the metallic particles to come in actual contact with the mercury. This is, I believe, a new way of defining what occurs when what is called "prevention of flouring," and the establishment of metallic contact with the mercury, takes place.

The presence of the excess of an acid such as hydrochloric

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acid has to be provided for by the addition, say, of lime, to make the solution neutral; but this will not get rid of any excess of hypochlorite, previously mentioned, if present.

Lime, to remove free chlorine, will not be added until any metals in the solution, such as copper sulphides, or arsenides, have been formed into electrolysed salts through conversion into chlorides. These, which cause waste of cyanide of potassium, would to a certain extent go into solution, and, by electrolytic action, become deposited in the mercury, and their values saved.

It will be understood, therefore, that all this preliminary work not only amalgamates a large proportion of the free gold and silver, but also prepares for the more effective action of potassium cyanide, both as a solvent and as an auxiliary in producing the condition of increased conductivity in the solution.

Thus, everything that chlorine can do to help the disintegration of the metallic ores which are found accompanying gold facilitates the action of potassium cyanide when it is added. This preliminary disintegration would be helped, not hindered, by temporary increased acidity of the liquid, when bodies like sulphides are oxidised at the expense of the chlorine; showing that the best time for neutralising the solution is at the end of this preliminary stage, and just before the potassium cyanide is introduced.

It will be seen that in this preliminary stage the eventual economy of potassium cyanide (or other solvent) should be aimed at, because, although with "percolation" processes the "leaching" can be effected with even half a pound of it, as much as 2 lbs. of it to the ton of ore under treatment might be inadvertently used up. Obviously, the greater part of it would be wasted, as, in treating a low-grade ore containing, say, 15 grammes of gold and, say, 30 grammes of silver, 60 per cent. of the gold and 40 per cent. of the silver may be separated and amalgamated in the first two or three hours, before the potassium cyanide is added. Clearly, it does not require 2 lbs. to treat 6 grammes of gold and 18 grammes of silver, even if all the value that remains in the sludge *could* be dissolved.

The addition of potassium cyanide (K C N) to an ordinary

agitating vat causes the solution of some of the finely divided gold. A further solution will ensue when, by the passage of an electric current, a decomposition occurs which yields free cyanogen at the anode and caustic potash and hydrogen at the cathode. So far the reactions are common to all arrangements which combine agitation with electro-cyanide processes.* Maj.-Gen. Webber

The addition of potassium cyanide in excess in the Pelatan-Clerici process has been doubtless not inadvertent, but due to the necessity of allowing for the oxidation of it by any excess of hypochlorite previously mentioned.†

It is in any case desirable to have some cyanide to spare, because the cyanogen ($C_2 N_2$) formed at the anode and dissolved in the solution will readily unite with the minutest and lightest particles of gold, to form with some of the said excess of it the double cyanide of gold [$Au K (C N)_2$], which is a salt that is easily soluble and readily electrolysed.‡

These conditions point to the advantage of getting rid of any excess of hypochlorite and of acidity by neutralisation before the cyanide ($K C N$) is added; also avoiding excessive oxidation of it, when, if an insufficiency were to ensue and tests were neglected, it is possible that $Au C N$ might be formed and remain insoluble, and hence escape electrolysis and pass away in the tailings.

There is an advantage also in the simultaneous electrolysis of both sodium chloride and potassium cyanide, though the duty of the former is principally to facilitate the conductivity of the electrolyte. Chlorine and cyanogen are both yielded at the

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* They are: With dissolved oxygen alone available—

$$4 K C N + Au_2 + H_2 O + O = 2 K Au (C N)_2 + 2 K H O;$$

with nascent cyanogen—

$$K C N + Au + C N = K Au (C N)_2.$$

† $Na Cl O + K C N = Na Cl + K C N O$.

‡ Observation to ascertain the continued presence of cyanide ($K C N$) is made from time to time with the nitrate of silver test—a drop in a test-tube of the solution. If too much chloride of sodium is present, the effect would be masked by the formation of chloride of silver, when it would be difficult to distinguish the cloudiness of the liquid caused by the one from that caused by the other. In such a case iodide of starch, which is bleached by potassium cyanide, and not by chloride, is useful as a substitute for nitrate of silver.

Maj.-Gen. Webber. anode, and would probably lead to the production of cyanogen chloride, which (in common with cyanogen bromide) would be in the presence of potassium cyanide as effective in attacking gold, if not more so, as cyanogen itself. The difference between this and what is known as the Sulman-Teed process is, that in it cyanogen chloride (or bromide) is added mechanically to a solution containing potassium cyanide and gold or silver ore,* whereas in this process the conditions to effect a corresponding reaction are produced electrolytically.

To Mr. E. F. Herroun, of King's College, I owe every thanks for assisting me to prepare the above investigation of some of the features of the process.

When some members of the British Association visited Rossland, in British Columbia, last September, Professor Armstrong is reported to have said: "One thing that has struck me most "forcibly is that some better method than smelting should be "applied to your low-grade ore. It seems to me that some of the "recent discoveries in electrolytic treatment might be applied to "such ores."

Nearly a year before these remarks are said to have been made, I was instrumental in submitting samples of some of the Rossland ores, on a working scale, to the electrolytic treatment of the General Gold Extracting Company by the Pelatan-Clerici process at their testing works in Denver, Colorado; and the following drawings (Figs. 10 and 11) will give some idea of the arrangement of a mill for treating ore by that process, which, in consequence of the results of those trials, and others of a more recent date, is now in course of erection between the Red

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* Thus: $CNBr + 3KCN + Au_2 = 2KAu(CN)_2 + KBr$.

This equation no doubt represents what happens so far as the cyanide which actually combines with the gold is concerned; but the whole of the cyanide is not converted into $KAu(CN)_2$: probably the largest proportion will be decomposed by the cyanogen bromide, giving potassium bromide, potassium cyanate, and hydrocyanic acid.

The following two equations represent actions that are taking place together in the solution, but which have no constant ratio to one another:—

(I.) $CNBr + 3KCN + Au_2 = 2KAu(CN)_2 + KBr$.

(II.) $CNBr + 2KCN + H_2O = KBr + KCNO + 2HCN$.

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Mountain Railway and the Little Sheep Creek, about three miles from the city of Rossland.

At A, in plan and section, is shown a railway siding with a shed over and an ore bin under it. At B and C are placed the stone breakers and rollers. D is where an elevator discharges the crushed ore over a sampler into a second ore bin, E, whence it is drawn into the Chilian mills, F, F, F. Thence the sludge that passes the screens is taken to the mixing tanks, G, &c., and when mixed in due proportion the liquid is passed to the treatment tanks, or vats, 12 of which are shown (H, H, &c.).

The motive power is situated to the left, and at about the same level as D; and with about 75 B.H.P. actually in use at one time, about 50 tons of ore can be pulverised and treated daily on 300 days of 24 hours in the year.

By adding two Chilian mills, three mixing, and 10 treatment tanks, and 35 B.H.P., 100 tons a day can be similarly dealt with. The sketches—which do not show the power house, the sampling, cupelling, testing, and other subordinate departments, of such a mill—are intended as an example of how ore carried straight by rail from the "dump" can be pulverised and treated for extraction without any handling.

The designs were made and the work laid out, under my own supervision, by Mr. Fisher, of Helena, who has been engineer and millwright to many well-known smelting works and extracting mills in the Western States of America, but who had had no previous experience with this class of treatment plant.

Below is a simple means, given me by Mr. Pelatan, of testing quantitively for the presence of gold and silver in the solutions with potassium cyanide as a solvent at each stage of treatment, which can be easily used by the assayer in charge.*

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* Take 500 cubic centimetres of solution. Heat nearly to boiling point. Add 2 grm. *sulphate of copper* (bluestone) and 2 grm. of *sodium sulphide*. Add hydrochloric acid so as to have a strong acidity. Heat to boiling point. Filter the black precipitate in which all the gold and silver are retained. Scorify the precipitate with 50 gr. litharge. The precious metals are recovered from the lead by cupellation.

The sodium sulphide used in this test is made in a very simple way, by fusing in a crucible one part of *carbonate of soda* and one part of *sulphur*. The crucible must be well covered.

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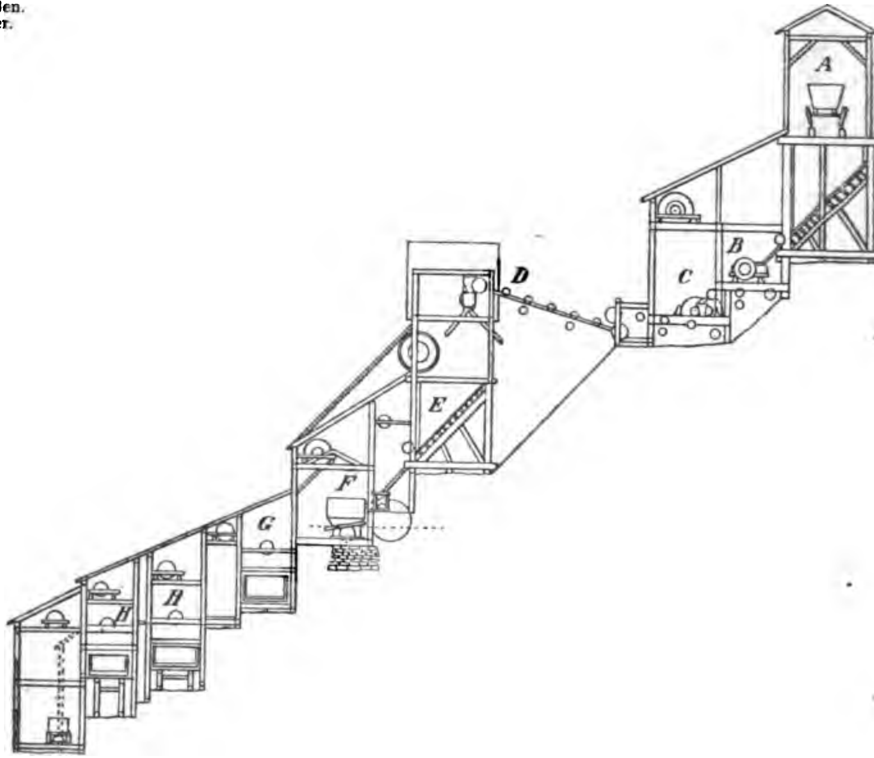

Fig. 10.—Rough Section of Mill at Rossland (B.C.) for the Treatment of Low-Grade Ores by the Pelatan-Clerici Process.

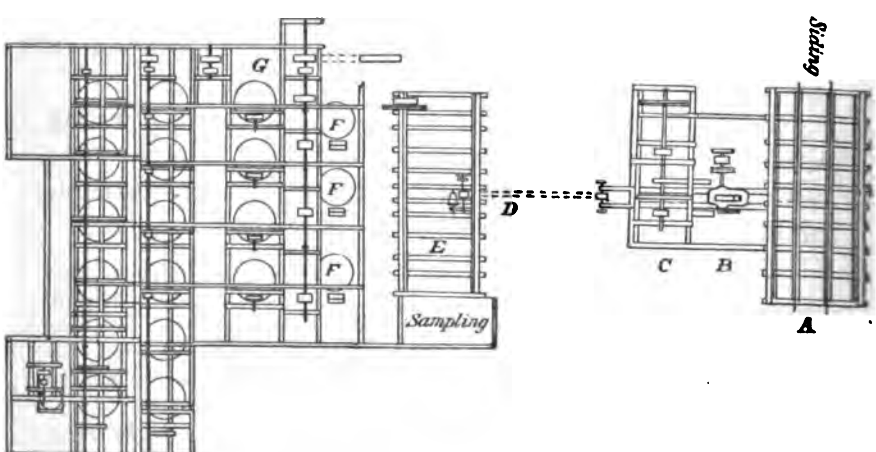

Fig. 11.—Rossland Mill in Plan.

Dr. F. L. TEED: It is really difficult, Sir, to discuss a paper like this, in which a lot of processes for the electrolytic removal of gold by means of solvents are mixed up anyhow with processes for making solvents by means of electric treatment. To make my meaning quite clear, the Siemens-Halske process is mentioned; but that process is a process for removing gold from a solvent: it is not a process for getting gold into a solvent. The Pelatan-Clerici process, on the other hand, seems to be a process for making a solvent by means of electricity, and dissolving the gold by means of the solvent so produced; that is quite apart from any deposition of the dissolved gold subsequently by means of the electric current. These remarks apply also to Molloy's patent, which attempted to remove the gold from solution by means of making an amalgam with mercury. There is one part of the process which I could not understand. The author says in the paper: "Although the preliminary mixing of the solution in a separate "tank may be thus implied, there is nothing to prevent the mixing "being carried out in the treatment tank itself." Now, if that statement is taken in conjunction with the tabulated list that occurs further on in the paper, I should like to know whether part of this gold is extracted in the vat, and the other part, as I gather from the list, extracted from the liquor after that liquor has left the vat. According to the table a certain quantity of gold is precipitated direct, and a certain other part is precipitated from the solution, which I imagine implies that the solution is removed from the vat for subsequent treatment, electric or otherwise. With regard to the chemistry of the process, it is, I think, a little involved. There is nothing in the paper that differentiates between one sort of ore and another. If you electrolyse common salt and get some chlorine, undoubtedly by means of that chlorine you could dissolve gold, and demonstrate it on the table there. You could possibly treat a quartz ore—that is to say, an ore containing nothing but quartz and metallic gold—but neither Mr. Pelatan nor anybody else could treat a pyritic ore that way; it would be simply an impossibility. In order to treat a pyritic ore by this process, using a common salt solution only, it would be essential

Dr. Teed. that that ore should be thoroughly well dead-roasted first. Now we come to the question of cost. "During the whole time of "treatment, agitation, as distinguished from percolation or cir-"culation, is going on." We should very much like, those of us who are interested in the practical application of these matters, to know how much per ton it costs to agitate—I think in a 5-ton lot—how much per ton it costs in electric current, and how much it costs in cyanide of potassium, of which the author cheerfully says 2 lbs. may be required. A little further on it is said this would be wasted! Two lbs. of cyanide of potassium is an important consideration. It would probably cost 1s. 6d. per lb., and there is 3s. gone—a dwt of your ore gone. Compare these statements with the facts of the case as they exist, say, on the Rand at the present moment, where they are not using power to agitate, and not using electric currents to form a solvent; they are not using electricity to break up and waste their cyanide of potassium, either alone or in conjunction with sodium chloride; but they are extracting from ore with cyanide of potassium alone, and using 4 oz., or possibly 5 oz., to the ton of ore. Five oz. would cost, say, 5d., as against the cost in chemicals—3s.—and the cost of electric current and agitation, information on which I have no doubt we shall be shortly supplied with. That the author has not faced the difficulties is, I think, obvious, if you refer to the paper. Nothing that anybody could say could be a greater condemnation of this process than the facts honestly put forward by the author. He says: "Although when not in excess"—referring to sodium hypochlorite—"it is a useful oxidiser, this formation of sodium "hypochlorite has little advantage." (Of course the trained man then says, "Why form it?") "And when potassium cyanide is "added to the solution, it is, when in excess, even when alkaline, "likely to oxidise some of it into potassium cyanate—a decided "disadvantage." Could any opponent of the process have said anything more to its damage than these statements, which I am glad to say have been honestly put forward by the promoters? The paper goes on to describe how the chlorine, produced at such enormous expense, unites with the metallic bases, iron

and copper, and with sulphur, selenium, and arsenic—"being "oxidised by the compound action of chlorine and sodium hypo-"chlorite above alluded to, into sulphuric, selenic, or arsenic "acids." We need not go further. If this is a process for oxidising iron, copper, sulphur, selenium, and arsenic, then it cannot claim to be a gold process. The two things are incompatible. If you have a little gold dissolved, with these compounds and pyrites present, it would precipitate again on the pyrites as long as there was any present. Copper sulphide is practically used as a precipitant daily in large-scale operations for removing the gold from its chloride solution, so that in order to get gold dissolved from one of these ores you would require to oxidise or chlorinate the whole of that ore. Just imagine what that means in expense. Dr. Teed.

There is one statement here which is not a matter of very much moment, as I do not think the Sulman-Teed process will be interfered with very much by this. It says in the paper: "The "difference between this and what is known as the Sulman-Teed "process is that in it cyanogen chloride (or bromide) is added "mechanically to a solution containing potassium cyanide and "gold or silver ore, whereas in this process the conditions to "effect a corresponding reaction are produced electrolytically." I have no doubt that has been put in in error. In the Sulman-Teed process chloride or bromide of cyanogen in presence of cyanide of potassium is claimed as a solvent, however made, and you have only to refer to the patent claim to see that that is substantially set forth as part of the process. The onus of proof rests on the authors of this process to show that it is a success, and this they can only do by supplying figures of the cost in power, chemicals, electricity, and labour, and contrasting them with figures of other processes dealing with similar ores. While, however, plenty of material has been supplied upon which the process must be condemned, the author has not supplied any facts or figures upon which the process could be pronounced a success.

Mr. E. F. HERROUN: I do not think, Sir, I have any special remarks to make. General Webber has kindly thanked me for Mr. Herroun.

Mr. Herroun. some opinions I gave as to the chemistry of certain changes that took place in this process, and any questions asked with regard to any points of chemical detail in connection with the process I shall be quite prepared to reply to. Otherwise I should offer no special remarks. My relation to the subject is merely that of a theoretical adviser, and not of a practical engineer. With regard to the remark which has just fallen from Dr. Teed in reference to the treatment of pyritic ore, I would merely mention that this process may be perfectly valid for treating an ore containing gold plus pyrites, provided it is not swamped with pyrites. Frequently quartz ores contain relatively large quantities of pyrites, yet such quantities may be dealt with by a moderate expenditure of electrical energy in the treatment of them.

Mr. Sulman. Mr. H. L. Sulman: My colleague, Dr. Teed, has already anticipated my remarks on several points, but I first wish to thank the Society for affording me an opportunity of listening to this very interesting paper. Messrs. Pelatan and Clerici, the inventors of the process which forms the essential part of the paper read by General Webber, have set themselves to solve the problem which so many others have tried to accomplish during many years past, but hitherto unsuccessfully, viz., the use of electric energy as an aid to the solution of gold from its ores.

If their process is economically successful—which yet remains to be seen—it will be the first of its kind. This method of using electricity must not be confounded with its application to the precipitation of gold, already one of the best known means for recovering gold from its cyanide solution.

General Webber has given us an interesting *resumé* of the electro-metallurgy of gold, but there are several additional names occurring to us in this connection. Sir W. Crookes, I believe, took out patents some time since for hastening the solution of gold from ore by means of a solvent plus electricity. Atkins was another who some years ago was electrolysing gold-bearing pyrites with a chloride solution, but with what permanent success I am unable to say. General Webber is, however, correct in narrowing down the points for discussion by his assertion that up to the present the only "electric solution process" which has achieved

any success is the process which he has described to-night, as indicated by the list given of ores treated. Mr. Sulman

Glancing shortly at the historical retrospect, the Pielsticker process, being that which gave rise to the litigation which practically broke down the monopoly of potassium cyanide in gold-extraction work, must be gratefully remembered by us all; but the curious point to be noticed here is, that during the trial, as the result of careful experiment, the claim to aid by electricity (originally an essential part of that process) was thrown overboard by the defendants themselves as being practically useless. This fact is a rather serious commentary on the general type of electrical solution processes.

When in Western Australia last year, Dr. Earp (of Kalgurli) gave me details of an experimental trial by one of the later electrical processes described by General Webber. A sample of a certain West Australian ore, containing some 5 oz. of gold to the ton, was, after careful crushing and sampling, submitted to a demonstration plant erected in the neighbourhood. The tailings after treatment were also carefully sampled and sent back for assay, together with the gold actually extracted The latter amounted (so far as my memory serves) to from 40 to 50 per cent. of the whole. The tailings assay, however, came out at practically the same figure as the original ore. Here, therefore, we have an electric dissolving process giving something like 150 per cent. extraction! The explanation, I believe, was that by some oversight the same mercury had been used without clean-up for a previous ore charge. This is, however, the only instance I can recall (beyond what we have heard to-night) of an electric solution process being economically successful.

I would direct attention to the remarkable process patented by Danckwardt in the United States, who claims to use 10 lbs. of cyanide to the ton of ore, besides 2 or 3 lbs. of sulphide of ammonium (or that of another alkali), the object of which is said to be the reduction of the consumption of cyanide to a minimum. The idea of putting ammonium sulphide into a cyanide solution will amuse every cyanider. A large part of the cyanide would be converted into sulpho-cyanides, whilst the

Mr. Sulman. balance would go to form the volatile and readily destroyed ammonium cyanide.

In Clark's patent we have again the ever-recurring extraction of gold from auriferous pyrites by the electrolytic decomposition of salt solution, which (as a process) is old as the hills. Dr. Keith's method mentioned is a *rechauffé* of the work done by Crookes and others years before. As to the use of mercury salts in conjunction with electricity in a cyanide solvent, I think the invariable experience has been that, while for an extremely short time there is a rapid solution of gold, this ceases again almost immediately, owing to the deposition of mercury over the gold particles; from which moment the solvent action of cyanide is almost *nil.*

In considering the Pelatan-Clerici process, we find agitation an essential necessity. It seems to me that this is really a retrograde step. Agitation cyanide methods have been tried for years past; in fact, I may remind members that the cyanide process itself, in its inception, was an agitation method. As first brought forward it was a proposal to agitate crushed ore with potassium cyanide solution in apparatus resembling the old barrel amalgamation plant. But it was soon found that the energy consumed was so great, and—what was of far more importance—the destruction of the cyanide by agitation with the ore was so large, as to render the method economically useless. It was only when agitation was abandoned, and the cyanide employed as a percolation solvent, that it began its successful career as a process. Many times since, in various localities, agitation of cyanide with ore has been revived, and as often abandoned. One of the last occasions was upon another Westralian mine, where agitation and cyaniding barrels were employed together with mercury; but the result was an absolute failure. We have further to consider the plant required by this process to deal with a given mass of ore. As Dr. Teed has pointed out, in South Africa, New Zealand, &c., it is by no means uncommon to have one tank treating 200 or 300 tons of ore in a single charge. One set of tanks in the Transvaal is capable of holding no less than 600 tons of ore each. There the ore is dealt with, directly,

by a dilute cyanide solution without electricity or agitation. The solution is run on, and after a certain number of days it is drawn off, with resulting high extractions, sometimes running over 90 per cent. If, however, we are to treat 200 tons of ore by the Pelatan-Clerici process, we should require, as we see, no less than 40 tanks; and those 40 tanks also demand elaborate fittings of cathode and revolving anode apparatus. They further necessitate the consumption of a large amount of energy, and at present a quite unknown cost of solvent materials. What inducement, therefore, is there to use such a process, when a far simpler plant is doing equally good work? It may, however, be that the author of the paper has unwittingly done the inventors an injustice in giving no single figure of cost. I need not remind members that economy is the touchstone of success. We have had described to us an elaborate process; it may be a very efficient one, but what we must first of all be told is, *its working cost*. As we have seen, the consumption of cyanide is very high. By one of our own processes (bromo-cyanide) we are extracting an arsenical pyritous ore in Canada which sometimes contains as much as 40 per cent. of arsenic. The massive mispickel is here dry-crushed, and filled into vats in 50-ton charges. It is then simply treated with a percolating bromo-cyanide solution, the consumption being only 1 lb. of cyanide and one-third of a lb. of bromide of cyanogen per ton: in 36 hours we extract 90 per cent. of the gold (originally 15 dwt. per ton) without agitation or the use of electricity. It would take 10 Pelatan-Clerici tanks to deal with the same amount, with admittedly a much larger amount of cyanide, and with salt, lime, organic acid, agitation, and electrical energy in addition. Mr. Sulman.

Mr. Herroun remarked that the Pelatan-Clerici process will deal with pyritous ore, provided the gold is not "swamped" with pyrites. But in perhaps the majority of such cases the gold is found in the form of "films" between crystals of the mineral. Here the gold must be attacked by a solvent "edge-"wise," and is consequently "swamped" by the pyrites however small the actual amount of the latter. With such ores, a chlorination treatment, followed by cyaniding and agitation

Mr. Sulman. (which is what the Pelatan-Clerici process practically amounts to), I consider to be ill calculated to extract economically, but await the necessary figures and working costs.

Mr. Bauerman. Mr. H. BAUERMAN: I am glad to have the opportunity of hearing what I believe to be the first public description of the Pelatan-Clerici process given in this country. Like many of the older men familiar with the treatment of gold ore, I have a large sympathy with the system of gradual reduction indicated in the diagram, although that sympathy is no doubt tempered by the remembrance of several disappointments in the same field. The dissolving and electrolysing apparatus in several features recalled the old Hungarian mill, but very largely expanded in size—from about 15 inches to 9 feet in diameter—but the proposal to work it with slimes containing only about 50 per cent. of water seemed to be giving it something of the duty of a brickmaker's wash mill. This seemed to be rather a bold departure from existing practice, but I hope that it may prove to be successful in practice, as such a method could not fail to be of extreme value in districts where a proper water supply is difficult to obtain. I am sorry that I am unable to discuss the electrical problems arising in the process, as such matters are beyond my knowledge.

Mr. Jenkins. Mr. H. C. JENKINS: The paper that we have before us appears to have given rise to a discussion rather upon one particular process mentioned in the paper than one upon a class of processes. We must, I think, thank General Webber for having put together so lucid a *resumé* of what has been done in the past; but one must follow Dr. Teed in the request for more information, particularly as to the "gold recovered from solution," as well as in criticising upon one or two points. In endeavouring to obtain amalgam we certainly do try to get the mercury as full as possible of gold and silver, but we also try to keep out by all possible means some of the other elements that have been named, such, for instance, as the iron and copper, which I am afraid would give rise to very serious difficulty when that amalgam came to be cleaned up, apart from very serious mechanical losses that are sure to occur if the concentration is allowed to go on to any

considerable extent in the vat itself. I should like to ask Mr. Pelatan whether he could give us any particulars of the work done at the De La Mar Mill, Idaho, especially as to what is the cost of working, because it is quite conceivable that under some circumstances a process of this kind would be useful. It is quite possible that by keeping away from the use of an excessive amount of chemicals, and sticking rather closely to the common salt solution, Messrs. Pelatan and Clerici will solve the problem that Molloy set himself, and so get a quick solution of the gold by means of mercury electrolytically charged with sodium where ordinary amalgamation would be of no use. Molloy in his process simply endeavoured to load up his mercury with a certain amount of potassium and sodium, and he utilised these metals to keep the surface of his mercury perfectly clean while he passed a current of crushed ore over it. Some ores, as has been pointed out already, contain just enough easily decomposed sulphide to dirty the surface of the mercury, and to prevent it from wetting the gold, and yet not sufficient to interfere in the very serious way that has been pointed out as being possible by Dr. Teed. That interference certainly could be very serious indeed. Mr. Jenkins.

We must remember that in these electrolytic processes for the solution of gold from ores we never have the ore, and never can have the ore, actually the anode; all we can do is to put the ore in a crushed condition near the anode and attack it with common, rather than with nascent, chlorine. We certainly cannot utilise the special action that we have in mind when we speak of electrolysis—an action due to an artificial potential. If we could only make the ore into a conducting anode, true electrolysis would take place, but Nature has not allowed us to do that. We can make the mercury into a cathode, and with that we have to be content. So that the process would appear, even if it works out at all well in the way of costs, limited to comparatively rich ores rather too dirty to be classed as "free milling" ones, and yet not heavily pyritic. Of course, if an ore is a free milling one, we need have no other appliance than a stamp mill, which is in nearly every case the best for it if intelligently used; and if it is distinctly refractory, there are other processes for its treatment,

Mr. Jenkins. already well known, that are far cheaper than the Pelatan-Clerici process can possibly be. The energy used in the plant seems to be very large indeed. From the figures that one can get as to the De La Mar Mill, and the figures indicated in this paper, we seem to require something like 36 H.P.-hours per ton of ore. That is a considerable thing in itself as regards cost, let alone the cost which is represented by two or more pounds of cyanide per ton of ore, which is really serious for any but rich ores. One must remember, in looking over the ores given in the tables in the paper, that such ores would be treated at a total cost of about 2s. 6d. or 3s. a ton, besides crushing. I have one or two figures that I have picked haphazard quite recently from the Rand; at the Crown Reef, they have been treating their slimes, which are more difficult to treat than tailings or fresh ore, for something like 3s. 9d. per ton. Altogether the ore seems to run out about 6s. a ton, millings and cyanide. Again, at the Wemmer Mine, the milling—that is to say, the crushing and treatment over the amalgamated plates—seems to run to within a very small fraction of 4s. a ton, and the cyaniding at something like 2s. 4d.; making a total, with concentration and other charges, of a little over 7s. a ton of ore. In face of those costs, it is quite out of the question to contemplate so costly a plant as this seems to be for such ores as are given in the table. At the same time, it is possible to admit that there are a few rich ores slightly refractory that would be treated by this process perhaps as economically as by any other, or even better.

Mr. Pelatan. Mr. L. PELATAN: I am not going to criticise any process, but simply will give some explanations which have been asked for. It has been asked if the mixing vats which have been alluded to by General Webber in his contribution were part of the process, and if part of the gold was recovered in these vats, and part in the treatment vats. The object of these mixing vats may be thus briefly expressed: When the ore is crushed in a stamp battery it is pulverised gradually; but 2½ tons of the crushed ore are treated in a treatment vat at one time. It is therefore impossible to let the pulp flow direct from the stamps into the treatment vats; that is the reason why mixing vats of a sufficient size to collect a

certain quantity of pulp are, when crushing with stamps, placed between the battery and treatment vats in order to act as reservoirs of the sludge to be treated. Mr. Pelatan.

After each charge has been treated, the treatment vats are filled up again from the mixing vats.

From this description it is obvious that the mixing vats are no essential part of the installation, and they may very well be dispensed with when, for instance, dry crushing is applied.

The explanation of the fact that part of the gold has been described as recovered partly in one way and partly in another is this: When the pulp, consisting of ore and water, is charged in the treatment vat, the stirrer is run for about two hours without cyanide, only common salt being added.

The PRESIDENT: And current.

Mr. PELATAN: The electric current is passed, of course; and if a sample of the pulp after these two hours is taken, it is noticed from the assay then made that a certain quantity of the gold contained in the ore has been extracted and caught in the mercury in the bottom of the vat.

After this first stage—that is, after running the charge for some time without solvent—the cyanide is added, and treatment is continued until it is completed.

The balance of the gold recovered is extracted during this second stage of the operation, after cyanide has been added. In this way may be explained the difference made by General Webber in his paper between the gold caught in the first and in the second stage of one operation.

Great stress is put on the power that is consumed in agitating the pulp. Of course, in order to compare things, we must deal only with those that are comparable.

The cyanide process was first an agitating process, but the agitation was performed in heavy barrels, which had to be revolved as well as the charge to be treated.

We only agitate the rather thin pulp contained in the treatment vat with a stirrer to which a circular motion is imparted. We have calculated over and over again, and we know by experience in large works, that the power consumed for the

Mr. Pelatan. agitation is half H.P. per vat capable of treating 5 tons of ore in 24 hours. This is equal to one-tenth of H.P. per ton of ore treated.

With regard to the expense of electricity, I may say that we usually employ a current of about 7 volts pressure. The number of amperes of course must vary according to the quantity of gold that has to be precipitated.

With the ordinary ores which we have to deal with we find, as a rule, that about 70 amperes is sufficient.

70 amperes by 7 volts is equal to 490 watts, which is considerably less than 1 H.P. It is about ⅔ H.P. In fact, we very often consume no more than ½ H.P. for the current. This means the consumption of 1 H.P. by the agitation and the current for 5 tons treated, or ⅕ of H.P. per ton of ore.

It is said in General Webber's paper that 36 H P. more will be required in a 50-ton mill which is now in course of erection at Rossland, B.C., in order to eventually double the capacity, but the extra horse-power includes part of the extra power required for the crushing. I know no process that can treat the ore without milling, except in a very few mines. I know only of one group of mines in America where they can cyanide the ores without crushing them, but that is quite exceptional. These are the Mercur Mines, in the Camp Floyd district of Utah, where the ore is of so porous a nature that it is perfectly sufficient to break it to the size of pigeons' eggs, when it is charged in the treatment tanks and the cyanide run in, with the result that about 85 per cent. of the gold contained is readily dissolved and extracted.

Several explanations have been given by Major-General Webber with regard to the treatment in the vat; these are of a chemical nature, and they are intended to explain the rather complicated reactions that occur, most of these reactions being incidental. I am not quite sure that some of them may not be explained in some other way; and others which certainly occur cannot be prevented, although they do not help the treatment, and some even make it more difficult. The main thing is that we extract and recover the gold at a cost of so much, which is considered in a great number of cases as very economical.

So far as the solvent is concerned, we use cyanide, and the assumption that we claim to deal with a new dissolving agent is not correct. Mr. Pelatan.

The consumption of cyanide is a very important point, I admit, but here again we must compare things that will compare together. If you take the tailings of the Rand, you will find that they consist of a washed material run simply over amalgamated plates, the slimes of which have been taken away, and therefore these tailings are in the best of conditions to undergo the cyanide treatment. You cannot compare such clean tailings containing a low percentage of gold with very rich ores—for instance, with the Hannan's Brownhill ores of Western Australia, which have already been mentioned in the present discussion, and which contain as much as 4 or 5 oz. of gold, and are altogether different as to their physical nature.

The consumption of cyanide will necessarily be very different in the two cases.

The 2 lbs. of cyanide mentioned by General Webber as being possibly required is altogether a maximum. I may say that as an average we do not consume more than 1 lb. when treating ores (not tailings). We have even a mill in Russia where we have been working rather difficult ores with 4 or 5 per cent. of arsenical pyrites, and we need not use there more than about ½ lb. of cyanide.

So many points have been raised, and so many questions made, that I really am at a loss to remember them all just now. However, I am at the disposal of the audience, and if I have forgotten to explain some points, I shall try to give the best explanation I can.

Mr. D. A. LOUIS: May I suggest, Sir, that perhaps the character of the ore might be added to the table given in the paper? Mr. Louis.

Mr. H. L. SULMAN: May I ask one further question, Sir, relative to the use of the organic acids referred to in the paper? This has been mentioned as an essential part of the process, and it therefore seems important that we should receive information as to the reason of their use, cost, &c. Mr. Sulman.

The PRESIDENT: I think it would be better to adjourn the discussion until our next meeting.

I have to announce that the scrutineers report the following candidates to have been duly elected:—

Member:

A. E. Brooke Ridley.

Associates:

Charles Clement Atchison.
Robert John Halliburton Beaty.
Jehangir N. Bellihomji.
Ignatius Bulfin, B.A.
Alfred Quintin Carnegie.
Edgar T. Everett.
Samuel Hearne.
S. Pauls.
Walter Powles.
Frederic H. Read.
Henry Sclater.
Joshua Shaw.
Ernest F. Szlumpér.
K. M. Tarachand.
Harry John Taylor.
James Alexander Walker.
William Bryden Walker.
George Cambridge Weston.

Students:

Anthony Marinier.
Charles Bernard Monson.
Charles John Simeon.
Hugo Stephens.
Harold John Winton.

The meeting adjourned to February 9th.

The Three Hundred and Tenth Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Wednesday evening, February 9th, 1898—Mr. JOSEPH SWAN, F.R.S., President, in the Chair.

The minutes of the Ordinary General Meeting held on January 27th were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council:—

From the class of Associates to that of Members—

Oswald M. Andrews. | Joseph Slater Lewis.
Alexander Alfred Dircks.

From the class of Students to that of Associates—

Roland Francis Browne. | Thomas Archibald Kerr.
William S. Naylor.

Mr. Hilton Johnson and Dr. Borns were appointed scrutineers of the ballot for new members.

A donation to the Library was announced as having been received since the last meeting from the Italian Minister of Posts and Telegraphs, to whom the thanks of the meeting were unanimously accorded.

The PRESIDENT: The next business is the resumption of the discussion on General Webber's paper. Since the last meeting I have received a communication from Mr. Sulman, who sent a long list of questions, which at his suggestion were sent to M. Pelatan, and we have received that gentleman's replies; and I

propose that both the questions of Mr. Sulman and M. Pelatau's answers be read. They greatly assist in the elucidation of the subject.

The SECRETARY then read the questions of Mr. Sulman, with Mr. Pelatan's replies thereto, as follows :—

Mr. Sulman. Mr. SULMAN : *Question* 1.—What are the capital cost of each tank and the total tankage necessary, and the installation cost of this on a scale to treat 100 tons per 24 hours *without crushing plant?*

M. Pelatan. M. PELATAN'S *Answer*.—One treatment tank (*i.e.*, a 9-ft. diameter tank of 5 tons capacity per 24 hours), with connections and appliances, costs £60. Twenty such tanks are required to treat 100 tons of ore per day. The cost of an installation having a capacity of 100 tons per 24 hours does not exceed £5,000, everything complete and ready to work, but *without* crushing plant.

Mr. Sulman. 2. The sludge having been stated to contain 60 per cent. of water only, and being therefore very thick, how is it possible to agitate thoroughly such a mass at an expenditure of only ½ H.P. ?

Would not this imply that the whole mass resting upon a layer of mercury would be revolved *on the mercury*, as on a turn-table, and hence efficient agitation would be reduced to a minimum ?

Where agitation of an ore charged with cyanide in a stationary vat, under like conditions of liquor contents, has been hitherto tried, it has been generally found to consume energy to an extent approaching 1 H.P.

M. Pelatan. *A*. A sludge with 60 per cent. of water may be still very thin. All depends upon the density and nature of the ore. It is true that sometimes with slimy and light ores as much as 100 per cent. of water must be used.

At all events, a very good agitation is obtained with only ½ H.P. per tank, corresponding to $\frac{1}{5}$ H.P. per ton of ore. The trials alluded to in which as much as 1 H.P. per ton of ore was required to ensure agitation must have been made with revolving barrels, with which friction is very great.

3. Total cost required for treating a double charge (of 5 tons) of ore in one tank for 24 hours, including the following items:— Mr. Sulman.

(*a*) Cost of electrical energy.
(*b*) Cost of the agitation energy.
(*c*) Cost of salt.
(*d*) Consumption and cost of cyanide.
(*e*) Details as to the use and necessity of the consumption and cost of the organic acids which are mentioned as essential parts of the process.
(*f*) What is the weight of the charge of mercury for each tank, and the loss of mercury on each operation?
(*g*) What is the cost of labour and supervision per tank?
(*h*) Total cost per ton, exclusive of crushing.

A. The cost of treatment per ton for the average ores, on the basis of 100 tons treated per 24 hours, will vary according to the consumption of chemicals, and with circumstances, between the following limits:— M. Pelatan.

	s.	d.		s.	d.
Cost of chemicals, including salt, cyanide, lime, mercury, &c.	2	6	to	4	0
Cost of power	1	0	to	1	6
Cost of labour	0	6	to	1	0
	4	0		6	6

In the case of slimes the cost may still be reduced.

600 lb. of mercury is charged in each tank. The loss is purely mechanical, and amounts to no more than 2 oz. of quicksilver per ton of ore at the outside.

4. What is the fineness of the bullion from the mercury obtained from ore varying in character? Mr. Sulman

A. The bullion obtained from average ores is at least 800 fine, unless they are oxidised and yield copper oxides or copper salts, when the copper is dissolved to a certain extent, and afterwards deposited in the mercury together with the gold and silver. M. Pelatan.

Mr. Sulman. 5. Is any cyanide recovered from the spent liquors? and how are these dealt with?

M. Pelatan. *A*. The exhausted sludge is thrown away without the liquors being separated; and no attempt has been made to recover any cyanide, as it would not be worth the while.

Mr. Sulman. 6. What would be the cost of dealing with 100 tons of pyritic concentrates, or heavily pyritic ore containing copper and arsenic, such as is being treated by the bromo-cyanide process at Deloro, in Canada, including cost of clean-up?

M. Pelatan. *A*. No accurate (nor even approximate) figures can be quoted for an ore the composition and nature of which are only given in a very general way, and which has not been tested.

Mr. Sulman. 7. What information can be given as to the nature of the ores scheduled on page 57, with reference to the mineralised contents, the nature of the gold contained therein; as to coarseness or fineness, and how far such could be amalgamated (before solution treatment) by ordinary methods?

M. Pelatan. *A*. The ores scheduled in General Webber's paper (page 57) are respectively:—

Rose—Oxidised iron ore with very fine gold (very slimy).

Delamar—Decomposed feldspar and quartz with some pyrites and some argentite.

Oaxaca—Rusty quartz with some pyrites.

Bassick Tailings—Quartz with pyrites, and tellurides.

Baby—Soft conglomerate with iron pyrites (very slimy).

Phœnix—Talcose quartz (slimy)

Leroi—Diorite with an abundance of iron and copper pyrites.

Alma—Talcose quartz with a considerable quantity of iron pyrites.

Miller—Oxidised iron ore with some manganese.

All these ores give very poor, and in some cases no, result by the ordinary methods of amalgamation. M. Pelatan.

8. In the treatment of an arsenical copper ore how much arsenic and copper go into the mercury? and how is this purified for re-use? Mr. Sulman.

A. Practically no arsenic goes into the mercury, and almost no copper when present as copper pyrites. Only copper oxides and soluble salts are really objectionable. M. Pelatan.

The mercury in a vat can be used over and over again, the amalgam being drawn from it every fortnight or every month, as the case may be.

9. We are told on page 62 that iron and copper go into solution to a considerable extent under the action of chlorine and hypochlorites, and that these metallic salts are precipitated by lime before the cyanide is added. What becomes of the ferrous and cupric hydrates that form in the sludge? and how is this cyanide-destroying effect overcome, and at what expense? Mr. Sulman

A. The preceding answer deals partly with this question as well as with the eighth. Nothing more than adding lime is done to prevent destruction of cyanide, and the result proves to be both good and cheap. M. Pelatan.

10. As to the foot-note on page 57 with reference to the production of slimes, I would point out that the *actual extraction* by fine crushing does not become less effective, *but more effective;* it is the difficulty of *percolation* that is the bar. Mr. Sulman.

As to the readiness of makers of milling machinery to supply plant for crushing ores to 80 or 100 mesh, there can be no question of this; they would be only too glad to get the orders for such greatly increased crushing plants as would be necessary. It is those who would have to pay for the milling of the ore to such extreme fineness who would look blank at the prospect.

A. All percolation processes are limited in their efficiency, as far as extraction of the gold is concerned, by the fact that it is impossible to percolate the solutions M. Pelatan.

M. Pelatan. properly through slimes. The Pelatan-Clerici process does not resort to percolation, and therefore its extracting power is much greater.

For medium-grade and rich ores very fine crushing will lead to an extra expense, which will be amply repaid by the gain made in the recovery. After having tested the ores of more than 200 mines in the United States, we find that, when crushed to from 80 to 100 mesh, only 10 per cent. of the ores treated will yield less than 80 per cent. of the gold contained therein.

Mr. Sulman. 11. On page 63 it is stated that the dissolved *cyanogen* will readily unite with the lightest particles of gold: this is quite incorrect. A solution of cyanogen in potassium cyanide is *quite* without effect on gold; it is only *nascent* cyanogen which acts in this way.

M. Pelatan. *A.* The cyanogen alluded to here is "*formed at the* "*anode*" (by the electric current); it is therefore in the nascent state, and, according to Mr. Sulman himself, it can unite with the light particles of gold.

Mr. Sulman. 12. Finally, an essential point has been missed in relation to slime treatment. The anthor of the paper has not shown in any way that slimes can be more readily dealt with by this than by any other process.

The tabulated results on page 57 show an average extraction of 82 per cent. The balance of 18 per cent. would easily correspond to the gold contained in from 30 to 40 per cent. of slimes produced from such ores, and the invariable experience is that *electric amalgamation* or electric *deposition* of gold from cyanide slime *will not work at all*, while the mass remains *turbid with suspended slimes*. If the contrary were the case, there would have been (for the past 10 years) no *slime question at all*. Hundreds of experiments have been made with the view of *electro-depositing the gold from a cyanide solution* in which the *depleted* slimes still remain suspended, but a *clear solution* is imperatively demanded to effect this. The same difficulty applies in as great

a degree to electro-amalgamation. I could instance at least a dozen cases within my own knowledge in which this has occurred, and would again ask what evidence the inventors have that they have overcome this difficulty. Mr. Sulman

A. The fact that the Pelatan-Clerici process can deal with ores crushed to 100 mesh, 150 mesh, and more is sufficient evidence that it can deal with slimes. The assertion in the opening sentences of the second paragraph in this question appears to be that, if the tailings of the Rand could be cyanided together with the slimes, the gold in the slimes would not be dissolved at all, or we fail to understand it. We have extracted 82 per cent. of the gold from the ores scheduled on page 57 (said ore being crushed to pass a 40 mesh). If the crushing had been made for a 60 or 80 mesh we would have extracted more, and consequently left less in the tailings. M. Pelatan.

The entire remark about electric amalgamation or electro-deposition is exceedingly valuable to us, especially as it is expressed by an inventor. The novelty of our process could not have been established and emphasised by a more competent authority.

Mr. D. A. Louis [*communicated*]: I regret I shall not be able to be present at the adjourned discussion on Major-General Webber's paper, "On the Electro-chemical Treatment of Ores, "&c.," inasmuch as I had not an opportunity of speaking last time, and I take a particular interest in the main object of the paper, namely, the Pelatan-Clerici process, as it is the process that has been mentioned to me as of probable utility by some directors of companies with which I am connected; and I must remark that the paper before me does not offer any basis for coming to such a conclusion in the particular cases in which I am interested. But, as this may be due to reticence on the part of the author, I should like to draw attention to some omissions in the paper, because the process seems to me to be admirably conceived, and should be successful in the treatment of certain Mr. Louis.

Mr. Louis. classes of material where obvious mechanical difficulties and chemical complications could not be encountered.

Turning now to the paper, the general data given towards the end and elsewhere are far from indicating economy; but, as these have already been commented on by others, I will not refer to them further, beyond saying that the mill illustrated is prodigious.

It is therefore in connection with the achievements set forth in the table on page 57 that it would be well for the credit of the process that further information were forthcoming, for as the numbers now stand there is nothing whatever to show that the process is even as good as, much less better than, any process in use—that is, whether as good or better results could not have been obtained by any other process: for instance, there is nothing to indicate that the ores might not have yielded even better results by simple amalgamation, chlorination, or cyaniding; there is nothing to show the extent of the operations from which the results were obtained; there is nothing to show how the operation progressed in each case; and there are no particulars of materials, time, power, labour, and cost; and then there is nothing to show whether the extraction was good, bad or indifferent, from the point of view of the product obtained.

Without further multiplying examples of omissions, it seems to me that considerably more information than is included in the paper is required to demonstrate the Pelatan-Clerici process to be what we are told by its advocates it is.

Mr. Cooper. Mr. W. R. Cooper: Might I ask, before General Webber replies, whether the action of chlorine as described is essential to the Pelatan-Clerici process? Owing to the constant agitation very little of the gas can escape combination with the caustic soda which is simultaneously formed. It would therefore seem more advantageous to add the chlorine mechanically. There is no particular virtue in electrolytic chlorine for such a purpose, as it can only be considered nascent at the anode surface.

Mr. Sparks Mr. J. Noel Sparks [*communicated*]: As one of those interested in such metallurgical processes as have been referred to, and having—particularly in 1894 and 1895, as well as pre-

viously and subsequently—especially investigated the earlier Mr. Sparks. application of metallurgical processes for the extraction of gold in the United States (the home of so many such processes) and elsewhere, I submit some remarks that may not be uninteresting on General Webber's paper.

I believe Dr. Teed has already drawn attention to the difficulty of following the evolution of the various processes in the arrangement of the earlier ones referred to by the author. While hoping to avoid any repetition of his remarks, I might suggest that we should have been able to form a clearer idea of the sequence of the improvements suggested by the various inventors referred to if, in place of dealing with them chronologically, the author had grouped them under separate headings, according to the especial purpose or function of the electric current in each process. Thus in several of the processes the electricity is merely employed to render the mercury more active for amalgamation, as in the first-mentioned experiment by Crosse. These might form Class I. Class II. would include processes where the electricity is intended to assist a chemical solvent in bringing the gold in the ore into solution, as is required by Rae's 1837 patent described, and his followers. Class III. would be composed of processes using the electric current for separation of the gold from chemical solutions by which it has been previously dissolved by electrolysis of such solutions, as proposed by Rae in 1837, and now exemplified by Siemens & Halske, &c.

The processes in Class I. should, of course, be more properly described as "Electro-metallurgical" than "Electro-chemical" processes. I venture to think such an arrangement would have enabled us more clearly to follow the development of the use of electricity in this industry, which I take to be the object of the historic portion of the paper.

To turn to the paper itself, on page 39 the author describes Bagration as having dissolved gold in cyanide of potassium in 1843 "under the influence of the galvanic current." This is evidently a mistake, as in the article referred to the operation is described as follows:—Gold dust was precipitated from a solution of auric chloride by ferrous sulphate, well washed, and mixed with

Mr. Sparks. cyanide of potassium; and Bagration himself continues, after describing the *recovery of the gold* by the galvanic current: "It "is evident, then, that in this operation the gold must have "dissolved chemically, and without the intervention of the "galvanic current, seeing that platinum was used for the anode, "and not gold."

Regarding the statement mentioned that Rae's proposals never went beyond the experimental stage, it may be interesting to hear that Professor Rossiter W. Raymond, of New York, has stated that about 1870 he saw a working apparatus of Rae's in the form of a cylinder, or barrel, capable of treating from 10 to 15 tons of ore at a charge.

It was probably, from the description, an amalgamation process—using the aid of electricity—described in a later patent by Rae than the one here given. This was at Fall River, Colorado. It may be noted also that Rae was a most prolific inventor, and indefatigable in applying electricity in processes for the extraction of gold in almost every conceivable way, as testified by his numerous patents between 1867 and 1887. When I met him in 1895, however, in America, I could get practically no information as to his early doings—probably because he thought his information had a monetary value in view of the litigation then proceeding in the cyanide case.

I may note in passing, with reference to the author's remark in describing Hannay, on page 43, as to making the treatment vessel itself a cathode, Rae seems to have also done this in some of his early patents. Indeed, some anticipation of most contrivances can be found in his descriptions.

With reference to the author's remarks on the Haycraft apparatus of 1894—with which, however, I am not acquainted—it reads to me, from this description, to be identical with that of Pelatan and Clerici, with the exception that the bottom of the pan is depressed towards the centre. When cold it is only partially filled with mercury, which expands on heating and covers the whole pan; thus, as is quoted in the note, "coming into contact with the coarser" (*coarse* would have sufficed) "particles of gold, which settle down" (*i.e.*, have already

settled down previous to the heating) "at the bottom of the "pan" (on the hitherto exposed portions of the pan) "by "their own specific gravity." As to the second portion of the note, would the quotation not be quite easily intelligible if the space of $\frac{1}{4}$ inch be taken as the distance, horizontally, of the end of the anode, or stirring arm, from the side of the containing vessel or cathode, and not taken as the vertical distance between anodes and cathodes, as is apparently done? If this reading would hold, the only difference between the two forms of apparatus would lie in the baffle boards, regulating pins on stirring arms, &c., introduced by Pelatan and Clerici, and the conclusions (2) and (3) on page 49 regarding the Haycraft apparatus might be modified. Mr Sparks.

As to Keith's process, on page 52, and Professor Silvanus Thompson's opinion, it is only to be expected cyanide of mercury should act more strongly on gold, as, weight for weight, it contains more available cyanogen than the potassium cyanide.

I am rather surprised to find no reference to Dr. H. R. Cassel's process of 1883, the object of which was to produce nascent chlorine in presence of ore by electrolysing salt solution, which is apparently credited to Pelatan-Clerici on page 61 as a novel feature, and for the development of which the well-known Cassel Company was originally formed. On this I need only refer to Dr. T. K. Rose, who, in his book on gold, says: "A solution of salt in "contact with the ore is decomposed electrolytically, and the "chlorine thus set free attacks and dissolves the gold, which is "deposited in a hollow iron shaft in the centre of the vessel. . . . "It is interesting as being the first attempt to generate chlorine "by electrolysis for attacking gold."

Referring to the author's remark concerning the production of "slimes"—a couple of paragraphs down—it may be as well to remark that the loss in slimes is due often rather to the impossibility of catching these slimes produced by wet crushing under stamps, when the pulp is fed direct into the leaching vats, and in such cases the loss is due to the fact that wet crushing is used in place of dry crushing, now so prevalent in the United States for leaching processes.

Mr. Sparks. As to the author's description in detail of the Pelatan-Clerici process, we must acknowledge the ingenuity of the inventors in their devices for overcoming well-recognised difficulties in such work, and we shall await figures as to cost, &c., from actual work with interest. These can only be given reliably after a considerable time. In such plant it appears certain that, as to cost of working and maintenance, work on a small scale—I mean with few vats, &c., rather than small ones—cannot give infallible data for large installations. For comparatively rich ores—suitable to pan amalgamation—we have here a process that undoubtedly promises excellent results. As to the applicability and efficiency of the process to treatment of low-grade ores with necessary economy, we shall only be able to judge when mills on an extensive scale have been some time in operation; and I think positive discussion on this point is rather premature—at least upon the facts as yet before us. It must always be borne in mind that the chief requirement of apparatus is simplicity and the minimum of wearing and adjusting parts, whilst that of an extraction process is that it should as far as possible work of itself without the aid of externally introduced force. I must say that purely chemical processes seem to me to be those most likely closely to approach this ideal for the solution of gold from ores. We all wish the author and inventors every success with this new plant in British Columbia, and shall hope to have full details when it has been achieved.

Maj.-Gen. Webber. Major-General Webber, in reply, said: There can be no reservation as regards the pleasure that has been afforded me by the remarks of all the speakers, except here and there where they have replied rather fully to statements that they did not hear me make, such statements not having been contained in my notes. For instance, we were told that my figures gave an expenditure of energy of 36 H.P. hours per ton of ore treated, but that is not the case. On that statement some argument was raised as to the cost. I need not say that the 36 H.P. has no foundation. I am reproached for not giving costs in a paper which was intended to elicit information on the scientific aspect, and not the commercial side, of my subject. The answer to the gentlemen who have given

you the costs of the cyanide treatment of slimes at the Rand it does not concern me to give, beyond dealing with one or two points. For instance, with reference to Mr. Jenkins's remarks with regard to the treatment of tailings and slime at the Rand, I think he must have forgotten that in the past they were quite two separate questions. The treatment of tailings is an operation which has long been in full work by means of the MacArthur-Forrest process, but the treatment of the slimes in the enormous heaps that have been accumulating for years is one that has only quite recently approached a practical solution. Mr. Jenkins gave us costs—6s. and 7s. per ton—for milling and cyanide treatment, which does not include the further cost of the treatment of the old slimes. The question is one that can only be approached when the treatment of fresh slimes forms one stage of the whole process, and when there are very large quantities to be dealt with. For instance, 500 to 1,000 tons a day on the Rand is a figure they have to deal with; and that is a condition of things, as the meeting no doubt is aware, that is extremely rare in other parts of the world; and therefore 6s. to 7s. is only a very exceptional cost. In making any fair comparison of costs, that of milling or pulverising being equal, there remains on one side—and that is the side which Mr. Jenkins had in his mind in speaking of 6s. and 7s. a ton—three processes, namely: (1) Concentration, (2) treatment over the amalgamating plates, (3) cyaniding and deposition of the tailings; as against only one treatment with electrolysis and agitation, as I tried to describe, in the Pelatan and Clerici process, and as illustrated in the diagram overhead. I think the speaker forgot to lay the subject of costs in this way before you—although without any intention of being unfair—and probably the meeting did not understand that point. He also failed, I think, to tell you that the time occupied in the process, and the space covered by the plant, is three to four times greater in the former—that is to say, in the process costing 6s. or 7s. which he referred to—than in the latter; besides which there is a factor which varies everywhere—namely, the cost of labour—and I think that alone is a sufficient reason for my having avoided costs. It would have been too large a subject. Now the question

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Maj.-Gen. Webber. of labour is a factor which alone prevails to make the treatment of the enormous mass of tailings on the Rand at all commercially possible. I do not know whether the speaker was aware of these conditions when he took up his figures "haphazard," as he told us—namely, that the Kaffir hand labour employed to fill and empty the percolating vats, each containing as much as 600 tons, costs only 2s. a day, as against 10s. to 12s. in mining camps where white labour is available. That upsets all comparisons of cost. It is also noticeable that two other speakers—Messrs. Sulman and Teed, who are themselves inventors of a process, which they mentioned—were anxious for some information as regards costs; but they did not give us any scientific details of their own process, which would have been extremely interesting to all of us, and would have enabled us to arrive at a comparison between the processes which deal with the various combinations I have referred to. Much less did they give us any estimate of its cost. There is only one report that I can find as to its costs. It is, I believe, a percolation process pure and simple. It is used, I understand, for dealing with an ore at a mine in Western Australia, called the Hannan's Brownhill Gold Mining Company. The ore contains 4 oz. of gold to the ton, with an estimated extraction of 80 to 90 per cent. But in that case the dusts which are formed in the mill, and which, when wet, form slime, have to be separately treated by what is known as the "filter press" plant; and when telluride and sulphide ores are dealt with, roasting, following on to dry crushing, must be used before the extraction stages are even begun. I am not surprised that these, at the rate of labour in West Australia, run up the aggregate cost of pulverising and treatment to at least 18s a ton. This is a figure far in excess of anything I have described, and it is quite out of the question for the practical treatment of low-grade ores. The meeting will have observed that the list of results I gave in my paper is one of extremely low grade ores, of under an ounce to the ton. Of course when one has to deal with 4-oz. ore a cost of extraction at 18s. a ton is not so important. I only refer to these matters because I want the audience to understand that costs are far more governed by local questions than by anything

else. To give another instance, let me draw your attention to the section of the Rossland Mill which is given on the wall. By it engineers can appreciate that the cost, where very little labour in controlling machinery, and none in handling the material, is required, is affected in a very important way. If my paper had been an engineering description of the work at a particular mine or mill, naturally the costs would have been given; but even then their value for purposes of comparison, which I had no desire to elicit, would be largely affected in the way I have stated. Maj.-Gen. Webber.

With regard to Dr. Teed's remarks, I am sorry I failed to make myself clear to him in my description. The precipitation, or what might be called the settlement, of the free gold, for instance, is effected in the early stage of the one process, which is carried out during the, say, 10 hours' treatment in the vat, of which a diagram is given on the wall. There is no subsequent treatment, electric or otherwise. Whether the ore contains quartz, or is pyritic, it is subject to the same electro-chemical actions, only varying in length of time, quantity of chemicals, conditions of current, and results. I did not say that in very special cases the assistance of roasting may not be also resorted to. I am glad that speakers have testified to the "honesty" of the description of what may occur in the process of the treatment in presence of well-known various conditions that are encountered in various ores. In papers I have read before the Institution in the past, I have been in the habit of presenting whatever subject I have treated as impartially as possible, and therefore I take no credit for that "honesty." The special pleading for advertising or other purposes is so distasteful that one may err sometimes by disparaging that which one estimates most highly. At any rate, it is a fault on the right side. I think Dr Teed forgot that the object of our meeting—we being electrical engineers—was to deal chiefly with the electric side of the question. I do not see where his evidence is, that, what I attempted to describe is a condemnation of the Pelatan-Clerici process. I certainly did not intend it to be so. I tried to point out both its defects and its merits. I only hope that, if anyone will follow up what I may venture to call the

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foundation of what I have laid before them, they will be led to trace the course of failures and difficulties they may meet in the use of a process in which electro-chemical treatment is used; and I hope also they will be led to make further discoveries in the same direction. Why I have not referred to the remarkably good work that Sir William Crookes has done in the past, in connection with the treatment of ores, is that I do not know of any publication of his which deals with the electro-chemical treatment combined with agitation. One of the very few points raised in the discussion which appears to me to bear specially on my subject—viz., the work that is done between the anode and cathode—is interesting: namely, as to the effect of a turbid solution, as compared with a clear one, on the electrolytic actions and reactions. You have heard Mr. Sulman's remarks, and M. Pelatan's rejoinder, on that part of the subject. I may say, in passing, that neither of the gentlemen who are the inventors of the process ever saw my notes until they were printed and circulated, just before the meeting. I do not know that even now M. Pelatan has quite thrown the light on the subject that it requires. We know that the turbidity must be absent in the Siemens-Halske process, in which the gold is deposited from a clear solution that has quite settled. It is probable that in what may be called the "circulation" processes generally the same condition is essential to success. In my paper I alluded to two or three of the "circulation" processes so far as they were illustrative of my own particular subject; but of course there are a great many more—in fact, I should not like to venture on an estimate of the number of "circulation" processes that have been invented, and some of which have even been put to work. The speakers, I think, who condemned agitation, and who have had bad experience with that kind of treatment, have probably dealt with circulation combined with deposition only. The Siemens-Halske process of deposition of gold from a cyanide solution is the best example of the circulation of a clear liquid, but probably their (the speakers') bad experiences have been where they have circulated a turbid liquid and passed it over lead or zinc plates in several well-known ways. It also appeared to me as if the

objectors to agitation had forgotten that precipitation and amalgamation, and not deposition, is the ultimate object; but, from the questions that have been read out this evening put by Mr. Sulman to M. Pelatan, I find he did not forget it when he came to write out his questions after last meeting. At that meeting he certainly left me under the impression that he had forgotten we were dealing with amalgamation, and not with deposition; and I think his statement that "amalgamation produced electrolytically under these "conditions will not work," requires proof. He only says there are many cases in which it has been tried and has failed. His broad statement that "electric amalgamation from cyanide "slime will not work at all whilst the mass remains turbid with "suspended slimes, but that a clear solution is imperatively "demanded to effect this," is, I agree with M. Pelatan, entirely refuted by the results of the process of treatment which bears his (M. Pelatan's) and Mr. Clerici's names; because, besides ores of all grades direct from the pulveriser, both tailings and slimes separately have been successfully treated. These are statements which the meeting must accept as from these gentlemen, who are, I believe, neither of them, more inclined than other inventors to claim greater successes than they can verify after their system has been three or four years before the mining world. If there had been a shadow of authority for this statement of Mr. Sulman's, of course my notes ought never to have been submitted to this Institution. If electricity is no use, my paper would simply have been one which might have been brought before the Association of Mining Engineers, or any Institution which treats the question generally. Brought before this Institution, it was with the view of bringing under the notice of our members the assistance which electricity was beginning to give in connection with this very important and very widely extended industry. I believe, without statistics on the immense extent of this industry, the enormous number of mills all over the world treating ores in various ways, and, of course, the immense wealth that is involved, would be hardly credited. Maj.-Gen. Webber.

In answer to Mr. Cooper's question as to whether chlorine is essential to the process, chloride of sodium is in the first instance

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used for regulating the resistance of the electrolyte, and, in doing so, decomposition takes place and some chlorine is given off. My investigation was intended to suggest what that chlorine was incidentally likely to effect.

The President.

The PRESIDENT: The paper which General Webber has read deals with a very important subject, namely, the advantage of electricity in promoting the extraction of gold from its ores. We already know that in South Africa electricity is used for the deposition of gold that has been brought into solution by the action of cyanide of potassium. In that case, as General Webber has pointed out, you are dealing with a clear solution containing gold, and that is purely a case of electro-deposition. But the processes which General Webber has dealt with, and the one process which he has more particularly described in detail—the Pelatan-Clerici process—aim at doing something more than is done on such an extensive scale by the Siemens & Halske process in South Africa; they aim not only at depositing the metal, if they do aim at that at all——

General WEBBER: That is a question I am not quite sure about.

The PRESIDENT: At all events, the mass of mercury in the vat is the cathode in the electrolytic system. If gold is brought into solution, no doubt gold will be deposited by the action of the electric current. Another action, of course, is going on simultaneously, viz., the amalgamation of the gold that has not been brought into solution, but that has been brought into contact with the surface of the mercury, kept fresh and clean by the action of the electric current. The Pelatan-Clerici process appears to aim at producing an action which assists extraction by simple amalgamation, and also causes deposition of dissolved gold by electrolysis. It has, therefore, a wider scope than the process discussed on the last occasion, when Keith's process was before the Institution.

The subject of General Webber's paper is an exceedingly important one. The paper itself is very interesting, and has given rise to a most valuable discussion, well worth the time expended upon it.

General Webber is entitled to our thanks, and I am sure that it will be your wish to give expression to that feeling. I therefore propose that the thanks of the meeting be given to General Webber for his paper. The President.

Carried unanimously.

AN ELECTROLYTIC PROCESS FOR THE MANUFACTURE OF PARABOLIC REFLECTORS.

By SHERARD COWPER-COLES, Member, Assoc. M. Inst. C.E.

Glass mirrors at the present time are almost exclusively used for projectors for search lights and similar purposes, on account of the difficulty that has been experienced in producing a true metallic reflector that will not readily tarnish when exposed to the heat of an arc light. One advantage of a metallic reflector is that the rays from the carbon points are collected into a parallel beam by means of reflection only, and is not catadioptric, as most glass mirrors are. Spun reflectors are never true, as it is found in practice impossible to spin them quite true to the moulds. Experiments have been made with a view to substituting cast metal for glass, but the cost of grinding and polishing, and the unsatisfactory surface that is obtained, have resulted in the attempts being abandoned. Stamped reflectors have also been tried, but with no more satisfactory results. The process I propose to describe to you is an electrolytic one, one of the chief features being that the surface produced requires no after polishing or trueing up. When once a true mould has been produced, any number of reflectors can be taken from it at a small cost. A glass mould is prepared, the convex side of which is accurately shaped and polished to form a true parabolic, or other reflecting surface. As the mould only requires shaping and polishing on the convex side, it is comparatively cheap as compared to a glass reflector, which has to be ground on both sides. On the prepared surface is deposited a coating of metallic silver, which is thrown down chemically on the glass and then polished, so as to ensure the copper backing being adherent to the silver. The mould thus prepared is placed in a suitable ring Mr. Cowper-Coles.

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and frame (which I will describe later on), and immersed in an electrolyte of copper sulphate, the mould being rotated in a horizontal position, the number of revolutions being about 15 per minute. The copper adheres firmly to the silver, and together they form the reflector, which is subsequently separated from the glass mould by placing the whole in cold or lukewarm water, and then gradually raising the temperature of the water to 120° Fahr., when the metal reflector will leave the glass mould, due to the unequal expansion of the two. The concave surface of the reflector obtained is an exact reproduction of the surface of the mould, and has the same brilliant polish, and requires no further treatment to answer all the purposes of a reflector, with the exception that it must be coated with a film of some suitable metal to prevent tarnishing. Palladium is found to answer this purpose best, as a bright coating can be deposited rapidly to any desired thickness; the palladium resists tarnishing and the heat of the arc to a wonderful degree.

Palladium is a silver-white hard metal, and is sufficiently ductile to be rolled into thin sheets. Its specific gravity is 11·4, being about half that of platinum. The present price of palladium is about double that of platinum, but, its weight being only one-half, the same area can be covered at the same cost. It melts at an extremely high temperature—about the same as wrought iron. When only slightly heated in hydrogen gas, it has an extraordinary power of absorbing mechanically large volumes of this gas. Graham investigated this very curious phenomenon, and found that a piece of palladium foil when heated below 212° Fahr. takes 240 times its volume of hydrogen, but that it had not the power of absorbing oxygen or nitrogen. At a moderately high temperature palladium assumes a blue colour, due to the formation of a thin film of oxide, which it loses at a higher temperature, due to the decomposition of the oxide. Palladium is not readily attacked by sulphuric or hydrochloric acid.

In carrying out the manufacture of reflectors by this process, it is essential that the glass mould be perfectly clean and free from grease before the silver coating is applied. It has been found, however, that, if the cleaning is solely effected by chemical

means, there is a great liability of the silver adhering too firmly to the glass, whereby the mould is in danger of being broken during the removal of the reflector. This difficulty has been overcome by cleaning the glass mould with a suitable paste or powder such as peroxide of iron, then removing such paste or powder by washing the glass with a 50 per cent. solution of ammonia. It is necessary that this cleaning operation be repeated prior to the production of each reflector. After the convex side of the mould has been properly cleansed as described, a thin coating of metallic silver is applied as follows:—Ammonia is added to a solution of nitrate of silver until the precipitate that is first formed is re-dissolved, then re-precipitating by caustic soda, again dissolving in ammonia, then adding glucose to the solution. Excellent results have been obtained with a silvering solution made up of equal parts of solutions of the following strengths:—Silver nitrate, 0·5 per cent.; caustic potash, 0·5 per cent.; glucose, 0·25 per cent. The surface of the mould to be coated is immediately dipped into the solution face downwards. In from four to five minutes the silver begins to form on the glass mould, the solution changing from pink to dark brown and black; the film thickens quickly, and in from 30 to 35 minutes a good coating of silver is deposited. Dr. Common has found a good deposit of silver to be equal to a thickness of 1-28,000th of an inch. The silver coating is thoroughly washed, and then allowed to dry, and the silver which has been deposited is burnished bright with a piece of cotton-wool and peroxide of iron, preferably precipitated by ammonia from a dilute solution of ferrous sulphate. The cost of the silvering is found to vary from 2d. to 4d. per inch diameter. On the table is a film of the silver and copper stripped from a glass mould, which is quite transparent to transmitted light having a green tinge, but is capable of reflecting light.

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During the several operations that have been described, the glass mould (which in the case of large reflectors is of considerable weight) is handled by means of a sucker placed on the concave side of the mirror. The silver mould when silvered and burnished is placed in a ring, marked B in Figs. 1 and 3, which is attached

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to the frame, D; the ring serves to form an electrical connection with the silver coating. To determine the size of the reflector that is to be formed, and to ensure a clean edge, a ring, N, Figs. 2 and 5, is placed, having the proper internal diameter, and bearing at its inner edge against the mould as shown in the

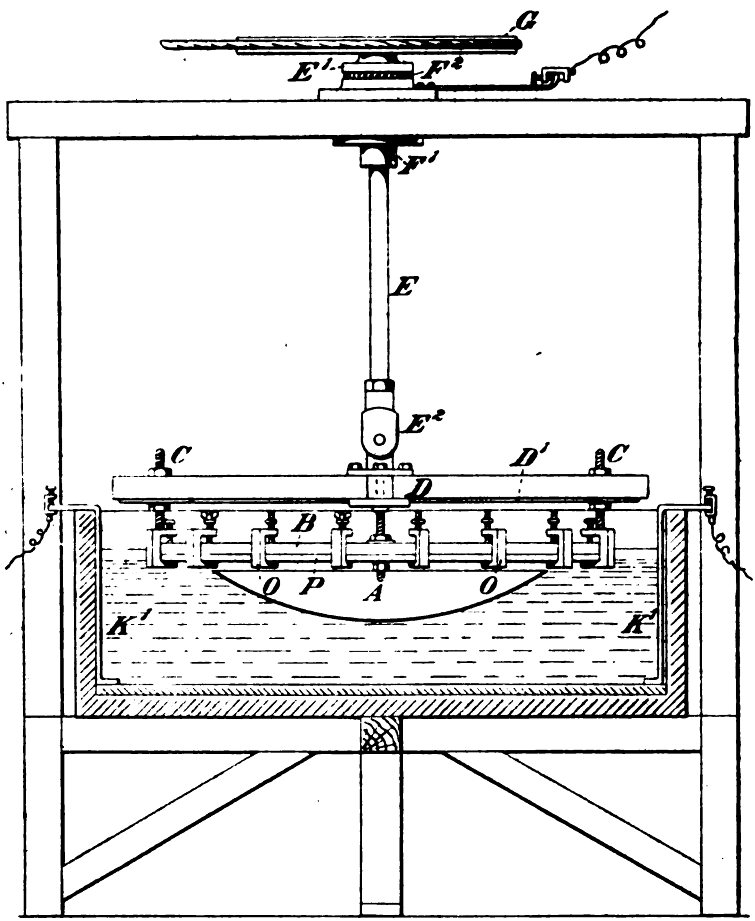

Fig. 1.

figures. Wooden blocks, N¹, of the required thickness are inserted between the rings B and N, and the ring is secured in place by clamps, O, as shown on Fig. 2. The ring N may be made of insulating material, or it may be a brass,

copper, or lead ring, having its lower face protected by suitable varnish to prevent the deposition of metal upon it. The ring B, Fig. 1, is suspended by bolts, C, and cross-bars, D, forming a frame which is connected to vertical shaft, E; the said shaft is carried on the main frame, E, of the apparatus by a bearing fitted with ball bearings marked F^2, which supports the shaft by means of a collar, and is allowed to rotate freely. G is a pulley through which shaft E and mould A may be rotated by a belt or cord. The depositing tank is carried by a frame, to which the mould is suspended, so as to be in contact with the electrolyte, which is a solution of copper sulphate; the anode is arranged at the bottom of the tank, and the current conveyed to it by means of copper strips. It is found advantageous to have the anode flat, as it reduces the tendency for the copper to "tree" at the edge of the mould; it also has the additional advantage of rendering the reflector thicker in the centre. Towards the end of this paper I propose to show you on the screen a cell containing copper sulphate and copper electrodes being electrolysed, from which you will see the importance of keeping the cathode in motion to remove the hydrogen, and also the importance of having the anode beneath the reflector instead of above, as small particles of metal are constantly detaching themselves from the anode, and would settle on the cathode, thus forming nodules, as pointed out by Mr. Swan in his Presidential Address, and as illustrated by some samples on the table. Mr. Cowper-Coles.

On the table is a section of a reflector which clearly shows the varying thickness of the copper deposited. The electrical connections between the negative terminal and the silver coating of the reflector is made through the ring B, bolts C, strips of metal, D^1, on the arms of the frame D, the shaft E, the ball bearing F^2. Fig. 6 is a perspective view of the cross frame for suspending the mould, and shows the metal strips for conveying the electric current. The connection of the frame to the shaft is made by a joint, E^2, that allows of the mould being tilted, for the reason that I will now describe to you. When first lowering the mould into the solution, it is advisable to avoid throwing the work of carrying the whole electric current on the

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silver alone, so the shaft E is raised by means of pulley blocks, or otherwise, to suspend the mould. The mould is then tilted and the shaft gradually lowered, bringing the edge of the mould in contact with the electrolyte, the circuit being thus established. A thin film of copper is deposited on the mould at the place of contact near the edge of the mould. The shaft is then lowered until it rests on the bearing; at the same time the mould is allowed gradually to assume its horizontal position. The operation I have just described occupies a very brief interval of time, and the current for a few minutes is worked at a pressure of about 9 volts, which is ultimately reduced. It is very important that the silver be flashed over with copper immediately on immersion in the copper sulphate solution. At this stage the ring N is not applied, and the mould simply rests on the ring B. The shaft is then rotated, and the operation of depositing the base metal continued with a

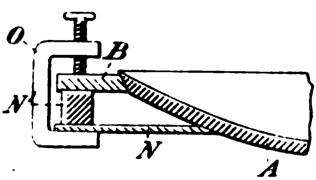


FIG. 2.

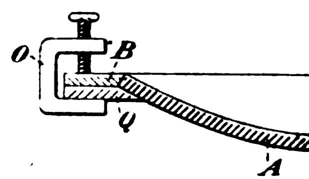


FIG. 3.

current-density of about 19 amperes per square foot, and is proceeded with until a sufficiently thick coating is obtained to act as a good conductor to the electric current. The copper solution generally used is of the following composition:—Copper sulphate, 13 per cent.; sulphuric acid, 3 per cent.; water, 83 per cent. The ring B, with the mould in it, is then lifted out of the bath, and the ring N applied to determine the size of the reflector that is to be formed; after which the mould is again placed in the bath, and the operation of depositing the backing proceeds until the required thickness is obtained. During this stage the copper is deposited on the mould up to the inner edge of the ring N, which thus determines the diameter of the reflector, and also ensures a clean. even edge to the reflector, which requires no further treatment. In place of the ring N, shown in Figs. 2 and 5, a leaden ring, P, Fig. 4, may be employed. The leaden ring is secured to

Mr. Cowper-Coles.

the ring B by the clamps O; this ring, being soft and pliable, will bend to the angle of the mould and the ring B, and therefore does not require to be blocked up as does the ring N. Fig. 3 shows a modification of a mould having its edges bevelled in the direction indicated—that is to say, in the reverse direction to that shown in Fig. 2. In such cases the mould is supported by a number of narrow rigid supports, Q, clamped to the ring by clamps O. After the thin coating of copper has been applied to the mould in the manner already described, the ring B, with the mould in it, is removed from the bath and turned over. The supports Q are then removed, and a ring such as P, Fig. 4, is applied to the mould to determine the size of the reflector. Or, instead of removing the supports Q and applying the ring P, if a ring such as N, Fig. 2, can be applied to the mould, it is then replaced in

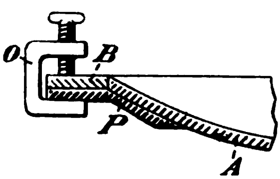


FIG. 4.

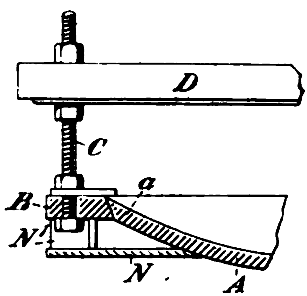


FIG. 5.

the bath and the depositing continued. As soon as the requisite thickness of metal has been deposited, the mould, with the reflector attached to it, is removed from the ring B and placed in a bath of cold or lukewarm water, which is then raised to a temperature of 120° Fahr.; whereupon, owing to the difference of the expansion of the glass mould and the metal backing, the latter separates from the mould. The only thing that requires to be done now is to coat the reflector with an untarnishable metal. This is accomplished by placing the reflector in an earthenware pan (Fig. 7) containing a 0·62 per cent. solution of palladium ammonium chloride in about a 1 per cent. solution ammonium chloride. The solution is used at about 75° Fahr., the current used for a 2-foot reflector being about 0·5 of an ampere, the E.M.F. at the

Mr. Cowper-Coles. terminals of the bath being 4 to 5 volts. An anode, S, made out of carbon, and curved approximately the shape of the reflector, is attached to a rod, marked T, which is connected by an arm to a rotating disc which causes the anode to swing to and fro, thereby ensuring an even coating of palladium, and agitating the solution and preventing the depositing upon the reflector of particles of foreign matter which may be present in the solution. 70 to 80 grains of palladium to the superficial foot is found to afford a good

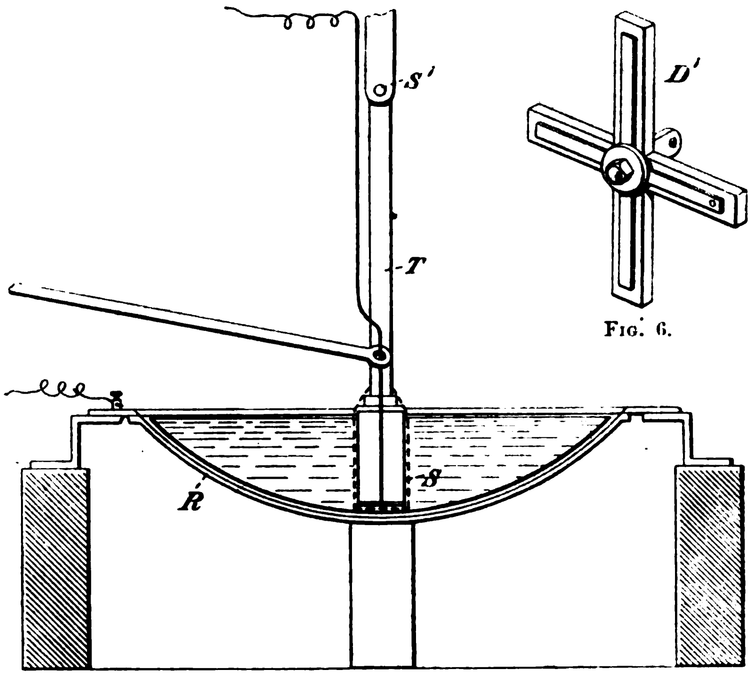


FIG. 6.

FIG 7

protective coating. The silver-faced reflector, previous to being placed in the palladium solution, is thoroughly washed with a weak solution of caustic soda.

The back of the reflector is usually varnished before placing it in the bath, to prevent local action setting up between the copper and the silver or palladium. The reflector is removed from the bath and dipped in boiling water, and then placed in boxwood sawdust, which is kept hot by means of a steam jacket.

The reflector is then ready to be mounted in a suitable ring, such as shown in Figs. 8, 9, 10, 11, and 12. The clamping ring shown Mr. Cowper-Coles.

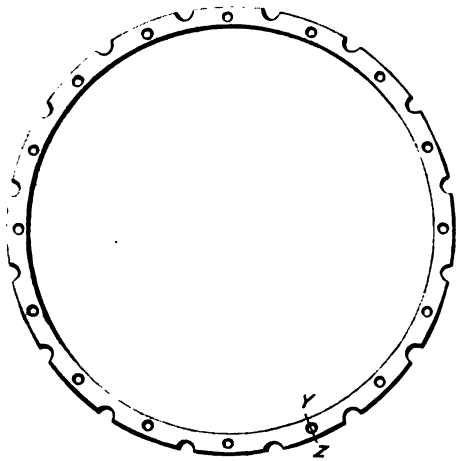


FIG. 8.

in Fig. 8 is provided with a knife-edge, marked F, Figs. 10 and 12. The knife-edge forces the reflector against a ring of asbestos,

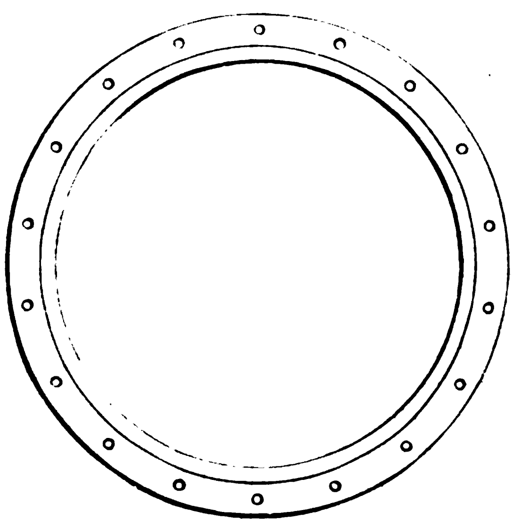


FIG 9.

marked G, and retains it in position after the reflector has been carefully centred whilst resting on the asbestos ring.

Mr. Cowper-Coles.

Reflectors made by the process which has just been described have been subjected to a number of tests, and found to withstand excessive heat without tarnishing. Salt water has been thrown on the reflectors when they have been too hot to touch, the result being that the water was driven off as steam, and the salt left as a white deposit on the reflector, which was easily removed with a wet cloth.

A reflector recently tested at Portsmouth had a number of rifle bullets passed through it, when the beam was found to be little affected. On the other hand, the first shot fired at a glass reflector splintered it to pieces.

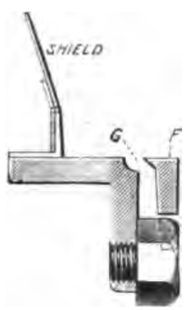


FIG. 10.

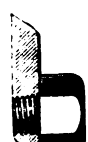


FIG. 11.

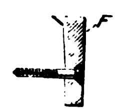


FIG. 12.

Although palladium does not reflect light as well as a silver surface which is perfectly clean and bright, silver is found quite unsuitable, as after being in close proximity to an arc light for a short time the silver tarnishes, and the light is greatly reduced in intensity. Palladium gives a very white reflection—as white, if not whiter, than that obtained from silver. With a palladium-faced reflector the intensity of light is found to remain practically constant, as little or no tarnishing takes place. Nickel has been found quite unsuitable.

Some of the reflectors have been tested optically by the process which has been extensively used by Tchikoliff. The method consists of photographing the image in a reflector of a white screen covered with black square network, as shown in Fig. 13, the lines being 0·2 inch thick and 0·6 inch apart. In the centre of the screen a square opening is left, through which the photograph

of the image of the network is taken through a pin-hole. The first steps in the process are as follows:—The reflector is placed at a distance of from 3 to 5 feet from the screen, which should exceed by at least 35 per cent. the height and breadth of the reflector, so as to ensure the image of the network covering the whole surface of the mirror. The test of the reflector is carried out as Mr. Cowper-Coles.

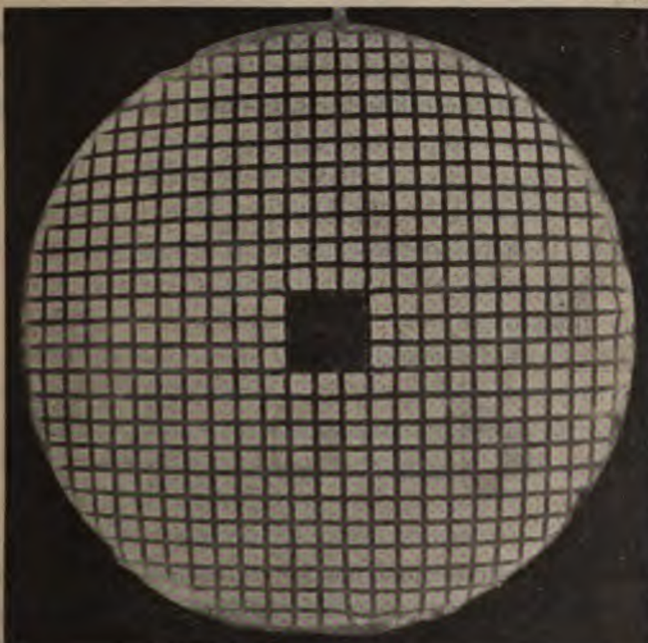


FIG. 13

follows:—The surface of the mirror in all planes is tried by a template, to ensure that the exterior curves of the surface are not far distant from the parabola; the image on the screen is then photographed, and the irregularity of the lines indicate any serious errors of the parabolic surface.

Mr. C. E. GROVE: I think Mr. Cowper-Coles is to be congratulated on the ingenuity which he has displayed in working out this process, which I venture to think promises to be of considerable practical value. The paper itself affords evidence Mr. Grove.

Mr. Grove. that the results have not been obtained without a good deal of trouble. I do not propose to criticise the process or the chemical particulars that are given—I am not able to do so; but I have had an opportunity of examining one of these reflectors, and it has struck me as being of value, if only for the reason that it can be handled without the excessive care that is necessary in handling the mirror of an ordinary projector. It seemed to me that the surface was not quite so bright as the surface of a silvered-glass reflector, but one has to remember that in ordinary work the silvered reflector of a projector becomes very soon covered with a kind of bluish-white soot from the carbons. I am not aware whether that deposit would adhere in an equal degree to these reflectors. If not, an advantage would be gained there. The idea has occurred to me that there is another application to which these reflectors might be applied with advantage. There is usually in front of the arc lamp a small opaque screen, which serves the purpose of preventing one's seeing the arc itself, and also helps to prevent the divergent rays from the back of the arc from interfering with the parallelism of the beam. I have tried —as I daresay many other people have tried before me—to see if a metallic reflector could not be substituted for that, which would have the effect of throwing upon the mirror the rays of light otherwise lost; but all experiments in that direction were utterly hopeless on account of the intense heat of the arc destroying the reflecting surface, volatilising silver and everything else one could possibly use. But if the palladium will so well withstand heat, it occurs to me that it might form a very useful secondary screen placed on the other side of the arc, and so help to increase the efficiency of a projector equipped on this method.

The PRESIDENT: Perhaps Mr. Hosack might say something?

Mr. Hosack. Mr. HOSACK: I am afraid, Sir, I can say nothing but endorse what Mr. Coles has read in his paper, as I have been intimately connected both with the experiments and manufacture of these reflectors.

Maj.-Gen. Webber. Major-General WEBBER: I have just now been objecting to be asked for costs; but when I first had to do with reflectors used for projectors for land defence purposes, I remember that

the great difficulty was not only the cost, but the monopoly of Messrs. Sauter Lemonnier in the manufacture of the reflectors for projectors. It is so long since I had anything to do with the matter that I have no doubt many changes have taken place, and therefore the conditions may be quite altered. I do not want to press the question as to details of costs—in fact, I do not think the object of the paper deals with costs—but I can well conceive that this field of invention may be of immense benefit in connection with purposes of defence and attack, if, as I imagine, the cost of projectors is perhaps two-thirds of what it was 10 years ago, and will be even further reduced by this invention. In such case Mr. Cowper-Coles has given great assistance to those who look forward to seeing the whole of our coasts illuminated in times of danger and of possible invasion. Maj.-Gen. Webber.

Mr. O. A. PILCHER: I should like to know whether these reflectors are much lighter than those made of glass. Those I have had to do with weighed about 56 lbs. I remember, in going through the Canal, the weight of the reflector was so unexpected that it was dropped overboard by those who were handling it. I think it would be a great improvement if these new reflectors were much lighter. Mr. Pilcher.

Mr. C. W. SPEIRS: Would it not be possible, after you have got a perfect reflector, to make a copper matrix, and use that matrix in future for making the mirrors, instead of using the glass again? The matrix, of course, should be silvered. I think it would thus be much more easily handled. Mr. Speirs.

Professor AYRTON: How would you deposit the silver?

Mr. SPEIRS: Chemically or electrically, in a silver bath. Instead of using a heavy glass matrix, such as Mr. Cowper-Coles now uses, to put in the bath, use a copper one. Glass is easily broken, but a copper matrix taken from a perfect reflector would be a perfect matrix.

Mr. MORDEY: I would like to protest, if I may, Sir, against General Webber's statement that questions of costs have nothing to do with this Institution. It seems to me that one of the most important things that we, as electrical engineers, have to do is to produce things not only in as simple a way as possible, but to produce them as cheaply as possible. Mr. Mordey.

Maj.-Gen. Webber.

Maj.-Gen. WEBBER: May I inform Mr. Mordey that that has nothing to do with the subject of the paper this evening?

Mr. Mordey.

Mr. MORDEY: Not directly, possibly, but indirectly I regard the paper as an object-lesson in economy of production. One often hears that questions of cost have nothing to do with scientific matters. I think that is quite a mistake. If true, so much the worse for science, for the highest and ultimate object of the study of Nature must be the benefit of man. And we, as engineers, have to direct the forces of Nature to the use and service of man. But if in doing so we spend so much of his money that man can get nothing to eat, it is not much good directing the forces of Nature to his advantage

The President.

The PRESIDENT: Before I ask Mr. Cowper-Coles to reply, I should like to make an addition to the queries which have been already put to him. A very important point has been brought out by Mr. Speirs. Mr. Speirs suggests that perhaps it might be better to use a light mould of copper instead of the heavy glass mould. But distortion has to be considered, and it is a very important consideration.

I think there would be great danger of distortion coming into play if a thin copper matrix were used instead of the thick glass matrix used by Mr. Cowper-Coles

There is often a tendency to distortion in electrotyping, and I confess I am surprised that Mr. Cowper-Coles has been so successful as he has been in making such beautifully shaped mirrors, so free from distortion as his test shows the mirrors to be.

This subject was mentioned to me some years ago by Major Bagnold. He knew I was working on the subject of electro-deposition of copper, and he submitted the question whether it was feasible to produce mirrors of the kind Mr. Cowper-Coles has produced. I confess that, at that time, the difficulties seemed to me to be very great, and I never ventured to combat them. I congratulate Mr. Cowper-Coles on having overcome the many and great difficulties of producing reflectors of such perfection as those he has shown to us to-night. I should like him to be explicit as to the proportion of acid in the solution; 3 per cent. Mr. Cowper-Coles said, and I suppose it is 3 per cent. by weight?

Mr. COWPER-COLES: Free acid.

The PRESIDENT: Three per cent. of strongest sulphuric acid, by weight? The President.

Mr. COWPER-COLES: Yes.

The PRESIDENT: It would have been interesting if Mr. Cowper-Coles could have told us exactly what was the difference between the amount of light reflected from the surface of palladium compared with a similarly polished surface of silver. Is Mr. Cowper-Coles able to supply that information?

Professor AYRTON: I do not think Mr. Speirs, owing to his modesty, has made his point quite clear. He says he uses the process that he describes every day, not for making mirrors, but for making perfectly smooth plates for printing purposes. He starts with a perfectly plain sheet of glass, not curved, which he silvers, and then deposits copper upon it. He removes that, and ever afterwards uses that sheet of copper as a matrix for all other deposits. As he has to print large things from that afterwards, it is a pretty good test of extreme perfection, and cannot, therefore, be much distorted. Prof. Ayrton.

The PRESIDENT: Is it a large surface?

Mr. SPEIRS: An ordinary block is 15 inches square. I make the sheets of copper 24 inches each way—that is, 4 square feet—and then cut them up as required. Mr. Speirs.

Mr. D. C. BERWICK: May I ask Mr. Speirs how he separates the plates? It seems to me it would be utterly impossible to separate two curved plates, such as were shown, without distorting one or the other, no matter how thick they were. Mr. Berwick.

Mr. SPEIRS: The surface of the plates are slightly silvered, chemically. If you get a perfectly clean sheet of highly polished copper, and give it a very slight silver deposit, the plate deposited thereon will separate from it easily, without any pressure or distortion. Mr. Speirs.

Mr. BERWICK: Where does the silver remain?

Mr. SPEIRS: It gives them both a stain—it is only a stain in the first instance.

Mr. BERWICK: That would probably utterly ruin the mirror for any reflecting purpose. It is quite out of the question to try Mr. Berwick.

Mr. Berwick. and polish one of these mirrors. The slightest touch of any polishing instrument distorts them.

Mr. Speirs. Mr. SPEIRS: The copper I speak of is not polished at all. Both sheets are like a looking-glass, and you can see the reflection of your own face in either of them.

Professor AYRTON: You do not polish them?

Mr. SPEIRS: Not at all.

Mr. Cowper-Coles. Mr. COWPER-COLES, in reply, said: With reference to Mr. Speirs's remarks about making a copper mould, I have tried a number of experiments with different metals, but could never get them to separate in a perfect form. It is due to the unequal expansion of the glass and copper that the reflector can be separated from the mould. If the difficulty of separation can be overcome, I see no reason why a metal mould should not be used.

Mr. SPEIRS: It might be much more difficult to separate two curved metals than to separate two flat sheets.

Mr. COWPER-COLES: I succeeded to a certain extent, but could not obtain the absolute true and accurate surfaces required. The surfaces obtained would not stand the optical test.

Mr. SPEIRS: My copper is probably not so perfect on the surface as yours, and I daresay it would not stand optical tests. I could not say, of course, because it has never been proved.

Mr. COWPER-COLES: With reference to Mr. Grove's remarks, there have been no accurate scientific measurements made as to the light reflected from palladium, as compared to silver, but a number of practical tests have been made by throwing a beam of light from a long distance on to a screen. As far as could be judged by a test of that sort, the light seemed to be as good as that reflected from a silvered glass reflector. The white soot mentioned by Mr. Grove is found to be very readily removed from the reflector, and does not affect it in any way. One of the speakers referred to the weight of the glass reflectors, and the probable advantage to be gained in this respect by substituting a metal reflector for a glass one. The comparative weight of a glass reflector to a metal one, without the ring, is about half the weight of a Parsons, and about one-sixth the weight of a Mangin reflector.

The President: I propose that we pass a vote of thanks to Mr. Cowper-Coles for his most admirable paper, and for the specimens he has shown us.

The motion was carried unanimously.

The President: I have to announce that the scrutineers report the following candidates to have been duly elected:—

Members:

Armistead Keith Baylor.
Archibald Potter Head
Herbert Louis Leach.

Associates:

Arthur Armitage.
Harold Lewis Coffin.
John Corneille.
Arthur H. F. Fitz-Herbert.
A. C. Hanson.
Edmund Rothwell.
Norman Arthur Thompson.

Students:

Thomas Philip Edward Butt.
Frederick Walker Purse.
Maurice Solomon.
Charles Henry Taylor.
Harold Henderson Williams
Walter Trevelyan Wright.

NOTICE.

The Institution's Library is open to members of all Scientific Bodies, and (on application to the Secretary) to the Public generally.

2. The Library is open (except from the 14th August to the 16th September) daily between the hours of 11.0 a.m. and 8.0 p.m., except on Thursdays, and on Saturdays, when it closes at 2.0 p.m.

// JOURNAL

OF THE

Institution of Electrical Engineers.

Founded 1871. *Incorporated* 1883.

VOL. XXVII. 1898. No. 133.

The Three Hundred and Eleventh Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, February 24th, 1898—Mr. JOSEPH W. SWAN, F.R.S., President, in the Chair.

The minutes of the Ordinary General Meeting held on February 9th were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council:—

From the class of Associates to that of Members—

Arthur William Ranken. | Maurice George Simpson.

From the class of Students to that of Associates—

Edgar Stopford Saunders. | William Steuart.
Frederic John Thompson.

Mr. A. B. Rayner and Mr. C. S. Whitehead were appointed scrutineers of the ballot for new members.

Donations to the Library were announced as having been received since the last meeting from Messrs. Alabaster, Gatehouse, & Co.; the Publishers of *Electricity;* and Messrs. E. & F. N.

Spon, Limited; it was also announced that a photogravure of the President had been presented by the Swan Electric Engraving Co. The thanks of the meeting were unanimously accorded to all the donors.

The PRESIDENT: I have now the pleasure to call upon Mr. Binswanger Byng to read his paper on "The Manufacture of "Lamps and other Apparatus for 200-Volt Circuits."

The following paper was then read:—

ON THE MANUFACTURE OF LAMPS AND OTHER APPARATUS FOR 200-VOLT CIRCUITS.

By G. BINSWANGER BYNG, Member.

Mr. Byng. In the progress of electrical industry manufacturers have become accustomed to sudden demands arising from a discovery or a successful experiment, and I purpose to deal with apparatus which manufacturers are called upon to make to meet the requirements of the latest innovation—*i.e.*, the distribution at a potential of 200 to 230 volts. Central station engineers have thus arranged their three-wire system, relying upon makers successfully to make such alterations in lamps and minor fittings as may be incidental to such a change. Their expectations have been fulfilled in a measure only. The ultimate success of the high-pressure system will depend largely upon the verdict of the consumer, and he will give it in its favour only if his fittings, in points of efficiency, economy, safety, convenience, and appearance, approach the standard which he can obtain by means of the lower voltage system.

It is, therefore, of importance that the central station engineer should assist the manufacturer to arrive at such perfection. His instructions, so far, have hardly gone beyond the demand to supply him with fittings to conform in appearance to the 100-volt system. It is true each central station has issued rules, but they are of little help to the manufacturer, and their very disconformity shows that there is neither unanimity nor correlation of ideas between the engineers in charge.

What is wanted is to have a thorough interchange of opinion

of engineers, contractors, and manufacturers. The latter would then know theoretically how far they may satisfactorily depart from the present practice, and thus save much time and money in adventitious experiments; and this also might tend to produce some degree of standardisation—much to be desired in the interest of all who have the success of the new system at heart. Mr. Byng.

With this object in view I bring this paper before you, and I think I can best serve the purpose by describing the chief appliances now upon the market, or under manufacture at my works, pointing out the existing deficiencies, and giving you my views upon the attainment, as far as possible, of higher perfection.

Incandescent Lamps.

Most important in connection with this subject is the incandescent lamp.

The lamp manufacturers have been compelled to supply 200-volt lamps at a given candle-power and efficiency, in bulbs of the same size as are used for 100-volt lamps. With flashed carbon the manufacturers meet with the great practical difficulty of properly disposing their long thin 200-volt filament in the same space as their shorter and thicker 100-volt filament, and therefore most of them solve this problem by resorting to a filament of much higher specific resistance than would be given by the flashing operation.

Unflashed Filaments.—Such a lamp is ready to the maker's hand by simply taking his ordinary carbon filament as it exists before being flashed—that is to say, before it is reduced by a fresh layer of carbon being deposited on the surface of the original filament.

The higher specific resistance of an unflashed carbon enables one easily to get over the difficulty of size of bulb, as such a filament will give the necessary resistance by taking a shorter length. Such filaments have also a greater emissivity, owing to the darker and rougher nature of their surface as compared with that of flashed filaments; consequently they require a less amount of surface per candle-power, and therefore the mass of an unflashed

Mr. Byng. filament, at a given candle-power and efficiency, is less than that of the flashed filament.

The filaments of high-voltage lamps largely used to-day would appear to be faster converters of energy per cubic mm. into heat and light than flashed filaments of the same candle-power and efficiency, although the watts supplied to each be the same.

On comparing the behaviour of such 200-volt lamps with that of 100-volt lamps, the roughest of tests shows that there is a far more rapid falling off of candle-power during life with the former than with the latter. At the same time the efficiency of the unflashed lamp decreases in a given number of hours by a far greater percentage than is the case with the flashed lamp. Mr. Robertson has made a series of life and efficiency tests on high-voltage lamps. They show that, in the average unflashed 200-volt 16-C.P. lamp, the percentage loss of candle-power in 600 hours is about 42 per cent., and the average drop of efficiency is about 35 per cent.

These two quantities seem to cover the chief practical merits desirable in an incandescent electric lamp—*i.e.*, the lamp which has the best percentage retention of original candle-power during its life, together with the best average percentage retention (or increase) of original efficiency during its life.

These tests show that these most desirable points, which have been worked on diligently for the last 13 years, have had to be thrown on one side in order to bring about the possibility of using the same sized bulb for a given candle-power at 200 volts as at 100 volts.

Tests of the behaviour of unflashed high-voltage lamps show that such lamps sometimes increase in candle-power during the first 100 hours or so. This also happens with badly carbonised or badly flashed 100-volt lamps (noted by Professor Ayrton in some of his recent lamp tests), owing to the initial lowering of their resistance in consequence of their not having been properly carbonised in the first instance; and this is often accompanied by a great alteration in the character of the surface (emissivity) of the filament. The carbonising or baking process is therefore still going on in the lamp, and the two above-mentioned changes, coming together, mask the fact that a great deterioration of the

filament has taken place; but a period is quickly reached when this fact is no longer masked. This period begins when there is no further decrease of resistance; but the surface deterioration still goes on, and thus soon brings about a large percentage fall of candle-power, and on the slightest increase of voltage there is now a tendency to increased resistance. These changes seem to be initially owing to the fact that the (unflashed) high specific resistance carbon is far more volatile than is the case with a good flashed carbon. Mr Byng.

A microscopical examination of a flashed and unflashed filament after each has been running 500 hours shows that the surface of the flashed filament is still quite smooth and shiny, whereas the surface of the unflashed filament has become very dull, sooty, and often full of small pit-holes. These pit-holes and soot form a large increase of surface, which therefore increases the emissivity of the filament, and consequently lessens its candle-power, as the watts supplied remain the same.

The property possessed by an unflashed filament of becoming so rapidly less efficient (increasing in watts per candle-power) acts as a preservative, because the increased emissivity lowers the temperature. This lowering of temperature decreases the tendency both to volatilise and also to produce a further great change of resistance. This power of self-preservation leads to such a filament giving some satisfaction to the general public, for the latter is satisfied when it obtains a good average, or sometimes an excessively long-life lamp; but this is very false economy, as it is only purchased by a very great falling off in actual efficiency.

Mr. Robertson's experience with carbon filaments seems to point to the fact that *it is impossible to obtain a carbon filament of high specific resistance without its being accompanied by at least the defect of greater volatility.* In other words, the lowest specific resistance carbon is the best, because it is less liable to evaporation, and therefore it gives the best retention of original candle-power and efficiency, and it is also mechanically stronger.

The specific resistance of many of the present types of 200-volt lamps is about 3,500 to 5,000 microhms per cubic centimetre, whereas it is easy to obtain flashed carbons whose specific

Mr. Byng. resistance is as low as 2,400 microhms per centimetre; a resistance as low even as 300 is possible, but not practicable.

Gases.—Another important consideration to bear in mind as to whether the high or low specific resistance carbon is the best, is that the high specific resistance filaments retain their occluded gases in a far more persistent degree than is the case with the low specific resistance flashed filaments.

It is probable that the occluded gases arising from the carbonisation of the filament are, by means of the flashing process, driven off to a large extent; and, in addition, the more dense and impervious nature of the flashed surface prevents the filament from absorbing the gases during its subsequent handling or treatment. This absorption is a property possessed by all carbon bodies in some proportion, varying with their density.

This greater power of unflashed carbon to absorb gases and to retain what it has absorbed, than is possessed by flashed carbon, leads in many instances to sudden deterioration of the vacuum in a finished lamp, accompanied by short-circuiting as soon as the pressure and the condition of the residual gases in the bulb have reached the point of maximum conductivity.

The consensus of opinion at the present day of the average types of high-voltage lamps undoubtedly points to the fact that a large percentage are expected to short-circuit as soon as they are put up, and I have heard several engineers say that they expect about one in twelve to go in this way.

From these causes, and others relating to the treatment of filament pointed out above, there seems to be no doubt that the average 200-volt lamps have a shorter life than 100-volt lamps. The above experiences have led Mr. Robertson to design all high-voltage lamps that are not restricted by size with well-flashed carbon filaments, and such lamps compare favourably with lower voltage lamps.

Horizontal Burning.—Another question which is very important in considering 200-volt lamps is that of horizontal burning, and contractors should take special notice of this. There is no doubt whatever that almost all the present-day 200-volt lamps are only suitable for burning in a vertical position. As

soon as any other position is adopted defects become prominent. Mr. Byng.
The long thin filament soon drops on to the bulb and cracks it. Also electrostatic attractions, owing to higher voltage, cannot be resisted by the long thin filament, and this is an additional cause of the filament approaching the bulb.

The effect of electrostatic attractions on long thin filaments is even noticeable with lamps burning in a vertical position. Such lamps have to be designed with the object of making their filaments more rigid, and to be thus able to withstand the effects of gravity and electrostatic attractions exerted by the charge on the bulb; and this is the chief point which makes high efficiency 200-volt lamps so difficult to produce. There is, therefore, a tendency, in trying to avoid the defects just mentioned, to make 200-volt lamps as low in efficiency as possible.

Leading-in Wires.—Another fault that exists with the bulk of the present forms of high-voltage lamps is, that, owing to the same size of bulb being retained, no greater separation can be given between the leading-in wires of the lamp. This is a special difficulty with high-voltage lamps which contain two filaments, as in this case the same size of cap is used, and four wires are passed through the sealing point instead of two, and they are therefore more crowded together. This question of distance apart of leading-in wires is a vital one, both in the manufacture of the lamp and in its after use. In the case of unflashed carbons this becomes a still greater defect, owing to small distance combined with probably greater gaseous emanation.

The higher the voltage, the sooner are these defects made manifest. Even with 100-volt lamps there is, under certain conditions, a tendency for current to jump across from pole to pole, owing to the remanent gases in the bulb attaining a high state of conductivity. The greatest conductivity of the remanent gases which lead to sudden short-circuiting appears to be when the pressure is about 0·01 mm. But, by reason of a continued discharge taking place in all lamps, there seems to be a tendency for the residual gaseous molecules to arrange themselves in a straight path between each pole. Through such a path discharge will take place even in a better vacuum than 0·01 mm.

Mr. Byng. This leakage current (sometimes called the "Edison effect") which leads to short-circuiting is very prominent during manufacture of high-voltage lamps, and to avoid it greater care is required as the voltage increases.

If the size of a bulb for a high-voltage lamp is to be restricted to the present dimensions, there is no doubt that the best lamp would still be that which has a single filament, were it not that other vital questions step in.

Electrostatic effects also increase with the voltage, and several patterns of lamps, most promising from all other points of view, have had to be put on one side on this account.

As to the best forms of cap for high-voltage lamps, preference will naturally be given to those in which the poles can be kept furthest apart.

If a B.C. or E.S. cap were on a larger scale, there is no doubt that considerable benefit would accrue. The simplest holder, having the fewest moving parts and adapted for always making the best contact, is undoubtedly the Edison screw, which, in the cases of excessive vibration, can be made with a locking device.

The slightest want of insulation in the cap between the poles eventually leads to a large leakage current between them or between the cap and one of the poles; and in many cases this is suddenly established to such a large degree as to result in the complete fusion of the lamp-cap, and sometimes of the holder. In such cases a non-metallic lamp-cap seems to offer great advantages, and has, in my experience, removed complaints on this score.

Standard Voltage.—From a lamp maker's point of view, a fixed standard of voltage and efficiency would only lead to an increased cost in manufacture, and the present practice of varying efficiencies, with voltages running in the case of low-voltage lamps from 95 to 120, and in the case of high-voltage lamps from 200 to 230, tends to keep the lamp at a lower cost than if these efficiencies or limits of voltage were more restricted.

On the other hand, voltages which lie outside these limits are a source of great expense to the manufacturer. It would therefore tend to cheapen lamps if a standard of voltage were

adopted which lay exclusively between the above (or even narrower) limits, but at varying efficiencies. Mr. Byng.

Combination Filaments.—In order to get over the difficulty of size of bulb, &c., there have been introduced many filaments (besides the unflashed pure carbon derived from cellulose in some form) which have a high specific resistance. This can only be obtained by using a less dense form of carbon than has hitherto been found most satisfactory in low-voltage lamps.

A form of high specific resistance filament has been tried in which the carbon has been admixed with various oxides, borates, and silicates of the earths. In addition to mixtures, electrolytic and chemical deposits of these bodies on the surface of carbon have also been tried; but, although it is a simple matter to obtain baked carbons containing these bodies, either incorporated with the carbon or on the surface thereof, it is quite another matter to obtain a finished lamp containing these bodies in a form to be of any practicable use. The difficulties met with are apparent as soon as the lamp is incandesced while undergoing exhaustion.

If such lamps be incandesced to a temperature exceeding that corresponding to about 5 watts per candle-power, there is a gradual separation of these bodies by evaporation from the carbon, and a resulting deposition on the surface of the lamp bulb.

The temperature of incandescence of the filament in order to obtain any advantage which might be derived from the "luminescence" of the rarer earths is apparently greater than that of 5 watts per candle-power; and since, as above stated, it has been found impossible so to evacuate a lamp as to leave any of the "luminescent" bodies incorporated with the filament at a temperature higher than that produced by 5 watts per candle-power, the object sought for is defeated.

From the above, it seems that, with our present knowledge, the best form of 200-volt lamp is that which has a well-flashed low specific resistance pure carbon filament in a large bulb, with a well-insulated moisture-proof cap allowing the poles to be placed at a reasonable distance apart. It should consist of a single filament, and be so disposed in the bulb that it can

Mr. Byng. withstand the disturbing effects of gravity and electrostatic charges on the bulb.

I wish to mention here that my co-director, Mr. Robertson, has given me great assistance in the remarks I have made upon lamps.

SWITCHES.

I come now to the matter of adapting switches, wall plugs, ceiling roses, lamp-holders, and minor fittings.

I do not apprehend any difficulty in changing existing types from the present standard of use to conform to the higher standard, yet maintaining the same appearance and size, and, when sufficiently in demand, approximately the same cost. The chief alteration will be in the increased break, and better insulation of the two poles. In smaller articles, such as combined switches and lamp-holders, the difficulty, if any, is more apparent. A discussion bearing upon the subjects involving the use and construction of double-pole switches, length of break, standardisation of terminals, position of fuses, the carrying capacity of contacts, &c., would be, to my mind, of great value.

Without enlarging upon the subject of switches to an undue extent, I will show some specimens of different types I find to be satisfactory in practice.

Here is a switch to take the place of the ordinary link or tumbler switch, and here is an ordinary double-break china switch. You will notice the formation of the china base, and the separation and action of the metallic parts, which are arranged to produce a long break and perfect insulation, so that an arc cannot be maintained if established, nor can a shock be communicated to the operator. I have also placed on the wall enlarged drawings of wall sockets and ceiling roses, to illustrate my further remarks under this heading.

It is not necessary to go deeply into the subject of fixtures such as electroliers, pendants, &c., but in connection therewith I wish to refer to the question whether it is advisable to recommend the use of two or more low-voltage lamps in series on a 200-volt circuit. Within my own experience, I know of several installations

Mr. Byng.

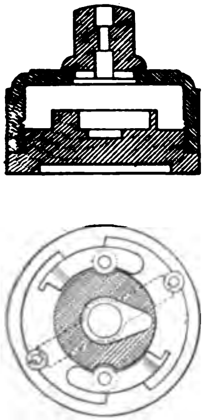


Fig. 1.—H.V. Double-Break Switch.

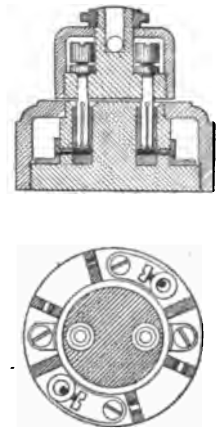


Fig. 2.—H.V. Wall Plug.

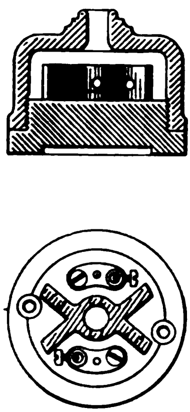
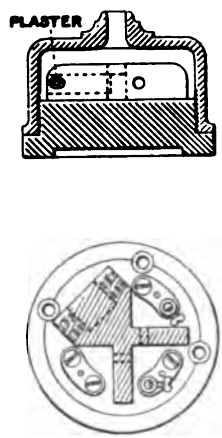


Fig. 3—H.V. Ceiling Rose



[Fig 4.—H.V. Ceiling Rose,
Lined Cut-Out.

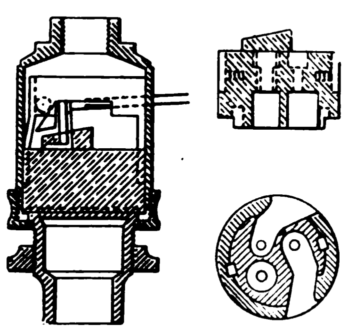


Fig. 5.—H.V. Key Socket.

Mr. Byng. fitted originally with 200-volt lamps that have, by reason of greater expense for current, and an inferior light, been re-wired for two 100 lamps in series, with satisfactory results. It is within the province of manufacturers materially to assist wiremen by designing fittings specially adapted to series wiring, such as series holders, ball fittings, brackets, or electroliers with arms in multiples of two; and such a practice might be extended with advantage to many other details.

Fuses.

The question of fuses for higher voltage requires more careful investigation, and would repay thorough discussion. Central station engineers agree to differ upon the various points of

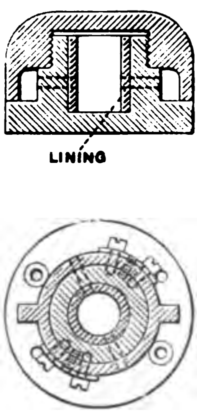


Fig. 6.—H.V. China Cut-Out, Lined Chamber.

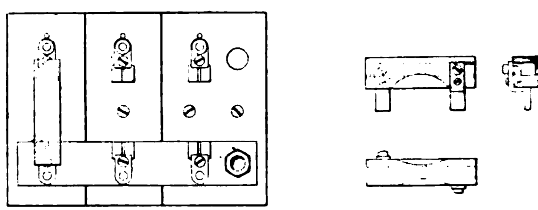


Fig. 7.—H.V. Cut-Out Board; Spring Clips for Hollow China Fuse-Holders.

efficiency, as evidenced by the different rules issued for our guidance. Some lay stress upon increasing the length of fuse wires, others insist upon ventilation holes, and in some cases the height of covers is to be increased. But there is no unanimity between them, and none of these rules, in my opinion, indicate the right direction.

I have made some extensive experiments, and believe that the results are of interest to the profession generally.

When a circuit is opened by the disruption of a fuse, the

combined metallic vapours and hot air produced by the high temperature of the resulting arc may extend and maintain it so as to bridge over the terminals, which, melting and becoming volatilised, feed the arc, and rapidly increase the temperature. I find, in practice, that under these conditions the china base supporting the fuse and terminals is easily volatilised also, and not only contributes towards the maintenance of the arc, but is ruptured as if by explosion, tending to set fire to any inflammable surroundings. This rupture has hitherto, I believe, erroneously been attributed to the expansion of air confined by the cover; hence the ventilation holes, which, according to my opinion, are Mr. Byng.

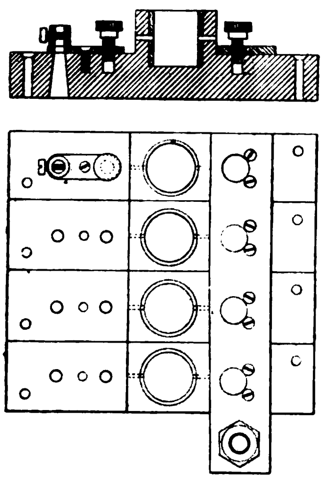


FIG. 8.—H.V. Cut-Out Board with Lined Chambers.

useless. The same experiments tend to prove the fallacy of using a long fuse wire, with the concomitant disadvantages of a greater demand for space and difficulty in renewal; and I feel sure that full efficiency may be attained with a short fuse.

My deductions from the aforesaid experiments are that—

(1) It is essential to arrange a fuse wire so that it will break at a definite part of its length, *i.e.*, approximately the centre.

(2) The arc formed on breaking the fuse must be so confined that it cannot be maintained so as to damage the terminals, base, or cover.

Mr. Byng. I will now describe how I have observed these essential points in making china cut-out boxes, which are the general type of fusible cut-outs used in house installations in this country.

These fuses are arranged on china bases, or in groups upon cut-out boards, and their carrying capacity varies from 1 to 100 amperes. In all these I provide what I call a "fuse chamber"—*i.e.*, a round china wall forming a central hole, from $\frac{1}{2}$ inch to 1 inch in diameter—and this is pierced with two holes near to the terminals, which are fixed upon the base outside the wall. The wire is threaded through the holes from one terminal to the other, passing through the fuse chamber. Both ends of the fuse wire are supported by a material which is a better heat conductor than air, whilst it is free in the central fuse chamber. Thus the same current raises the temperature of the fuse wire within the fuse chamber more rapidly than at the supported ends; therefore the disruption takes place there, and the resulting arc is enclosed. I find, however, that china as an isolator is not sufficient, because of its tendency to volatilise at high temperatures—a fact I have already mentioned. Therefore I line the interior of the chamber with another material which is a better heat conductor and less liable to fracture, and I find that ordinary plaster of Paris is most convenient for this purpose, although several other substances may be equally efficient.

Fuses constructed on this principle require but a short length of wire, and are perfectly safe on high voltages, and the general appearance and size does not differ to any great extent from those now in common use. The principle once established of surrounding part of the length of fuse wire with a substance that is a better conductor of heat than air, we can easily construct any other type of fuse upon the same principle.

Fuse wires enclosed in glass tubes filled with plaster or cement, but with a free central space for fusion, for instance, fulfil the same conditions.

I find that such fuses have been patented by Mr. Mordey in 1890. The mere fact of enclosing a fusible wire in a non-conducting refractory filling would not fulfil the essential functions of a perfect fuse as I have described, without a clear

space at or about the centre for disruption. Mr. Mordey describes such a space in his specification, but it is for the purpose of observation only, and it is not clear from the tenor of the specification that the inventor had this purpose of the localisation of fusion in mind. Mr. Miller, of Kensington, has also altered the ordinary Edison fuse by substituting thin copper wire, and partly filling the centre with asbestos fibre. So far as localising the point of disruption and confining the arc, this fuse acts very well, although copper or alloyed fuses are not perfect under any circumstances, by reason of the maintenance of a dull red heat under normal load. Mr. Byng.

Arc Lamps.

Coming now to the subject of arc lamps in relation to the higher pressure, I do not see that with alternating currents the increased potential materially affects the consumer. The practice may bring a single-parallel system into vogue, with economy coils—an alteration which will, in my belief, increase the commercial efficiency of alternating arcs. But the disadvantages are apparent in the case of a continuous-current circuit. For instance, in small installations of one or two lamps on the 100-volt circuit, these must be doubled on the 200-volt circuit, or useless resistances interposed. To equalise the conditions it has been proposed to substitute low-current lamps—say four 5-ampere instead of two 10-ampere lamps.

As this substitute has been advocated by one of the foremost central station engineers, and many others will possibly follow him, it may be useful and not out of place to prove that, both in theory and practice, low-current arc lamps are deficient in points of economy and efficiency.

It is generally accepted that the current-density of an arc is independent of its size; assuming this as correct, the area of an arc must be proportional to the current, and the cooling surface proportional to its diameter. This is the case with the cooling surface of the carbons, if these are used of areas proportional to the current taken by the lamp. Thus the cooling effect of the atmosphere will have a direct relation to the diameter of the

Mr Byng. arc and to the square root of the current. Therefore, for illumination, large arcs are more efficient than small ones, and the practical arc is attained when the benefit of increasing the number of light centres balances the inefficiency of small arcs. This gives, in practice, an arc of about 10 amperes.

Small arcs worked under similar conditions are more unsteady than large ones, which is due to the fact that with an arc of a given length the E.M.F. decreases as the current increases. An experiment in which a fixed length of arc at 5 amperes and 44 volts was suddenly increased from 5 amperes showed—

Normal	5 amperes	44 volts.
Sudden increase from	5 to 6 amperes ...	40·5 „
„ „ „	5 „ 7 „ ...	38·5 „
„ „ „	5 „ 8 „ ...	37·2 „
„ „ „	5 „ 9 „ ..	36 „
„ „ „	5 „ 10 „	35·2 „

So an arc, or series of arcs, with a total voltage approximating to the E.M.F. of the circuit, is unstable and fluctuating—probably because of the disproportionate variations of the cooling surface, coupled with the decreased resistance of carbon at higher temperatures. If the arc flares, the current will increase, unless there be sufficient resistance in series to reduce the voltage across the arc at a greater rate than the above figures show. Certainly the mechanism tends to lengthen the arc, but to no advantage, because the movement continues until the current decreases to the normal value, and the acceleration would extinguish the arc unless an interposed resistance allowed of a rapid increase of the voltage across the arc. Such a resistance is necessary to compensate the "negative resistance" of the arc, which may be more appropriately termed "decreased cooling surface per "ampere." An additional resistance in series is necessary to ensure steadiness. It follows, therefore, that 5-ampere lamps must be worked on a higher E.M.F. or "pumping" will ensue.

The possible current through four lamps on 200 volts, allowing

40 volts across each arc, is 5 times the normal; with five lamps on 230 volts, 7·6 times the normal; while with four lamps on 230 volts it is only 3·3 times the normal. Therefore, when 5-ampere lamps are used upon a 230-volt circuit, it is better to run four with steady long arcs than five with unsteady short arcs. Mr. Byng

It is probable that the enclosed arc lamp will be brought into prominence in this direction, offering certainly many advantages; but I would point out that in practice the current cannot be largely increased, because of the frangibility of the enclosing envelope under an accession of temperature. If the cooling surface be increased so that the temperature of the gases surrounding the arc remain about the same, the efficiency of the lamp is considerably reduced.

Adverting to the manufacture and installing of arc lamps to meet the contingencies of the high voltage, we have to consider that, if the carbons run short or the slides stick in one lamp, the other lamps close together, and the full voltage of the circuit is maintained across the shunt coil of that particular lamp. The possible troubles are that the shunt coil is burnt up, and that the carbon-holders are damaged with the excessive flaring of the arc before it breaks.

It would hardly be practical to make magnets to stand such overload. Of course we can instal a cut-out and equivalent resistance to each lamp. But this expedient is very costly, and presents the further difficulties of finding suitable room near the lamp or making it self-contained with the lamp, and of teaching the consumer that the full current can be used although the lamps are not alight.

Some sort of cut-outs must be installed, and I am of opinion that there is a field for inventors in this direction. I will indicate how I have endeavoured to meet these difficulties.

If, as is generally the case, one pair of carbons burns at a greater rate than the others, the slide in that lamp will touch the stop first, and the stumps will burn away until the gap is wide enough to break the arc. I append a table showing the results of five trials.

Mr. Byng.

FOUR 10-AMPERE LAMPS ON 200 VOLT CIRCUIT.

+ *carbon*, 18 *mm. cored*; – *carbon*, 11 *mm. solid.*

(1)	Arc flared and was extinguished at ...	$1\frac{1}{8}''$	gap.
(2)	,, ,, ,, ...	$1\frac{3}{16}''$	,,
(3)	,, ,, ,, ...	$1''$	,,
(4)	,, ,, ,, ..	$1\frac{1}{8}''$	,,
(5)	Current switched off when arc was at	$2\frac{1}{8}''$	,,

In the first four trials the arc broke while flaring; that is to say, it travelled up the side of the carbon and ignited the loose dust, taking a spiral course, this course being continued until the length was too great for the voltage. The arc only leaves the point of the carbon when there is sufficient dust to maintain it, and to counteract its increasing length; thus, when the supply of dust fails, the arc is extinguished before it can return to the points.

During Trial No. 5 the carbon became pointed, and the temperature rose to the extent of freeing the surface from dust; hence the arc did not leave the crater.

We may deduce from this the desirability of maintaining a considerable gap, exceeding even 3 inches, between the carbon-holders. For absolute safety it is better to extinguish the arc automatically. Now an automatic switch is useless, because it is necessarily controlled by the potential across the lamp, and could not discriminate between the increased voltage caused by the carbons burning short and that caused by the extinction of a flare; and, since this may happen at any time, the arc could not be re-formed even when the carbons came together. The circuit, in fact, would be inert, and the arc would have to be re-established by hand.

An efficient cut-out must extinguish the arc, and simultaneously cut the shunt coil out of the circuit, the mechanism of the lamp also being free that the carbons may travel together. The shunt must on no account be cut out whilst the brake is on, since it could not then compensate the series coil and draw the carbons together.

For the protection of the shunt coil I have used a temperature

fuse, made of an alloy with a melting point of 210° Fahr., and having sufficient sectional area to be independent of the amount of current likely to traverse it. The carbons could be held apart several minutes before fusion took place. Although a decided advantage was gained over a plain lead fuse, and the shunt coil was efficiently protected, the carbon-holders were not protected, and the fuse required renewing each time it became ruptured. Mr. Byng

The chief difficulty in constructing an automatic "cut-in" and "cut-out" lies in the necessity for a rapid make or break, to save vibration and sparking. My system may be briefly explained, in that the arc is first short-circuited through a shunt path, and so put out by reducing the voltage across the terminals. This short is then broken by a quick break switch, the same action reversing the shunt switch simultaneously, ready to fall upon its normal contact when the carbons touch, or are replenished. The mechanism is actuated by the main armature of the lamp, and the movements take place while the armature is below the feeding point, so as not to interfere with the working of the lamp.

You can see the actual working of this novel "cut-out" on the lamp which I show here.

Heating and Motors.

The effect of the increased pressure upon such applications of the house current as heating, cooking, &c., does not entail a sufficient alteration structurally or electrically to need an exhaustive description.

The resistances forming or causing the heating surfaces must be arranged to conform to the higher E.M.F. at the terminals, and it is mostly preferable to increase the length rather than decrease the diameter of the resistance wires; but this fact presents some difficulty in such articles where the space available is small. If the space is too limited, such apparatus can only be used in series, or in connection with an external resistance.

With motors, the greatest difficulty also lies in adapting the smaller sizes, say from one-twelfth H.P. to one-sixth H.P., to suit the altered conditions of higher pressure. A certain structural alteration is doubtless necessary to arrange a new winding to

Mr. Byng. produce the same efficiency as heretofore on a 100-volt circuit. In the larger sizes, I am, in order to facilitate keeping stock, using a double or differential winding, which, when coupled in parallel, conforms to 100 volts pressure, and with the same winding in series gives an equal efficiency on a normal load at the 200-volt pressure.

In reviewing the subject of higher voltage generally from a standpoint of *cost*, I am of opinion that sufficient time and experience, naturally resulting from an increased demand, will place the cost of most fittings for 200 volts within the margin of those of the lower voltage—except, perhaps, a few cases, among which I may instance incandescent lamps. These will necessarily always be more expensive, owing to increased cost in mounting of larger bulbs and extra supports, and also through increased time of exhaustion and percentage breakage.

But we must not overlook the fact that in the matter of wiring there must be a decided saving. The smaller sectional area of conductor per lamp employed, without the necessity of increased insulation, as also in a minor degree smaller connectors and contacts, will in all probability compensate some other apparent disadvantages, and may bring the balance of cost in favour of the high-voltage system

I do not wish to bring the subject of cables and wires or wiring systems within the scope of my present paper; but I will only mention that, in my opinion, such matters as the establishment of revised wiring tables, the use of twin wires, the smallest gauge allowable for single lamps, the best and cheapest system of wiring for high-voltage supply, would be subjects well worthy of the immediate consideration of, and an interchange of opinion between, engineers and manufacturers.

The President. The PRESIDENT: I have received several intimations from gentlemen who are not present at all our meetings—gentlemen who have come from a distance—of their desire to take part in the discussion on Mr. Byng's very interesting paper, and I think it would be only treating our more distant members fairly if I gave them precedence in the discussion. I just make one

exception to that rule by asking Mr. Crompton if he would be kind enough to open the discussion on this paper. The President.

Mr. R. E. CROMPTON: Well, Sir, while I am fully aware of the great compliment you pay me by asking me to open the discussion, I think, with your permission, I much prefer to speak later, for the reason that you have already given, and for a second reason—that I wish to confine my remarks to the arc lamp question, and I am not quite ready with them. Mr. Crompton.

Mr. H. L. P. BOOT: I have only a few remarks to make about this paper, as I think we are all agreed with the author on most points therein mentioned. The author deserves our hearty thanks for bringing such an important subject before the notice of central station engineers and wiring contractors in general. There is certainly a tendency in the present day rather to ignore these minor matters, although they help, to a large degree, to form the success or the failure of an undertaking. I am also pleased to note that manufacturers—or, at least, one manufacturer—is waking up to the fact that it is necessary to design standard fittings, and to keep to those standards for wiring work, as it is only by this means we can cheapen and reduce the expense. The one point which is certainly noteworthy with regard to the advantages of higher pressure is undoubtedly the saving effected in cable, due to the smaller sectional area required. I have had several instances brought before my notice lately with respect to this—that is to say, where firms, in tendering for certain work, have made a mistake, and sent in their tender for 100-volt pressure when the work was to be carried out at 220 volts or 200 volts. They have then reduced their tender, and I have been rather surprised to find that the saving in the cable—especially, of course, where large buildings are being wired – would more than counterbalance any extra expense which a consumer might be put to in installing first-class fittings, or fittings suitable for a higher pressure supply. I note the author, in his remarks, draws attention to the peculiar fact that we have no such thing as standard length of fuses for a given voltage. Of course each central station engineer has his own idea with respect to this; but I may mention—although, no Mr. Boot.

Mr. Boot. doubt, it is probably well known—that experience seems to point out that where a fuse is used on alternating circuits there is less necessity for taking precautions to prevent "porcelain-smashing," or "explosions," and things of that nature which occur, than there would be on direct-current circuits at the same pressure. I have proved this on several occasions, having taken fuses—practically the same fuses—and submitted them to 200 volts alternating and 200 volts direct, and I have found that invariably the greater damage is caused by the direct current. From the author's paper, he rather leads us to infer that high-voltage lamps are decidedly bad. Now it would be a pity, especially as most of the stations are changing over to the higher voltage, that this should get about—that it is impossible to make lamps certainly not better than those described by the author. My experience has been rather the reverse of that. I admit their life is not so long; I also admit that their candle-power falls off more rapidly; but there are many points in the author's paper which I am not prepared to admit with reference to high-voltage lamps. Anyhow, he makes out a very good case for engineers using the "Robertson "lamp." I must admit that point, because evidently Mr. Robertson has found more difficulties in a high-voltage lamp than central station engineers have in its use. There is one important point which might often be observed in practice, and that is the series lighting, or, rather, placing two 100-volt lamps in series. Take, for instance, houses where it is a very usual thing to see a two-light bracket placed on one switch: it would be very easy to wire those in series, and the consumer would also have the advantage that he can now buy 100-volt lamps absurdly cheaply from other central stations, which have discarded them, and which have changed over. In the town for which I am responsible—Tunbridge Wells—I may mention that in the side streets, and on our arc-lamp columns, we have fixed two glow lamps on each column for lighting the streets when the arcs are turned out. Also for side-street lighting we have several incandescents; and it has been our practice, as we have changed over to the 220-volt supply, instead of using the 200- or 220-volt lamps on our street lighting work, to place two 110-volt lamps in series. This has effected an

important saving, inasmuch as our street lighting—or, rather, the renewal of lamps—has practically cost us nothing, because we have taken the consumers' lamps and used them for that purpose. Mr. Booth.

Mr. W. M. MORDEY: I have been watching with considerable interest the growth of the 200-volt movement, and I think that the author has done us a great service by bringing up this subject at this time. It is a subject that can be most usefully discussed now. I wish to compliment him on his paper. It strikes me as an excellent practical paper. Might I be allowed, Sir (although it is, perhaps, going a little beyond the immediate subject of the paper), to make a few remarks on one of the points mentioned—a matter, perhaps, rather of theory than of practice? I refer to the question of the specific resistance of carbon. The author mentions it in several places in the paper, and I think it is a matter of some interest. I would ask whether the specific resistance of carbon ought not to be considered in relation to its specific gravity. I think it has been the custom to look upon it quite apart from any question of specific gravity. I speak, of course, quite as an amateur in these things; and in your presence in particular, Sir, I would say any remarks I have to make are with an idea of bringing out, rather than with any idea of imparting any, information. But I think, if we look at what carbon is, we shall find that nearly always the differences in specific resistance have to do with the actual state—not the physical state, but the mechanical state—of the material. The author has done well in pointing out that, in order to get the necessary high specific resistance, unflashed filaments have to be used. Anybody who will consider an unflashed filament will see that it is far from being a solid—all the volatile constituents of the original carbonaceous material have been driven off, leaving the carbon in a porous condition—it is simply a long string of cinder full of holes; it is a series of point contacts, or what we used to call "microphone contacts." When the filament is flashed it becomes more or less a solid; but even then, regarded as a solid, it is in a very different state from a metal. On this subject I may refer to some remarks I made on the first occasion Mr. Mordey.

Mr. Mordey. I had the privilege of addressing this Institution—I think it was in 1882 or 1883. I described some experiments showing that there was perhaps some misconception in the idea that carbon differed from metal in the effect of temperature on its resistance. Finely divided carbon, I pointed out then, behaved exactly the same as finely divided metal, as far as the effect on its resistance of changes of temperature. It was a matter really of point contact. I just mention this matter in the hope that some of the lamp makers may tell us to what extent they find that specific resistance and specific gravity vary together. It would be interesting to find from some of the makers of lamps whether the lesson of the gas retort has ever been considered in connection with filaments. In gas-making the carbon which is deposited in the retort is very often found in the form of pendulous filaments on the top of the gas retort—a particularly hard and solid form, and probably a fairly pure form, of carbon. I do not remember that practical use has ever been made of this, except that some years ago some maker—I forget which; perhaps it was yourself, Sir—used those little tiny filaments for the purpose of "fairy lamps"—the tiny lamps worn by people in scarf-pins, and by people on the stage. I have often wondered whether that process could be carried further, and whether filaments could not be produced in that way. The author, in the first paragraph of his paper, makes a very important admission. He says that the ultimate success of the high-pressure system will depend largely upon the verdict of the consumer. I think, perhaps, Sir, that view has rather been lost sight of by some of those who, on account of the very great advantages in transmission, have been advocating the use of high-tension lamps. I think these advocates have sometimes overlooked that important point alluded to by the author—that this question was really being tried by a jury; that the verdict of this jury of consumers is what must ultimately settle the matter. I would ask the speakers to give us—I think it would be very useful information—actual facts as to the relative cost of lamps and of light by 200-volt and by 100-volt lamps. I suppose sufficient time has now elapsed to enable useful results to be given. Central station engineers

and consulting engineers must not imagine that the jury upon whose verdict the ultimate fate of this matter depends has been asleep. It has not been asleep. I have heard on various occasions remarks of an entirely unsatisfactory character with regard to the use of 200-volt lamps from the jury's point of view—that is, from the point of view of the consumer. Of course their view, important as it is, is only one aspect of the question, and it may very well be—I can quite imagine it is—the case that other advantages in the system counterbalance any increased difficulties or cost arising from the use of 200-volt lamps. But if that is the case, then I think the matter should be considered more fully than it has been hitherto. The question of the supply of light should be considered as a whole. It should be considered whether it would not be better for supply organisations to take on their own shoulders the whole burden, supplying customers with their lamps, so that the customer can have no ground of complaint, except that he may have a little more trouble with regard to renewals. It is not right that the supply station should have all the advantages and consumers all the drawbacks. And there is the advantage in supplying lamps that central station engineers would then have some hold over the amount of energy they have to send out for any given candle-power. They must not—and I am sure they do not—forget that what they have to sell is really light, and nothing else—that is, so far as the lamps are concerned. Might I be allowed, Sir, to refer, as a matter of great interest, to a paper that was contributed by Mr. Robertson to one of the technical journals—I think the *Electrical Review?* I wish we had it here; it was a most interesting paper—on the difference in the effect of alternating and direct currents on the life of lamps. Mr. Robertson told me all about it, and afterwards I saw his paper. He, in Vienna, had opportunities, in connection with a large supply organisation, to find out, by the most exhaustive tests, the relative life of lamps on a large scale—on a scale of 12 inches to the foot, with thousands of lamps on each system. He found that in the two systems in use in Vienna—one an alternating system, and the other a direct-current system—there was a very decided gain in the life of the Mr. Mordey.

Mr. Mordey. lamps when alternating currents were used. I am not speaking as an alternating-current advocate—I hope you do not think that—but it is a question that is very well worth looking into; and it was particularly interesting as Mr. Robertson put it to me, because the commercial foundation of the organisation in question was the supply of lamps to customers at a certain contract price per annum, and it was very important to know how long the lamps lasted on each of the two systems.

The author has been good enough to refer to some fuses that I brought out some years ago. I am very pleased to find that the author, who has worked quite independently on the subject, has arrived at pretty much the same conclusion that I did. I am very pleased that he has. I am also pleased to say that, as soon as he found I had a patent, he did not suppose that I had taken out that patent entirely for the purpose of paying the patent office and the patent agent's fees. I would like—but I will not prolong my remarks to do so—to say something about arc lamps; but I will just make one remark, if I may be allowed, as to the power of the lamps. I have not studied this section of the author's paper minutely, but I would say that my experience is that the getting out of the light from the arc is the important thing—that the light is roughly proportional to the watts rather than to the current—simply because with high voltage you get a longer arc, and the light gets out instead of being kept in and shadowed by the lower carbon. I think that is the explanation of the success of the small-current lamps and high voltage. I quite agree with the author that small-current lamps with low voltage are very poor things; but I think if high voltage is used—and that is the case, I believe, with the enclosed lamps that are so successful now—the explanation is that the light is able to get out because the voltage is so high, and the carbons are so far apart that the light, even from the small crater due to the small current, can get out. With regard to a passage on page 133 of the paper referring to the arc lamp cut-outs, I think the author has forgotten the old Brush lamp, which has done pretty good service in its time. I believe the Brush cut-out quite complies with all the conditions the author has laid down as essential to a

cut-out, cutting in and cutting out and lighting automatically, and so on. Mr. Mordey.

The PRESIDENT: Perhaps Mr. Robertson would like to speak now?

Mr. ROBERTSON: If such a thing is allowable, I would prefer to keep my remarks until a little later on, inasmuch as I have practically embodied all I wish to say in the assistance which I have given in this paper. Mr. Robertson.

Mr. J. S. RAWORTH: I desire to thank Mr. Byng for his excellent paper. I think it came just at the right time, except it had come sooner. I cannot but feel, Sir, that Mr. Boot and his friends, who spoke with so much delight of the rapidity with which this 200-volt system is spreading, would not be quite so happy if they had to make the lamps or had to live with them. I do not make them, but I live with them, and so far I am enjoying it very much, because several lamp makers are keeping me supplied with lamps entirely free of charge, in order that I may form an opinion as to their various merits. The only effect so far has been to fit me up with an enormously fine cemetery of 200-volt lamps. Mr. Raworth.

Mr. Byng said that there were cases where as many as one in a dozen had short-circuited when put in the holders. Now I have a dozen of which 25 per cent. went when they were put into the holders; and I have another dozen from another manufacturer, whose name I will not mention, of which I think only two survived. They had the peculiar quality—that very unfortunate quality—Mr. Byng mentioned, viz., that some of them will not burn in a horizontal position. The moment they were put horizontally the filament came down and touched the glass, and off they went. Those things are rather important to consumers, therefore it is desirable they should know before they put up their fittings what they may expect.

We are all anxious to develop the very best system of distributing our current with the smallest expense, but we know that our electrical engineers who manage central stations are very conservative. They do not like any kind of coil or switch that may give trouble in a station. If they had had the care of these

Mr. Raworth. lamps in their stations, they would have gone a trifle more slowly than they have done.

All I want to do is to give the lamp makers sufficient time to perfect their lamp-making. If we run ahead of them, and fit up the whole country with 200-volt lamps before they are quite competent to make them, then our consumers who are not supplied with lamps free of charge will begin to grumble.

I quite agree with Mr. Byng that these high-voltage lamps do take more watts per candle than the 100-volt lamps. This means that every bill of £5 contains £1 to 30s. more than it would have done if the houses had been wired for 100 volts. That is rather an important matter from a central station point of view. But I am afraid that, if the consumer should learn that the use of 200-volt current may increase his bill by 25 per cent., he will not like it.

Mr. Shoolbred. Mr. J. N. SHOOLBRED: I am much obliged to you, Mr. President, for calling upon me; but, as I am in no way connected with the manufacture of the incandescent lamps mentioned in the paper, I have come here rather to learn something on the subject. For it is one in which I naturally take a considerable interest. With regard to the high electric pressures which are now coming into use, I have had a certain amount of experience.

As you probably may remember, Sir, the use of 100- and 110-volt lamps—on which you were good enough to give me very sound advice—was quite as much an experiment nearly 10 years ago, at Bradford, as the 200, 220, and 230 is at the present time. That voltage replaced the 50 volts, which was the voltage in those days. So far, my own experience at present makes me feel sanguine that the 200-, 220-, and 230-volt lamps will equally succeed; the more so as there are a very large number of indirect advantages in connection with the wiring which accrue to the consumers themselves, as also to the suppliers of the current. Without any doubt, with a little more experience, it appears to me, we shall all acknowledge that the higher voltage lamp is the one that will really suit us best in our various requirements. There are many things to be considered in the standard pressure of a town besides simply the lamps.

The question of motive power is now coming to the front; and there is no doubt whatever that, if we are enabled to utilise about 500 volts as the normal pressure, it will be a great advantage in the application of electrical industries. The author has referred to the absence of any opportunity, in the past, for an interchange of opinion between engineers and manufacturers on the question of the most suitable pressure to be adopted as a standard one. This is no doubt unfortunate; but I have been informed, on what appears to be good authority, that the question was suggested, and a paper submitted to the Council of this Institution some years ago, when central stations were very few in number, for the purpose of trying to arrive at some uniformity on the point. But, whether through pressure of other matters, or through the subject not being then considered of sufficient importance, the opportunity has apparently been lost; with the result that a great variety of different standard pressures exists in various towns. Mr. Shoolbred.

Unfortunately, in my opinion, it will hardly be possible, except at considerable expense, to obtain now anything like uniformity; since there are, at present, a large number of central stations over the country with varying pressures. Of course, as the author says, this variation in the E.M.F. of the lamp is, in one way, an advantage to the lamp maker. It also enables the different position of the several premises to be more suitably corresponded to by the varying pressure of the lamps; though it would have been very much better, no doubt, if we could have arrived at something like unanimity in the matter. The result, it seems to me, will be something like the old battle of the gauges in the early days of railways being fought over again. It is to be hoped, however, that in the end we shall arrive at "the survival of the "fittest." My own experience of the behaviour of these high-voltage lamps so far—and it has been mainly with 230-volt ones—has certainly not been discouraging, on the whole,—provided one adheres to well-known makes of lamp. Indeed, I should be very glad to use a pressure even of 240 volts, or more, for incandescent lamps, if the manufacturers could satisfactorily provide such lamps. The author also refers to the restriction, that in some quarters has

Mr. Shoolbred. been placed upon the development of these high-pressure lamps, by requiring the same size bulbs as with the lower pressure lamps. For my own part, I do not see why such a restriction should have been placed upon manufacturers; and I should not be disposed to place any such difficulty in their way. At the same time, I do not see an insuperable difficulty in thus allowing a larger sized cap, so as to permit more room between the leading-in wires. Of course some inconvenience might be caused in the first instance where 100-volt lamps were replaced in the same holders by 200-volt ones; but none in new cases where the higher voltage rules from the commencement. With regard to the suggestion as to the use of 100-volt lamps in series, I think it is not sound. I think it to be merely an expedient, and one that will soon fall through. The experiment has been tried. It reminds one of what took place years ago, especially on board ship, when two 50-volt lamps were placed in series. But this soon gave way to single 100-volt lamps; and the same will, I think, again take place, the two 100-volt lamps giving way to the single 200-volt one.

There is only one more point I should like to touch upon. and that is the question of the horizontal position. Of course, with incandescent high-voltage lamps, where long filaments are needed, any considerable deviation from the vertical position should be avoided; but with some makes of lamps, such as those known as "zigzag" lamps, the difficulty of the horizontal position nearly, if not entirely, disappears, as far as my experience goes. But on this point we shall no doubt be glad to hear opinions of manufacturers themselves.

May I be permitted, in conclusion, to join with some of the earlier speakers in congratulating the author on bringing forward this paper; especially at the present time, which is, so to say, one of transition, as far as electrical pressure is concerned; and I hope the Institution will reap a large amount of information in the discussion, which may guide us in the future in this matter. For my own part, I can only again repeat that I feel sanguine in the success of the higher voltage; and I am by no means discouraged by any apparent difficulties, or checks, which may have appeared in the way of the use of these high-voltage lamps.

The PRESIDENT: As I have here the names of several gentlemen who wish to take part in the discussion, and as the subject is an exceedingly important one, there appears to be no alternative but to adjourn the discussion to our next meeting.

I have to announce that the scrutineers report the following candidates to have been duly elected:—

Member:

Hiram Stevens Maxim.

Associates:

John Charles Bannister.
John Dewar Cormack, B.Sc.
Frank G. Mahon.
Robert Arthur McClymont.
Laurens Schmitz Meintjes.
H. Fentum Phillips.
Frederick Massingberd Rogers.
Richard Christopher Simpson.
William Ashbee Tritton.
John Walton.
W. B. Winfield.
Charles James Wood.

Students:

Thomas Forster Allden.
Arthur John Bohringer.
Walter Howley Derriman.
William Noël York King.
William Turner Marsden.
C. F. Maypee.
Francis S. Miller.
Percy Godfrey Pettifor.
Frederic Saunders.
Robinett Scruby.

The Three Hundred and Twelfth Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, March 10th, 1898—JOSEPH W. SWAN, Esq., F.R.S., President, in the Chair.

The minutes of the Ordinary General Meeting held on February 24th, 1898, were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council:—

From the class of Associates to that of Members—

Charles C. Hawkins.	Roland H. Streatfeild.

From the class of Students to that of Associates—

Walter James Coles.	Urban B. Gilbert.
Francis Edward Davies.	H. Godfrey Nicholson.
Norman Endacott.	Henry Joseph Norton.
Arthur Fredk. Malyon Gatrill.	H. Y. Packard.

Messrs. A. Jacob and R. W. Weekes were appointed scrutineers of the ballot for new members.

Donations to the Library were announced as having been received since the last meeting from T. B. Browne, Limited, and Professor Mengarini; to whom the thanks of the meeting were unanimously accorded.

The President.

The PRESIDENT: Before the discussion commences, there is one matter I should like to say a word in regard to. A testimonial is being organised in Belgium to show appreciation of the work done by M. Gramme. A banquet will be held in Brussels on the 29th, and at that banquet M. Gramme will be entertained

and presented with a testimonial. It was the feeling of the Council—and I am sure it will be your feeling too—that we should not be unrepresented in connection with this important matter. Every member present knows in what large degree electrical engineering is indebted to M. Gramme. M. Gramme undoubtedly was one of the first to produce a generating machine of the type into which the modern dynamo has developed, and I am sure there will be a widespread feeling that we in England should show that we are not ignorant of our great obligation to M. Gramme, nor indisposed to recognise it. The Council have resolved that a circular be issued to the members asking for a subscription, and the amount of subscription has been suggested. It is a small amount, and there will be, in connection with it, the issue of bronze and silver medals. Every donor of 10s. will receive a bronze medal, and every donor of 25s will receive a silver medal, referring to this commemorative occasion. It will be gratifying to the Council, and I know it will be in accordance with your feeling, that we should not appear apathetic in this important matter, but fully recognise our obligations in connection with it. The President.

The Council will present an address on the occasion in the name of the Institution, and Professor Ayrton and Dr. Silvanus Thompson have kindly undertaken to act as the delegates of the Council to attend the banquet and present the address.

We will now pass to the discussion of Mr. Byng's paper.

Mr. C. H. STEARN: I think, Sir, it is almost exactly two years ago since a discussion took place in this room on the subject of high-voltage lamps. I have little to add to, and certainly nothing to retract from, the observations I made on that occasion as to the principles on which I considered the construction of high-voltage lamps should be conducted. With your permission, I will just read two or three extracts before going on. I stated then: "The difficulties in the way of the construction of high-voltage "lamps are chiefly mechanical. Owing to the great length and "small section of the filament, it is extremely difficult to manage "in any reasonable compass, unless the construction usually "employed in lamps of lower voltage is altered. By increasing Mr. Stearn.

Mr. Stearn "the specific resistance of the carbon we can reduce this length "by about one-half; and if the filament be then supported in the "centre, or—what is equivalent—two filaments of half the length "be used in series, the filament is sufficiently rigid for commercial "use."* I then went on to say: "The double-filament type, for "many reasons, is the most convenient as a transitional form; but "there is no doubt that the single-filament will ultimately become "the predominant type." I exhibited a form that I suggested might meet the mechanical difficulties of a single-filament lamp, and I then said: "Besides the additional rigidity given by this form, "there is another advantage gained by the sinuous filament. The "change in candle-power is no doubt slower at the same efficiency, "if carbon of low specific resistance and low emissivity for light is "employed, than if we use the filaments of higher specific resistance "and of greater emissivity. As the mechanical objections to the "employment of low-resistance carbon will be thus overcome, we "can push the efficiency higher for the same rate of descent in "light." I pointed out then that there were two alternatives—the one, high specific resistance, with the drawback that there would be a more rapid change in candle-power; or, the retention of low specific resistance, with all its advantages, by an alteration in the form of the filament. Now the first alternative has, as I expected, come into more general use for a time, and I still consider it was more suited than the other for the transitional period, for the following reasons:—At the time that this change was proposed there was a very great feeling of timidity on the part of the public, and considerable opposition on the part of some electricians. The necessary conditions for carrying a movement like this through, in the face of the timidity which existed, seemed to me to be that the lamp should be durable in the first place, and reliable, so that there should not be frequently a number of lamps prematurely giving way—a fault which at low voltages would be taken no notice of, but at high voltages would be made the most of. I thought, too, that it was an essential condition at first that the lamp should not absorb a greater amount of energy than that to

* *Journal of the Institution of Electrical Engineers*, vol. xxv., pp. 272, 273.

which people had been accustomed. It would also have been desirable that there should have been no more rapid fall in light; but this, I thought, was a minor point at first, as it could easily be attained afterwards when the necessary change in the form of the carbon could be carried out. It was necessary also to show that small units, such as 8 and 5 candles, could be used. We were told at that time that the most terrible results would happen if electricians would persist in "flinging electricity about at this "tremendous pressure:" fires would occur, fatal accidents would be frequent, insurances would be raised, and the general confidence which was reposed by the public in electric lighting would be shaken to its very base. Now it seemed to me that the proper policy in a matter of this sort (I speak only, of course, in reference to my own practice, without attempting to prescribe that of others) was to move quickly, without waiting till we met every possible objection,—to walk, if we could not run. The adoption of the double-filament lamp with untreated filaments seemed to me to fulfil the above conditions of durability, ease of division into small units, and avoidance of increase in the bills of the consumer through increase in the energy absorbed by the lamps. Admitting the superiority of a treated carbon in respect of the light curve, yet this one point is not everything that is necessary in a lamp. The remarks that some speakers have made with regard to untreated carbon seem to me rather to refer to carbon of an earlier period than to that of the present day. If we go back to 1880 and 1881, undoubtedly untreated carbon at that time was rough, cindery, and fragile, and the duration without treating was very short indeed. The treating was adopted in the first instance for the purpose (I speak in reference to this country only) of patching up irregularities, and of strengthening the carbon for commercial use. I remember the first experiment to test the relative fragility in transit of treated and untreated carbon was tried in January, 1881, upon Mr. Crompton. A number of lamps were sent to him, half of which were made with the untreated filaments, and the other half prepared by the hydrocarbon process. The result of that was that none of those with the treated filaments were broken, but a great many of the Mr. Stearn.

Mr. Stearn. others were. Therefore for this purpose only at first it was adopted. We soon found its advantage in regard to the durability in running, but the advantage with regard to light curve was not discovered until some time later. Since then (in December, 1883) Mr. Swan has given us that beautiful process of preparing filaments by projecting a solution of cellulose through a fine jet into a setting solution.* This invention of Mr. Swan's has completely altered the aspect of the matter, and has rendered the construction of the high-voltage lamps of the present day possible at reasonable efficiencies; for, though some of the details have been subsequently varied, the essential principle of the precipitation of a cellulose solution into a setting fluid in thread form, as proposed by Mr. Swan, is still used. Carbons prepared in this way, *when the filament is skilfully made and skilfully carbonised*, do not require the treating process for equalising or strengthening, but are in all respects, except that of their more rapid descent in light, better and more reliable than if treated. The treating process, good as it is, has many drawbacks, and many more perils than attend the manufacture of good untreated carbon. If you take a large quantity of untreated carbon you can be sure, if you have taken proper precautions, that you will get equal, or nearly equal, consumption of energy with the same candle-power in all the filaments of the same batch when cut to a uniform length. You can depend on equal diameters, you can depend on very nearly equal resistances, and the durability is all that could be desired. When you once begin the treating process, you are liable to all sorts of variations if your workpeople are at all careless; and you are liable to variations, not in batches, as in the other case, but every individual carbon is exposed to the risk of being altered in regard to its diameter, its specific resistance, and the radiative quality of its surface,—by variations which are very easily introduced during the manufacture by the slightest change in the conditions, such as the temperature, the pressure, the rapidity of flow of vapour, the kind of hydrocarbon employed, &c., &c. For this reason it seemed to me, in view of the experience that we had had in

*British Patent No. 5978, 1883.

previous years of the failure to construct high-voltage lamps on the ordinary lines, it would be much better to use a process which was *safe*, and to introduce, as I said, the final improvements later on, when the form could be rendered more suitable. Treating, to be reliable, must be thick. Mr. Stearn.

> "A little treating is a dangerous thing :
> Treat deep, or touch not the accursed thing."

But the deeper the treating, the longer does the filament become.

If you wish to construct, say, a lamp of 230 volts, 16 candles, with a reasonable amount of carbon deposited upon it, you will have a length to deal with of something like 14 or 15 inches at least. Of course you may make it less if you make the deposit thinner, but ordinarily it would be something like that. Now a filament as long as this cannot be safely used, either singly or doubly, unless some special mode of disposing of it be used. For instance, *here* is a piece of wire which represents the length of a filament of this kind. It is just about 16 inches long, and is the length that you may have to deal with. If you wind it into two or three spirals it is still a very cumbersome thing to deal with; and when you consider that the diameter you are dealing with at the time, even with 16-candle-power lamps, is about the 280th of an inch, you will see that a carbon of such a length and of this diameter would be like a very flexible pendulum, and utterly unsafe unless shackled. If you, as I proposed before, bend it backwards and forwards upon itself, you may bring it into so small a compass that it will go within the *loop* of the other form of filament; yet we have in the zigzag form the same length of 16 inches that we were dealing with before, but the filament has now been rendered stiff and self-supporting. But if either that construction or something analagous to it is not adopted, then balancing the mechanical difficulties against the somewhat worse light curve, I should prefer to use the untreated filaments. But we are not, I think, reduced to the latter alternative, as we can, by modifying the old form of a filament, have a filament that will go into the same size of bulb that we have been accustomed to without any danger

Mr. Stearn. whatever, and we can treat it to our hearts' content and make it as long as 16 or 17 inches if desired. The fears of the public have no doubt passed away, and the demand which is gradually arising for a better light curve can now be continuously supplied; but for a long time to come the public will prefer a lamp of long life to one with an uncertain one, even though the latter may be the more economical. The factor of *convenience* is often overlooked in scientific calculations. The preference of the public for a lamp of long life is, after all, not an unreasonable one. Supposing that the light curve does fall, it falls gradually, and the user has the option of replacing it whenever he thinks proper. If the lamp has a good light curve and lasts a short time, he has no option—the lamp goes out whether he likes it or not. I was certainly surprised to hear from the author that the general consensus of opinion is that large numbers of high-voltage lamps, say one in 12, are expected to short-circuit at once. It is given us on the authority of several engineers, and I suppose there must be *something* in it; but may not this be a case of little Tommy and the thousand cats? On being remonstrated with, he admitted, "Well, after all, there was our Tom and another one." Is it not possible that on further consideration these large figures may be similarly reduced, and although in particular cases there may have been such disasters, these may, after all, not represent the general average of high-voltage lamps? But even if the statement be correct the causes of the short-circuits are probably not those imagined by the author. What are the causes of short-circuiting? Well, there are two causes which may not be due to faults in the lamps. One—and I think the most fertile cause of short-circuits—is the entanglement of the double filaments in transit. If users do not take the trouble to shake them apart when they get thus shaken together, of course short-circuits will occur. This defect would be entirely removed when single-filament lamps come generally into use. There is another cause—accidental cracks in the bulb. If a very minute crack occurs and the air enters very slowly, a short-circuit may occur from this reason. These are accidents which may occur at any time, as long as glass is as fragile as it is now, and apply to lamps of all voltages equally.

You may have external short-circuits through carelessness in bringing the wires together in capping, or carelessness in using improper soldering fluids. There is another, but not a very frequent, cause—when people attempt to frost the bulbs of finished lamps with chemicals. Of course the penetration of these into the cap causes a short-circuit, but that we may disregard. Now for internal short-circuits, which I understood Mr. Byng to allude to principally, it seemed to me that (if we except short-circuits occurring from accidental contact between the filaments) there is only one cause, and that is careless exhaustion. If lamps are carefully exhausted, taking certain precautions, it seems to me that this should be utterly impossible. Mr. Byng proposes to put the wires further apart, in consequence of this risk of short-circuiting. That reminds me of our Newcastle experience, when we went from 50 volts to 100. We thought that, as the distance between the wires was sufficient for 50 volts, it ought to be double for 100 volts, and we made a very clumsy form of lamp in consequence, with the wires at the side. When the public could not endure the hideousness of this lamp any longer, we tried the distance formerly used for 50 volts again, and, to the astonishment of everybody, no short-circuits occurred. I think that the same idea that was in our minds then, is probably in the author's mind now; but, as a matter of fact, the distance between is amply sufficient if the vacuum be properly made. I do not mean merely made good at the moment, but made so that it will be permanent. Under these conditions the distance is sufficient for 50, 100, or 250 volts, and I think for 500 volts, although I have not actually tried it. I have not been able to discover any reason for making a difference between high- and low-voltage lamps, either in the time of exhausting or in the manner of doing it, if only proper precautions are taken. Nor have I found any more difficulty in getting rid of occluded gases from untreated filaments than from treated filaments. The source of the gas that has to be removed in exhaustion is not from the filament itself, because all that must be driven off as soon as the filament is made white hot, almost instantly. The source of the gas comes from the thickened ends, or from the platinum wires—which only slowly heat up—or

Mr. Stearn.

Mr. Stearn from the surface of the glass bulb; but these are the same whether you use treated or untreated filaments. The increase in candle-power during running to which the author alludes is entirely under the control of the maker, whether he uses treated or untreated filaments. He can either have a very large rise in candle-power after the lamp is installed, or none at all. I was of opinion at one time that the best practice would be to eliminate this rise in candle-power entirely; but then, if you do so, the lamp begins to descend in candle-power from the time it is put up, and you throw away a certain part of the useful life of a lamp. Of course, if you leave the lamp in such a state that you have an enormous rise, the efficiency of the lamp increases so greatly that it produces, as the author has said, a very rapid descent afterwards; but lamps running at moderate temperatures, such as 3½ watts per candle or thereabouts, can be so regulated in manufacture that this rise in candle-power takes place very gradually and gently, only expending itself after about 300 hours. If you start at 16 candle-power, and the light rises within that time to, say, 17 or 17½ candle-power, or even to 18, there is no such great increase in the temperature of the lamp as to render it dangerous, and you certainly will get a much more satisfactory average light from the lamp during its life; but, as I said, that is entirely within the power of the maker to modify or eliminate as he chooses. With regard to the author's proposal to make the efficiency vary with the voltage, it certainly surprised me to hear that this was the general practice of manufacturers. I do not dispute it; I merely say I was not aware of it. But is this a sound practice? I take it that the reason it is proposed is that, if the filaments are not made equal to begin with, and if they are brought in the treating process to a fixed resistance and measured at a certain candle-power, it will result that you get the thicker ones at higher voltage and at a lower efficiency. He proposes, in order to reduce the price of lamps, that they should be sold with efficiencies varying with the voltages. Let us see how this will work out. We will say, for example, Westminster has 200 volts, and Edinburgh 230. If you allow a variation of ½ a watt per candle in efficiency for this difference in voltage, we shall have

the lamps that are successful supplied to Westminster absorbing Mr. Stearn. 56 watts. (I am supposing that you are trying to make lamps of 200 volts, 16 candle-power, at 3½ watts per candle.) Westminster consumers, consequently, will be quite content. THEY get their lamps cheaper, and with no extra cost in current. But *Edinburgh* will get the failures: the lamps supplied *them* will absorb 64 watts for 16 candles. If the lamps last 1,000 hours, that is 8 units difference. Well, at 6d. per unit, that is 4s. Then during 1,000 hours, in order to gain a cheaper lamp, consumers in Edinburgh will pay 4s. a lamp more for current. That is very nice for the lamp maker, but what will the canny Scot think of it? I do not know how much it is proposed to reduce the price of lamps, but we will suppose that the maximum price of lamps of this kind at present is 1s. 9d. Suppose you supply the lamps in future even at 6d., you make the customer a present of 1s. 3d., and charge him 4s. extra in current. It seems to me that makers should aim at obtaining accuracy in classification rather than cheapness in the lamps; until this can be done in all cases, it will be best to maintain whatever average standard efficiency is adopted constant for all voltages. It is true that there may be, and there are, even with the best makers, far greater variations in individual lamps than is desirable, but still the evil will be minimised by maintaining the *average* efficiency constant for all voltages. The author proposes to make the higher limit at 230 volts, but why should new stations throw away the extra 20 volts allowed by the Board of Trade? Surely the standard should be, wherever possible, 250 volts. Old stations, of course, as 115 volts was the maximum before, cannot go above 230, but it seems to me the new stations ought certainly to go up to 250 volts whenever possible; and if very small units are not required—that is to say, if a 16- or 20-candle-power lamp (20 by preference) is the smallest unit required—there is no reason why 500-volt lamps should not be used for street lighting. The author's statement that the various substances he mentions—boron, silicon, borates, silicates, oxides, &c.—which it has been proposed to mix with carbon filaments, *all* volatilise at 5 watts per candle, seems to me rather doubtful. *Some*, no doubt, do, but I have

Mr. Stearn. seen at least one of those he mentions that did not show any material change in efficiency for 300 hours at 2·8 watts per candle. I do not say that necessarily it was due to the compound; I only quote this to show that the compound did not, at least, do any harm, and did not volatilise. My own impression is that the compound in question was simply inert. A good result *was* obtained, but the same result has been obtained even with simple carbon. To sum up, I consider that, (*a*) other things being equal, if cautiously done, it is desirable to apply the treating process, but *without increasing the size of the bulb or using shackles*; (*b*) there is no need to alter the distance of terminals, as no danger of short-circuit should occur if proper care be taken in exhaustion; (*c*) there is no reason to suppose that, when made in equal quantities, and of good design, there would be any difference in the cost of producing single-filament lamps of high or low voltage; (*d*) to make efficiency vary with voltage for the purpose of cheapening the lamps would be false economy. One interesting point was raised in the discussion with regard to the longer life of lamps on alternating circuits. I am not prepared to say that it is not so, but I cannot help thinking that the degree of exhaustion has a very material effect in producing this difference. If the vacuum were not absolutely perfect, it is very probable that with an alternating current it would last longer than with a direct one; but if the vacuum is produced in the best way, I rather doubt it. The discharge which takes place if the vacuum is slightly imperfect no doubt would shorten the life of the lamp much more with a direct than with an alternating current. It seems to me that further evidence is wanted before we decide this question either one way or the other. In regard to the question raised by Mr. Mordey whether carbon *decreases* its resistance with increase of temperature because of the greater looseness of its particles, I would observe that if this is so it tends to negative his theory that the particles of the deposited carbon are more closely aggregated than those of the untreated, for the untreated reduces its resistance *less* by heat than the treated. The ratio of cold to hot of untreated is, at ordinary temperatures of the lamp, usually roughly about 1·7 to 1·75, but with the treated it is considerably

more than 2. Boron, on the other hand, behaves like a metal, and a very small quantity, if present in combination with carbon, will cause the usual ratios of cold and hot resistances to be *completely reversed*; and by suitable proportions of the carbon and the boron the resistance may be kept practically unchanged from 0° C. to a temperature corresponding to an efficiency of 1 watt per candle. I suppose that is considerably over 2,000° centigrade. What the actual temperature is I do not know. If only this compound of carbon and boron were durable, even if it continues as useless as the author describes it to be for lamps, it would be invaluable for resistances. Another point which throws some doubt on the theory that the particles of a treated carbon are really more closely aggregated is that—at least, according to a series of observations made two or three years ago by my assistant, Mr. Topham, which, as yet, I have not had time to verify—the actual breaking strain per unit of section is much less for treated than for untreated filaments, of course assuming the latter to be of the best modern type. Mr. Stearn.

Mr. R. E. Crompton: As the author's paper will be of value to those who are interested in the problem of electrical supply at double voltage, I will confine my remarks to my personal experience in this matter. The lighting of Harrow was laid out from the first instance for supply on the three-wire system, 440 volts across the outside conductors, using 220-volt lamps. After two years' experience in working the system we are well satisfied with it, and I have some confidence in saying that the consumers are equally well satisfied. Far from having complaints as to the behaviour of the lamps or any of the accessories, I think the complaints in this case have been fewer in number than is usually the case on ordinary 100- or 110-volt supply. Taking the various matters in the same order as is followed by the author, we have had no special difficulty with the incandescent lamps. We acted to some extent on Mr. Stearn's advice, as we recommended the users in the first instance to put in a considerable number of lamps having double filaments, and these worked well and were efficient; but latterly we have been chiefly using single-filament lamps. I do not think that there is any Mr. Crompton.

Mr. Crompton. perceptible difference in the burning life of the double-voltage lamps when compared with that of the ordinary 100-volt lamps so much used in London, but the makers of the double-voltage lamps have been much more accurate in actually making the lamps to the candle-power named; and I have noticed that this fact, which makes the 16-candle-power lamp actually give 16 candle-power, has caused a prejudice against the double-voltage lamps as compared with the single-voltage ones, because makers of the latter for a long time past have been giving lamps which at the earlier part of their life were giving 18 candle-power and were taking proportionately increased energy.

As regards fittings generally, I have noticed most of the difficulties named by the author; but by somewhat modifying the switches, and by enlarging and ventilating the fuse-box covers, we have arrived at the result that the double-voltage fittings are practically not more troublesome than the single-voltage ones. In the course of last year I carried out experiments for the Committee appointed by the Council of the Institution in order to prepare, if possible, tables of the length of break which might be required for fittings at various voltages, but I regret to say that these experiments did not yield results sufficiently concordant with one another to enable such a table to be prepared. We did, however, notice the sudden fracture of the covers of fuse boxes; and in my opinion these sudden fractures, which occurred so rapidly after the fuse melted that they might well be called "explosions," could only have been produced by the sudden expansion of the air contained within the cover; and, as a matter of fact, when we used larger covers, or increased the perforations so that the air could freely escape, the explosive effects were minimised. I think the author's idea of using a material such as plaster or cement in the space immediately surrounding the fuse is a good one; and probably the cause of this is that the arc that is formed at the time the fuse melts is very quickly cooled, on account of its doing chemical work on the plaster or cement.

I am sorry that I cannot agree with much that the author has said on the subject of arc lamps in connection with double voltage.

I certainly agree as to the advantage of single-parallel working on alternating circuits, and as to the economical advantages of using 10-ampere lamps; but his long description of the supposed difficulties of working the increased number of lamps in series which is involved by double voltage would lead anyone who did not understand the matter to suppose that the art of arc lamp design and construction in this country was in a very different state to what it really is. If what the author has alleged were the case in practice, a very large number of most of the successful installations of arc lighting would not work at all. Take the case of Liverpool Street Station, where there are several hundred lamps, all worked four in series on 200 volts: the number of cases of shunts being burned out by the causes mentioned by the author have not amounted to 1 per cent. per annum for a period of years, although there are no such cut-outs as he proposes to use. The cost of renewing this very small percentage of shunts is so infinitesimal, as compared with the cost of upkeep of a large number of cut-outs, that no one who had practical experience would propose the use of these cut-outs. I could give many other cases of the same character, where as many as 11 lamps are burnt in series on a 500-volt circuit, and where a similar immunity from failure has been observed, although in this case also there are no such cut-outs. Mr. Crompton.

In almost the last paragraph of the author's paper he states that revised wiring rules, and instructions on the use of twin flexibles and on the smallest gauge of wire allowable, are wanted. I think, if he turns to [illegible] which we issued last [illegible]r, he will find th[illegible] satisfactorily dealt [illegible]h, and that noi[illegible] this matter, at least [illegible]

[illegible] lamp [illegible]
[illegible] ay know
[illegible] nderstand
[illegible] of my
[illegible] due to the
[illegible] Germany [illegible]

Mr. Swinburne.

"the end of a text-book of chemistry, especially of the ordinary "kind, they ought always to put a note: 'N.B.—Chemistry may "'be quite different.'" I feel that the modern incandescent lamp may be quite different. Perhaps now people always get their lamps to come out at exactly the pressure, current, and candle-power intended, with no exceptions, instead of merely saying they do, as they did in my day. Perhaps lamp makers have now stopped advertising high-efficiency lamps without reference to life, and persuading customers that a lamp tested at a very few watts per candle at 110 volts always gave that efficiency, even when run at 100 volts in an increasingly blackened globe with a decreased current. Perhaps lamp makers have stopped talking of efficiency in watts per candle, when they mean candles per watt. I tried once to get efficiencies taken sanely by labelling lamps in candles per watt, but it did not appeal to anyone; and while people talk of "megohms per mile" and "candle-feet," of course watts per candle is a luminous way of taking efficiency. There are a few things that have not changed, though. I see in Mr. Byng's paper the old fallacy that a pitted filament has more radiating surface. Has the law of cosines not penetrated the minds of lamp makers yet? I cannot make out whether a change has come over another point or not. In old days I used to be a minority of one in holding that efficiency depended on temperature, so that all radiating bodies at the same temperature are at the same efficiency whatever their emissivities. I remember in the great incandescent lamp trial, ten years ago, I pointed this out in the witness-box. One side thought I was a biassed witness, and the other some kind of lunatic. I know I am not still quite a minority of one, but I do not know if I am in a majority yet. Generally, when this statement is made, I am quoted as saying that all bodies at the same temperature give out the same light per square centimetre, or give out the same light whatever the temperature, or saying something else equally absurd. Let me put my statement in several forms. "All "bodies at same temperature radiate at the same efficiency." "All "bodies at same temperature give out light of same colour." "In "all bodies at same temperature the heat and light emitted are in

"the same ratio; same as to all parts of spectrum." These are all varieties of the same statement. A carbon with a white surface thus gives less light at a given efficiency than a carbon with a black surface, and must be made larger for a given candle-power. Thus, suppose a black filament at 60 watts gives 20 candles, if it is now whitened and run at 60 watts, it will give more than 20 candles, and will, of course, also be at a higher efficiency. This is because the emissivity is less, so that it has to be run at a higher temperature to give off the power as heat and light. If it is now run at the old efficiency, it will be at the old temperature, and give less light and take less power. Mr. Swinburne.

Mr. Evershed says, "How about the incandescent gas?" I see nothing anomalous in it. Oddly enough, I have had far more to do with it than the incandescent electric lamp lately, but see nothing anomalous in its action. People generally assume the temperature of a Bunsen is low, and that the mantle has some special way of radiating light rather than heat at a low temperature. They measure the temperature with platinum thermopiles, which never come near the temperature of the flame. The shell or zone in which the mantle works is a long way above the melting point of platinum, and will fuse a small wire at once.

If a mantle of any material which has a high emissivity, such as any of the earths with platinum, iron (oxide), &c., in it—that is to say, a black mantle—is put on, it does not glow at all. (I may mention that a good absorber is a good radiator, and *vice versâ*, so that black has the highest emissivity.) If the emissivity were infinite, the mantle would of course remain at the temperature of the room, however hot the flame. If the emissivity is reduced, the mantle comes more and more nearly to the temperature of the flame. If it had no emissivity, it would get quite to the temperature of the flame, and give no light. At the two extremes, therefore, there would be no light. A mantle with a very small emissivity gets nearly to the temperature of the flame, and therefore gives a very white light. Its efficiency is very high, and the blue end of the spectrum is well developed because the mantle is hot. A slight increase of emissivity will reduce the temperature and efficiency, but may increase the light,

Mr. Swinburne.

making it redder. Some of the oxides are too white to give much light when pure. Thoria, for instance, if very carefully purified, will give a poor light. The emissivity is too low. The addition of the least trace of a coloured oxide, such as ceria, or of didymia (whether that is a mixture does not matter for the moment), increases the emissivity enough for the mantle to radiate plenty of light, without increasing it so far as to enable it to radiate energy so fast as to fall far below the temperature of the zone of the Bunsen. Of course it is a well-known fact—I do not know if it is true—that erbia gives a discontinuous spectrum. But erbia is not used in ordinary incandescent mantles, and an exception like that does not go to show that my statements are not true of more exemplary bodies which give continuous emission spectra.

With regard to flashing or treating, I used to make lamps which were not treated—that is to say, no carbon deposited on them—as I found them last better at a given efficiency. They were electrically heated to a very high temperature, and this had to be done before mounting, to obviate weak spots at the joint. Some of these carbons had about the same resistance cold as hot. They were made of silk drawn through a very strong solution of fuchsine in spirits and aniline. The material was the best I ever tried, but it was very difficult to get uniform, or to carbonise evenly at all. The fuchsine was a great nuisance. It used to get all over the tables, and all over the girls, who had a perpetual blush, and all over the floor, and, in fact, all over everything. Everything in the works became *couleur de rose*.

Mr Wordingham.

Mr. C. H. WORDINGHAM: I really do not know whether I ought to take up any of your time, but perhaps I may be forgiven if I occupy a minute or two, as I have come from Manchester largely to take part in this discussion. I think Mr. Byng's paper is very valuable, because there is no doubt that the question of 200-volt supply is one that very greatly affects central station engineers at the present time. In my own case I have had to give it very considerable thought. Of course we have always been able to supply in Manchester at a pressure of 200 volts, because we had the five-wire system; but when it came to extending, it was a

very moot point as to whether it was desirable to extend with three wires and 200 volts pressure on the lamps, or with five wires and 100 volts. I began with five wires, and I am now going chiefly on the three-wire system; though in certain streets I am putting in five-wire culverts with three conductors, so that if I find 100 volts desirable I can change over,—if not, the two spare spaces will be available for a feeder. Although I think there are still disadvantages in the 200-volt supply, I am bound to say that personally I think they are more than counterbalanced by the advantages. I have had very little direct experience of 200-volt lamps, but, judging by consumers, we have received no complaints that the lamps burn out quickly; and I conclude from that there is not any very great difference in durability in practice between 200-volt and 100-volt lamps Moreover, in certain cases I have actually been asked to supply at 200 volts, although 100 volts was available. I do not profess to speak as a lamp maker, because I know nothing about the ins and outs of the manufacture. I merely give my experience of everyday use. I think that incandescent lamp makers are rather apt to overlook the fact that arc lamps have to be burned on the same mains as incandescent; and from the central station point of view I cannot think it is desirable to go as high as 230 or 250 volts, and so compel the consumer to have at least five arc lamps, or perhaps less, and waste a good deal of current in useless resistance. Were it not for the fact that enclosed arc lamps by many makers are coming into vogue, I hardly think one would be justified in forcing consumers into the higher pressure; but, inasmuch as there are now enclosed arc lamps to be had without limiting the consumer to one or two firms, I think that objection very largely disappears. Mr. Byng says that the ultimate success of the high-pressure system will depend very largely on the verdict of the consumer. I really fail to see what choice the consumer has. If you say you are going to supply him at 200 volts, he has no large choice in the matter. He has to do the best he can. If he has never had 100 volts he has no opportunity of comparing that pressure with 200 volts. If you supply him with the higher pressure, he really gets the benefit of this in reduced price, owing Mr. Wordingham.

Mr. Wordingham. to the diminished cost of production, and also in lower cost of wiring. The matter which I take particular interest in is the question of fittings. At the Convention of the Municipal Electrical Association in 1896 I ventured to utter a warning that ordinary 100-volt fittings would not do for 200 volts. I was pooh-poohed, and told that the only way to get the destructive effects I had obtained was with my five-wire system; however, I think experience has shown that we must make a difference, and with 200 volts you must have different fittings. When we first began to supply 200-volt lamps, it was necessary that I should draw up some regulations to which the fittings must conform, and I then specified a minimum length of break, both in switches and fuses. Experience very soon showed that that was altogether wrong, as the length of break required varies with the design of the apparatus. The course I have adopted now, for some time past, is to specify that any switch which is to be used for 200 volts must break a 50 per cent. excess of current at 50 per cent excess of pressure; that is to say, if a 10-ampere switch is to be used on a 200-volt circuit, it must break a current of 15 amperes at 300 volts. Good switches do it all right. With regard to the cut-outs, I simply specify that the cut-out must be capable of breaking a short-circuit on 200 volts with a fuse wire in the cut-out of the size for which the cut-out is rated. That, I am sorry to say, is a very destructive test. But I have applied it to the particular fuse Mr. Byng has brought before you, and, although candidly I did not think it would do, I think, after trying it, that it is the best fuse I have seen. It acts perfectly. The 10-ampere fuse with 15-ampere fuse wire breaks a short-circuit at 200 volts perfectly. He says it always goes in the chamber, but it does not—it goes quite as often outside the chamber. My impression is that a good part of its working so well is due to the air being driven out of the holes and blowing the arc off the terminal. But that does not occur in all cases.

Mr. Rawlings. Mr. W. R. Rawlings: The lamp-makers have had a great deal to say upon this subject, but I should like to make a few remarks from the consumer's point of view. I am not exactly a consumer, but am one of those unfortunate contractors who have to

deal between the supply company and the consumer; and I might tell you that since the 200-volt supply first made its appearance I have been constantly worried. It has been pointed out that the consumers have no choice in the matter as to whether they should have 100 or 200 volts. I may say that during the last 12 months I have had to deal with something like 60 consumers whose installations have been changed by my firm from 100 volts to suit the higher pressure of 200 volts: they, therefore, have seen what 100 volts will do, and they now know the results obtained from 200 volts; and I can assure you, Sir, it is painful to have to meet those consumers. My firm has also fitted up houses for 70 new consumers during the same period with 200 volts, and it is really quite a pleasure to meet these people, for they know nothing whatever of the luxurious 100 volts, and are therefore quite satisfied with what they have. Keeping to the consumer's point of view, we are told that we must adhere practically to a 16-candle-power lamp. But this is ridiculous. My firm have provided 130 consumers with 8,000 lamps of 200 volts, and personally I have had to do with society consumers (not shop consumers or street lighting), and unless they can have a small lamp they prefer, and will use, a candle. We are told by some to put our lamps in series. But is that advisable? I know of houses now which have lamps of no less than four separate voltages and a very large number of separate candle-powers. This is a very unsatisfactory combination, especially when a 50-volt lamp breaks down and the servant replaces it with a 25-volt lamp, with the result that a fitter has to go next morning to repair a fuse. There is no doubt that the electrical supply of 200 volts will be a decided advantage to all, more particularly if we can get the supply cheaper; but at the present moment the lamps are most unsatisfactory, and unless the lamp makers come forward at once with some good lamps, small enough to allow of their being adapted to fittings as they are now designed, we contractors shall, I fear, have much trouble before us. With regard to fittings, I have placed on the table one or two switches for which I hold patents. They are made by Messrs. Verity, and I naturally consider them perfect switches for 200 volts; but I will

Mr. Rawlings.

Mr. Rawlings. not say any more, as specimens of them are on the table for inspection.

In conclusion, Mr. Byng deserves our best thanks for having brought before us the fact that—and the reason why—a 200-volt lamp is not equal to those of lower voltages; and if only the supply companies will be as frank with their consumers, we poor contractors may not receive the blame for supplying a bad lamp, when we are doing our best to assist them in their advanced method of supply, which neither the consumers nor the lamp-makers are prepared to deal with.

Mr. Gaster. Mr. L. Gaster: The question of incandescent lamps is very important, and requires great consideration. Some 10 years ago Dr. H. F. Weber, Professor at the Polytechnic, Zürich, formulated a law expressing the interdependence between the radiating power (Strahlungsvermögen), temperature, wave-length, and quality of the solid bodies, and was thus able to formulate a general and complete theory of the incandescent lamp.*

Examining the formulæ expressed by him, we find that the temperature to which the filament is heated; the factor B^2, called by him "Leuchtungsvermögen" (lighting power), and the product of $B^2 \lambda^2 T^2$, where λ = wave-length, play a very important *rôle*. It is sufficient to vary B^2 by only a few units per cent. to increase the efficiency a great deal. I have had the opportunity of examining a good many lamps, and some tests very carefully made with different lamps, and I find that this factor B^2 varies in such a manner that it is greater for those with higher efficiency. For the Edison & Swan, Woodhouse & Rawson, new Cruto, and Siemens & Halske lamps, it was found that B^2 varied about 8 to 9 per cent., being greater for the two first mentioned; and this difference for B of only about 4 per cent. led to a great variation in the efficiency. This variation may be due to the material used by different makers, and also to the process of manufacture, which very likely may be improved still more in time to come.

The temperature to which the filament is exposed in incan-

* For the mathematical development of that law, and further valuable information, see "Sitzungsberichte der Berliner Academie," 37, page 933, year 1888.

descence is a very important factor in the efficiency of the lamp. Mr. Gaster.
The temperature of volatilisation for the most of the present carbon filaments lies between 1,594° C. and 1,607° C., and if we do not want to shorten the life of the lamp we must not go beyond this temperature. I believe we might find some means of making the filament of such materials that we could not only increase the lighting power, B^2, but also the volatilisation temperature of the filament, which would render the lamp more efficient than it is now. It is very likely that boron—which possesses some properties analogous to carbon—or its compounds may, under special treatment, enter into combination with carbon, and so lead to a successful result. Experiments in this and other directions have been carried out, and promise to lead ultimately to success.

It is better for the central station engineers, by means of their machines and governors, to keep the voltage fluctuations as low as possible (1 to 2 per cent.); otherwise the lamps, especially those with high efficiency, will give way, and so shorten enormously their life. The relation which exists between the energy spent and the light given out by an incandescent lamp seems to be best expressed by the formula laid down by Professor Voit, of Münich, and corroborated by the law and experiments made by Professor Weber. That is,

$$H = q \times E^3 = q \times \frac{\Delta P^6}{W^3} = q \times i^3 \times \Delta P^3,$$

where H = mean horizontal luminosity, i = current, ΔP = voltage, and q = constant for the practical limits under which we are working. I mention this formula because it corresponds most nearly to the reality, and shows how much the fluctuations of voltage will interfere with the steadiness of light, increasing the temperature of the filament, and so shortening the life of the lamp. This factor q may vary with different makes of lamps, but it is nearly constant for one and the same lamp tested. The shape of the filament does not only contribute to its rigidity and other mechanical and electrical requirements, but it adds materially, if it is well devised, to the more equal and better distribution of the light given out in all directions. From a great number of tests made at the Polytechnic in Zürich we

Mr. Gaster. found that this light emitted in all directions (räumliche Helligkeit) has a mean value lying between 0·71 and 0·85 of the mean horizontal value, this varying with the shape of the filament. This point is also, I think, worthy of consideration in selecting the shape of the filament. Experiments made by Messrs. Garbe, Violle, Nichols, Gruner, and Professor Langley corroborate the validity of the law formulated by Professor Weber. That being the case, without wishing to detract from the valuable results attained by Mr. Byng, I think that, until a complete study has been made of the different values which B^2 may assume when different materials are used, we are not entitled to look upon the researches in the direction of improving the filament as finished; and I believe that we may in time to come improve the efficiency of the lamps by varying either B^2 or volatilisation temperature, or both together, by a good combination and wise selection of the materials, and by due care in their manufacture.

In reference to the question of fuses, I quite agree with Mr. Byng that there is no uniformity either in the rules made by the central station engineers and manufacturers, or in the character of the material selected, or in the current-strengths at which the fuses should melt. There are a good many circumstances which have to be taken into consideration in selecting a type of fuse. Some consider a great cooling surface to be necessary, and therefore either introduce ventilating holes, or make the fuse thin and wide, or the like. But the greatest importance is to be attached to the forming of good contacts between the fuse and terminals, as very often fuses go before reaching the prescribed current-strength, merely through over-heating at the contacts. A careful investigation of the subject would lead at least to greater uniformity in the sizes and types of the different fuses, if not to a complete understanding between engineer and manufacturer, and to a clearing up and explanation of the discrepancies which exist between the different rules and points of efficiency.

Enclosed arc lamps have been mentioned, and I think that the higher voltage system will stimulate the introduction of lamps of this kind, because they will work more economically than they do now on the ordinary 110-volts circuit. That unnecessary waste

of energy in heating the resistance put in series with the lamp, which forms one of its chief disadvantages, may be reduced greatly by using three lamps in series at 230–240 volts, where now we have only one on the 110-volt circuit. The other drawbacks of these lamps—namely, that a coating may be formed on the inner side of the inner globe, due to impurities and to the imperfect combustion of the carbons, and that a double globe must be used with them, which causes a reduction in efficiency—may be counterbalanced by the lower consumption of carbons, and by the reduction in the attendance necessary. The paper is full of very interesting and useful suggestions, which I believe will bring about further improvements, and help in the development of the higher voltage system, which, within certain limits, is the more efficient one, taking into consideration the enormous reductions in the size of feeders and distributing wires all round. Mr Gaster.

Mr. A. A. C. SWINTON: The author, in his very interesting paper, appears to think that, when two wires at different potentials are contained in a vacuum inside a lamp, they have a greater tendency to discharge from one to the other the nearer they are together. Now, of course, this is so in air; but I believe that it is generally recognised that in high vacua the nearer the two wires are together the greater is the difficulty of causing a discharge to take place between them. I know from my own experience that this is invariably the case in very high vacua, and I believe that it is also so in vacua of the degrees employed in ordinary incandescent lamps. Mr. Swinton.

Of course, in making efficient lamps of the ordinary filament type for high voltage, the chief question is that of obtaining an electrical conducting substance which has the mechanical properties necessary to make a filament, and which will also stand a sufficiently high temperature without deterioration. I do not know, however, that in the future we need necessarily be restricted to lamps with a filament. It has always been a matter of some surprise to me that in the early days of electric lighting, when what was then termed the subdivision of the electric light was being discussed and experimented on, Sir William Crookes did not return to his much earlier investigations with cathode

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hat specific resistance should be taken Vith carbon as at present used in lamps, istance generally accompanies increased vity).

Mr. Swinton. rays, and endeavour to produce a satisfactory lamp on the cathode-ray principle. As Sir William Crookes showed years ago, there is no difficulty in obtaining enormous temperatures by means of cathode rays. It is possible easily to melt platinum, and it is also possible, I believe, to obtain the temperature of the arc in any suitable substance upon which the cathode rays may be concentrated. I imagine also—although I do not think that this has ever been actually measured—that these very high temperatures can be obtained more economically in this way than with the arc. This for the reason that with the ordinary arc in air a considerable amount of energy must disappear uselessly in heat convected and conducted away through the air; whereas, if the heat be produced in a body surrounded by a very high vacuum, you have practically no air either to convect or conduct the heat away. I know that there are great difficulties in making satisfactory lamps on the cathode-ray principle, but I am not at all sure that these cannot, and will not in time, be surmounted. One of the difficulties is that you cannot get such a lamp to work with less than about 20,000 volts, which is an amount that does not at present comply with the Board of Trade regulations. I do not know, however, that that need be a disadvantage for certain purposes, as lamps worked with even 20,000 volts might be used with a small step-up transformer for each lamp for street-lighting purposes, where they might perhaps take an intermediate place between arc lamps on the one hand and ordinary incandescent lamps on the other. However, as time is short, I will not enlarge upon this subject, though I could say a great deal upon it. I would point out, however, that the non-filament lamp has the advantage that in it you are not restricted to electrical conductors for your incandescent material. You can, in fact, use non-conductors, and consequently you have a much larger range of refractory substances, some of which would be quite inadmissible for ordinary filament lamps.

In any case I think it is well to bear in mind that very possibly in the future cathode-ray lamps may become commercial.

Mr. Miller. Mr. H. W. MILLER: We have in Kensington some 600 consumers, and the equivalent of 50,000 8-C.P. lamps, all working

at the higher pressure. About half of these consumers have been changed from 100 volts to 200 volts, and I may say that no difficulty has arisen in making this change. We have had a certain number of complaints from those people who have been changed over to the higher pressure, and the principal complaint is that they have not got the same amount of light that they had with the lower pressure; and I think, from my own experience, they are right. The average 16-C.P. 200-volt lamp at the present time on the market gives a very poor light, in comparison with the 100-volt 16-candle lamp. To make the change a success it is necessary to be very careful in selecting the lamps. There are on the market lamps which are quite as efficient and quite as satisfactory as the ordinary 100-volt lamp, but at the same time there are some very bad ones, and it is these bad lamps which are causing the trouble with consumers. I am sorry to see that the author suggests that the results of 13 years' experience in making the lower pressure lamps have had to be thrown away and a new type of filament manufactured. This practically means that an experimental and untried lamp is being put upon the market, to the detriment of the public. I think it would have been far safer in the early days of the higher pressure lamps to have kept to the standard 100-volt filaments and used two of them in series. Although it has been pointed out that there is some difficulty in finding room in the ordinary size of bulb, there are many lamps on the market which do contain two 100-volt filaments in series. I cannot agree with the author's statements when he speaks about combination filaments. I have made a great many tests of combination filaments, extending over some years, and the experience I have gained teaches me that the high-efficiency lamp of the future will be made of such a filament, for the simple reason that these filaments will stand a very much higher temperature than carbon. There is no difficulty in getting a high-efficiency lamp starting at 2½ watts to give results equally good, as to durability and candle-power, with those of the best quality carbon filaments working at 3½ watts. These results have been actually obtained in practice. Passing on to the question of fuses, I do not agree with what the author

Mr. Miller

Mr. Miller. has said about the working of the special fuse he describes. Mr. Wordingham has already pointed this out. The action of the fuse is really this: When the fuse melts, the hot air and the vapour generated in the central chamber rush out through the two side holes and blow the arc out; and therefore I think that it is wrong for the author to state that ventilation holes are of no use whatever, because he actually has to put two ventilation holes, or what corresponds to ventilation holes, in this type of fuse. If those holes were stopped up and the fuse subjected to a short-circuit across 200 volts, the cover would be blown to pieces, or the fuse would not act and break the circuit. There is another matter I should like to speak about. A number of engineers find some trouble in knowing what to do with a small motor, working at 100 volts, driving a ventilation fan. It is difficult to get it rewound—in fact, it is hardly worth the cost of rewinding. I find one way of getting over the difficulty, in the ordinary 100-volt shunt-wound motor, is to put a shunt winding in series with the armature, and run it simply as a series motor; it works very well under this condition. I forgot to mention one thing which is connected with my name in the paper. The author states that copper fuses are not satisfactory because they maintain a dull red heat under normal load. It is not the usual practice to run fuses at a dull red heat, or at anything approaching such a temperature. The reason why copper has been suggested, or any other hard metal, is because with very fine fuses it is impossible to use a soft metal, such as pure tin, under a binding screw without breaking it to pieces.

Mr. Robertson. Mr. C. J. ROBERTSON: I think it necessary to point out that the author has not run down *all* high-voltage lamps, but has only pointed out some of the peculiarities of high-voltage lamps with *unflashed* carbon. He has shown that for such lamps flashed carbon will give us the ideals sought for in a good lamp, stating on page 122 that "such lamps compare favour-"ably with those of lower voltage." The "Robertson" Lamp Company make several types of high-voltage flashed carbon lamps, as shown by the exhibits on the table; but in some cases it is necessary to use larger bulbs than those in general use.

For instance, the 220- and 250-volt lamps require bulbs about one-quarter to three-eighths inch larger in diameter; and we would like to feel that this small increase in the size of the bulb will be acceptable to engineers and contractors, although we are quite prepared to supply 220–250-volt flashed filaments in the usual bulbs. Mr. Robertson.

Now that engineers and others have passed over the responsibility to the lamp makers of providing high-voltage lamps as good as those of low voltage, success can only be obtained by an interchange of experience. For instance, smaller bulbs are possible if unflashed carbon, with its concomitant disadvantages, be used. On the other hand, to obtain the advantages of flashed carbon without any accompanying disadvantage, assent to the use of larger bulbs is sometimes necessary.

It is now a fact that 999 lamps out of every 1,000 manufactured have filaments derived from a solution of cellulose in zinc chloride (for whose introduction and practical working we have to thank, and are under great obligation to, Messrs. Wynne, Powell, & Evans), and that each of these filaments have approximately the same specific resistance in the finished lamp. When the mounts, vacua, &c., of such lamps are equal, then it is obvious that present-day unflashed lamps should be very similar in the characteristics and behaviour of their filaments. I have tested most of the high-voltage lamps now on the market, and find that this is so, and that such unflashed lamps have the peculiarities pointed out in the author's paper. In deference to the wishes of some consumers for a small-sized bulb, my company have made such lamps, some being exhibited on the table. I do not advocate that flashed lamps are a panacea for all ills, as there are certain styles of flashing which will produce a carbon which is no better than a well-baked unflashed one. Also, unflashed carbons, if baked in an electrical furnace, occupy the position of being between a properly flashed and an ordinary unflashed baked carbon.

Mr. Mordey thinks that specific resistance should be taken with specific gravity. With carbon as at present used in lamps, lowness of specific resistance generally accompanies increased density (or specific gravity).

Mr. Robertson.

The carbon needles formed of retort carbon as found inside gas retorts, suggested by Mr. Mordey for use in lamps, have been tried; but the quality of such carbon is not as good as that of well-flashed filaments, and it is, besides, in a very unmanageable form. I should have liked to hear the experience of others on the relative advantages of burning lamps on alternating and continuous currents—a subject referred to by Mr. Mordey. My later experience, since writing the letter to the *Electrical Review*, to which he has referred, strengthens my previous opinion, stated therein, that an alternating current of the same percentage variation as a continuous one tends to increase the life and retention of candle-power of lamps. Touching on this subject, I can recollect, 11 or 12 years ago, that Lord Kelvin drew attention (as a result of experiments he had made) to the fact that, with continuous current, lamps would last longer if the polarity of the circuit were constantly changed; and he advised all engineers to make provision for this by means of double-pole reversing switches. I have seen stations where such are still in existence, but they are not now used, as the object for which they were originally installed has doubtless been forgotten. This is really corroborative of my own experience with alternating currents, as continuous currents of very rapidly changing polarity are alternating currents.

There seems to be some additional proof as to the uneven wear of the filament subjected to a continuous current, from the experience of the manufacture of the original "Cruto" filament. This was formed by raising a very fine platinum wire to incandescence in a hydrocarbon gas. By this means carbon was built round the wire; and, as continuous currents were used for this purpose, it was found that there was always a far greater amount of deposit on the negative than the positive end, and for this reason the polarity of the current had to be continually reversed, in order to obtain an approximately even building up of the filament.

A close inspection of the filaments which have run on alternating and continuous current shows that there are distinct physical changes going on, which explain the advantages obtained by running lamps on alternating current.

Mr. Shoolbred has remarked that he has not found much trouble by burning 220-volt lamps in a horizontal position, at least with wavy-shaped filaments. Samples of such lamps made by my company are also shown among the exhibits; but my experience with these, and all similar types of wavy-filament lamps which are now on the market, shows that such wavy filaments, when used for horizontal burning, are not quite successful unless supported, or unless they have unflashed carbons, with their unavoidable peculiarities, as enumerated by the author—points, however, upon which Mr. Shoolbred does not touch. Flashed wavy filaments at present on the market seem to be no better for horizontal burning than other older types of high-voltage lamps, for, in spite of being wavy, their filaments soon sag on to the bulb. It is these experiences which have led to my company designing their new type of flashed high-voltage lamps with curl filaments, each curl being supported; as shown among the lamps exhibited and labelled, "New type 200- and 220-volt lamps with "supports." Mr. Robertson.

Numerous and lengthy tests prove that such lamps are giving every satisfaction, and are quite equal to 100-volt lamps. This type also has the additional advantage that high-voltage lamps with low candle-power are possible, such as those of 200- or 230-volts 8- and 5-candle-power, at 4 and 4½ watts.

Some of the previous speakers seem to have taken exception to the author's remark, on page 122, that "I have *heard* several "engineers say that they expect about one in twelve to go in this "way." This is not, however, the experience of the "Robertson" Company, and doubtless refers to some early experience with some foreign type of lamp, as our experience shows that carefully made and exhausted high-voltage lamps are no worse in this respect than 100-volt lamps.

With reference to Mr. Stearn's remarks, I am glad to hear that he agrees with the author that the ideal high-voltage lamp is one with a flashed carbon, and that unflashed carbons are only a transitional stage of development. The "Robertson" Company's new high-voltage lamps, however, have passed out of this transitional stage into the ideal one; that is to say, they have

Mr. Robertson. well-flashed carbons. Mr. Stearn now finds virtues in his present untreated carbon filaments—which, by the bye, are very ancient—and points out that previous filaments were soft, porous, cokey, &c. This might be misleading, unless Mr. Stearn refers to either filaments of the dark ages in the laboratory, or else to the parchmentised cotton filament of which he has had so much experience. The present "Stearn" filament is, I have every reason to believe, one of the 999 class to which I have previously referred (squirted cellulose in zinc chloride).

This filament is one with which I have had 10 years' continuous manufacturing experience, and it had, when properly made, even 10 years ago, the same virtues that it has to-day. Mr Stearn is therefore somewhat tardy in extolling its virtues. It was used by the Brush Company for their lamps in 1886, or even earlier, and is used by their old factory to this day. About 1887 this filament was in general use by the larger Continental makers, and is also used by them to-day. Right along from 1884 to the present time similar filaments have been continuously made from modifications of cellulose as used by Weston and passed on to Khotinsky and Siemens, and also, I believe, by the Allgemeine Company of Berlin, &c.

There is also the equally glass-like filament of Woodhouse & Rawson, of several years ago.

In comparison with the above long experience gained with this class of filament, Mr. Stearn would appear to be a comparatively recent convert; and the Edison & Swan Company have, I believe, been only recently converted to partially adopting it—about 18 months ago, or thereabouts.

In addition to the above ancient history of glass-like non-cokey filaments, which are considered the best to-day, we have the extraordinary fact that, in spite of the long manufacturing experience obtained with this filament since 1884, none of these firms left such filaments as they were, but generally flashed or treated them; and many valuable papers have been read in the past as the result of experience made on the comparative advantages of flashing this same filament. The final fact which will prove to everyone that the virtues of flashing or treating are

real, and are more than meet the eye, is that even those firms who now use unflashed carbon for their 200-volt lamps still continue to make all their 100-volt lamps with *flashed* carbon. Mr. Robertson.

I would point out, with reference to Mr. Stearn's remarks on efficiencies, in which he refers to the author's remarks in the seventh line from the bottom of page 124, that there is a printer's error in the position of the comma, which should come after the word "efficiencies," and not after "voltages." The author's sentence, instead of reading, "and the present practice of varying "efficiencies with voltages," &c., &c., should read as it will stand in the revised report in the *Journal*. With this correction Mr. Stearn's remarks show that he is in practical agreement with the author upon the variations of manufacture and the convenience afforded of supplying efficiencies at any required voltage, and that this is conducive to cheaper manufacture.

I quite agree with Mr. Stearn's remarks on the all-importance of a perfect vacuum necessary to produce the perfect lamp. This interference, in producing a good vacuum by the gases emanating from mounts and platinum wires, is, of course, a very old subject with lamp makers, and the necessity of getting rid of these gases is distinctly pointed out in Swan & Edison's old patents for heating and incandescing lamps while undergoing exhaustion.

In the author's paper this was not touched on, as it was so well known; but he points out that some consideration should be given to the varying amount of occluded gases in unflashed as compared with flashed filaments.

As also pointed out by Mr. Stearn, the accidental cracks are undoubtedly the greatest cause of deterioration of vacuum and premature breakage in high-voltage lamps. It is for this very reason that, after having had experience of manufacturing several millions of lamps with the form of seal now used by nearly all the Continental manufacturers, the "Robertson" Company have given up this seal (sometimes called the German seal) for the pinch seal, which we consider superior in this respect. I may point out that this form of seal is that now used by all the English manufacturers.

Mr. W. GEIPEL: I do not propose to make any remarks, Sir, Mr. Geipel.

Mr. Geipel. but I think there is evidence that this subject which is under discussion is of great interest to the profession, and there is no doubt that if it is to be thoroughly thrashed out it should be done now, and not five years hence. For that reason I should like to ask whether it is absolutely essential that the discussion should be closed to-night, or whether it could not be carried on to the next meeting.

The President. The PRESIDENT: Well, Sir, I may tell you that definite arrangements have been made for papers which will fill up the time of all the meetings, without allowing any unusual time for the discussion of those papers. We can only extend the discussion on Mr. Byng's paper by having an extra meeting, and there might be difficulty as to that in connection with the place of meeting. I think it would be better, on the whole, to finish the discussion to-night, even if we continue the meeting to a rather later hour than usual. If you have anything to say, perhaps you would like to say it now.

Mr. Geipel. Mr. GEIPEL: I will be quite content to submit my remarks in writing, but at the same time I think a question of this kind is very much better discussed verbally than by written remarks which are relegated to the Proceedings.

The PRESIDENT: If you think so, perhaps you will go on.

Mr. GEIPEL: When I made that suggestion, Sir, I did not make it with a view of bringing myself forward as one of the speakers. However, as you have suggested, I will occupy not more than two minutes. The whole question of 200-volts and 100-volts supply seems to me to resolve itself upon the ability to keep a constant pressure If you can keep a constant pressure with 100 volts, then I think that we are all agreed a 100-volt lamp is a better lamp than a 200-volt. Mr. Byng himself says that with regard to 200 volts there are certain difficulties. About 8 per cent. of the lamps break down immediately they are put up, owing to short-circuiting. Their life is admittedly shorter, their efficiency is admittedly lower, and their cost, if not double, is at any rate approaching that. Therefore I think the consumer has an undoubted interest in this question. Mr. Byng says it is the central station engineer who has been bringing forward this

question of the 200-volt lamps. I have, in this very room, heard makers of 200-volt lamps urge the desirability of adopting that system. If we come to consider that there are certain defects in the lamps as to price and durability which increase the cost to the consumer by about double—in other words, that the contract for lamps is made double what it would be in the case of 100-volt lamps—then the urging of this system by lamp makers is not entirely disinterested. When this matter was discussed two years ago, on Mr. Addenbrooke's paper, there were very few instances of the use of 200-volt lamps. They had just been brought into use. One was at Bradford, where Mr. Gibbings stated that his experience was that the lamps were satisfactory to the consumers; but, unfortunately, at that time they had not had their accounts. I hear they have had their accounts since, and the result has been that the Corporation had to give them free lamps to keep them quiet. I have several other points to which I should like to refer, but I really do not feel justified, if the discussion is to close to-night, in keeping Mr. Byng any longer from replying. I shall be very pleased to write anything further I have to communicate. Mr. Geipel.

I have pleasure in communicating the following remarks in addition to what I said at the discussion on this paper.

In doing so I desire to repeat that in my opinion the Council would have met the desire of the members if more facility had been afforded for the discussion of the questions which are brought before us by the author.

At the present moment there are many important supply works who are unable to maintain the pressure within the limits specified by the Board of Trade, and who are face to face with the necessity either of extending their present mains or of raising the pressure of supply, which involves a serious outlay on new plant and lamps; while there are many new works in projection where the question whether to adopt 100 or 200 volts must be decided forthwith. It is already late enough in the day, therefore, to discuss this question, but years hence it will be still worse, for those works which have adopted the wrong system will be practically unable to rectify their error.

I think we are all agreed that the aim of electrical engineers

Mr. Geipel. should be the supply of electricity at such a pressure as will enable the consumer to utilise his electricity to the greatest advantage.

I have heard it stated that, considered from the undertaker's point of view, the 200-volt supply is better, but that from the customer's point of view the 100-volt is preferable. I am quite unable to agree with that argument. In my opinion the interest of each lies in the same direction.

Now the author says that the verdict of the consumer depends largely upon the ability of the trade to supply fittings which shall be comparable in efficiency, economy, safety, convenience, and appearance, with the lower voltage system. I beg to differ entirely from that opinion. The verdict of the consumer, as far as concerns lighting, will be given almost entirely on the cost of the candle-power-hour. There may be a few wealthy consumers whose primary objects are those enumerated by the author, but not so the poor man. I agree with Mr. Preece that the electric light is the poor man's light, and it is from his point of view I have ventured to take part in this discussion.

If this is a correct view, then I take it that it is a *sine quâ non* that the consumer should be able to use an efficient 8-candle-power lamp. In the discussion on Mr. Addenbrooke's paper I asked the following question:—"Can lamp makers "produce an 8-candle-power 200-volt lamp to work at a strain of "3 watts per candle-power?" That question has up to the present remained unanswered, but I gather from Mr. Byng's paper that his answer would at any rate be in the negative, even after the further experience since gained; and I for one can say, of my own knowledge, that it is as yet far from being done.

I am aware that lamp makers claim great improvements, and very much needed they are, whatever be the pressure; but what great improvements have there been? Omitting for the moment the question of the increased strain which the cheapness of the lamp permits, have lamps, as made to-day, a much greater life (at a given strain) than they had years ago? My experience is that they have not. On the contrary, the improvements which have taken place since Edison first brought out his 100-volt lamp—that

was at the inception of lighting by incandescent lamps—have, unfortunately, been far less than could be desired. Mr. Geipel

I do not take the view that improvements may not be expected: we all want an improved lamp; what users want is a lamp which will give 8 candle-power for 16 watts, and I look forward to that desideratum being realised with the 100-volt lamp before it is with the 200-volt. With such a lamp, and energy at 2d. per unit, the cost would be equal to, say, a 4-foot gas burner using gas at 8d. per 1,000 cubic feet. We can then veritably say that the electric light is the poor man's light.

Again, let us consider what has been the effect on the price to the consumer of the 200-volt system in those works where it has already been adopted. I do not know of a case where it has resulted in a greater reduction than 20 per cent. in the price per unit. But to obtain this reduction a lamp must be used consuming at least 33 per cent. more energy—probably more like 50 per cent.—to say nothing of the extra cost of the lamp itself; so that the candle-power, instead of costing less, costs more.

I do not say that there are not isolated cases in which it may be advantageous to use a 200-volt supply; but I do say that in all important towns, where there is, of course, a preponderance of the poorer classes, the use of 200 volts is totally indefensible. It would only be defensible if it happened that it was impossible to keep the pressure at the lower voltage within the limits allowed by the Board of Trade. But it is not so.

There are other points in the paper upon which I had intended to touch, such as the unsuitability of 200 volts for arc lighting purposes, and otherwise; but I feel that they are, when compared with the above urgent question, of such minor importance, that I shall conclude by joining other speakers in thanking Mr. Byng for his excellent paper.

Professor SILVANUS THOMPSON: It is a little unfortunate, Sir, that this discussion is carried on so largely from the point of view of the station engineer and the lamp maker, for the poor consumer ought really to come in somewhere. Speaking from the point of view of a consumer, I must say that I would far rather remain where I am—at 100 volts—unless by some kind Prof. Thompson.

Prof. Thompson.

providence I could be supplied at 50 volts. Even with 100 volts the 8-candle lamp is not all one could wish, and 8-candle lamps are much better at 50 volts than 100. So long as we want in our houses lamps of small candle-power, an advance from 100 to 200 volts is a thing essentially to be deplored. The difficulties in switches that arise at the higher voltage have not at all been over-rated. I have not yet seen a satisfactory 200-volt switch to work as a two-way switch, as required in corridor lighting and staircase lighting, where it is desired to operate the lamps from either end of their circuit. The existing corridor switches are quite unsuitable for houses supplied at 200 volts; or, rather, 200 volts is an unsuitable voltage for houses so fitted. One difficulty is hardly mentioned at all by Mr. Byng in his otherwise admirable paper, namely, the troubles that arise in the use of flexibles. Is there a flexible twin wire in the market that can be put up safely at 200 volts? We ought to have something better in the way of flexibles than can at present be obtained, before we are compelled to adopt a 200-volt pressure in our houses. It is really unsafe for flexibles. Then, to go rapidly on to the question of lamp construction, I differ from my friend Mr. Swinburne on one point, viz., the question of the effect of the material on the emission of light. I do not agree that, whatever the carbon is, at the same temperature you get the same light. [Mr. Swinburne: "That is precisely what I did *not* say happened."] Because it seems to me that, if we are going to despair of improving our carbon, we are despairing very unnecessarily. Surely we have not got to the end of our resources yet in the improvement of carbon. I saw in my own laboratory nearly two years ago a very remarkable experiment. One of the gentlemen who have spoken to-night came to me to tell me that he had found a way of improving carbon for arc lighting. I took the best carbons I had, which happened to be some Siemens carbons, and gave him three out of half a dozen. In two days he brought them back to me, having subjected them to his special treatment. I tried these carbons in various ways. In one case I slit down lengthways one of my untreated Siemens carbons, and one of his that had been treated; clamped a half of each

Prof. Thompson.

kind together, bound them together with wire, and inserted this compound pencil in the arc lamp. I then measured photometrically, by projection, the brightness of the crater. In this test I was operating with the same carbon, the same current, and the same time; the crater being divided across diametrally. The half that had been treated by Mr. Gaster gave out 20 or 25 per cent. more light than the one that had not been treated; the intrinsic luminosity per square millimetre being decidedly higher. I have never since that day despaired of people not being able to produce a carbon which should have a higher luminous emissivity than the ordinary carbon by some method of treating, and I believe that such methods will be equally applicable to incandescent as to arc lamps.

Lastly, Mr. Byng touches on a very important question, viz., the possibility of getting a lamp of higher luminosity by coating a filament with one of those materials, such as erbia, thoria, and other rare earths, which—certainly in the case of erbia—have the property of emitting light of a higher quality in proportion to the temperature. That is a matter on which some years ago I made some experiments in conjunction with Sir William Crookes. They were carried out in his laboratory. We tried coating filaments with every substance we could get hold of, with the idea of heating up the materials internally. In the Welsbach incandescent gas light the earthy materials are heated by the gas from outside. In these experiments we heated internally, by the electric current, lime, magnesia, zirconia, and even the rare material yttria, which Sir William Crookes found to be so especially luminescent in his vacuum tubes. But the experiments were fruitless. The physical phenomena of the extra emission by these materials when exposed in a Bunsen flame of gas and air are not reproduced in the vacuum of the incandescent lamp. And, what is more, we were unable to find any material whatever, however refractory, which at the temperature of the carbon filament and in a vacuum was not either volatilised, or else reduced by the reducing action of the carbon and then volatilised. So that it seems to me that this suggested invention is not physically a practicable one. One is sorry to dissipate what seems to be a very ingenious suggestion—

Prof. Thompson.

a dream of a future lamp which, by the use of the rare earths, might have higher candle-power, and higher economy, for a given expenditure of energy; but apparently the chemistry of the incandescent lamp is so different that there seems to be little chance of success in that direction. If we want to stimulate yttria or zirconia into giving us its extra luminosity in a lamp, we must not proceed by the crude and brutal process of simply raising its temperature. Rather should we seek some more refined method of stimulation, as by the impact of kathode rays, thus producing without wasteful production of heat a true luminesence lamp.

Mr. Grimshaw.

Mr. C. O. GRIMSHAW: There is only one thing I would like to refer to, and that is the author's remark about 12 per cent. of lamps short-circuiting when they are first put on. The supply company I am connected with are supplying 200-volt lamps, and we have now supplied 40,000 lamps. Out of this number only 2·7 per cent. have gone wrong and given any trouble. I think that is a very different result from the 12 per cent. the author speaks of.

Mr Addenbrooke.

Mr. G. L. ADDENBROOKE: It does not seem to me that the small lamp which some speakers have referred to is impossible. I myself have now some 240-volts 6-candle-power lamps, which have been given me by Mr. Stearn. Unfortunately, I have not got a 240-volt circuit to try them properly on, but I see no reason why they should not do very well, and I think that there is every prospect of their being perfectly usable. The ability to use lamps of as small candle-power as that is very important, and I perfectly recognise it, as I did at the time of reading my paper. Nobody ever supposed that 200-volt lamps were going to be at once as good as 100-volt lamps—at any rate, I never did; and I always thought there would be a good many difficulties. But I do not think the difficulties have been as great as they were with the 8-candle lamp when it was first introduced. In the two years, or two and a half years, that have elapsed since the 200-volt lamp has been introduced, it has, I think, made greater progress than the 8-candle lamp did under parallel circumstances 10 years ago; and, considering the talent we have brought to bear on it now,

and the increased resources, it seems to me that there is no reason to despair, and that 200-volt lamps must practically become standards. The advantages are so enormous in the distribution question that for people who do not live in very dense areas where the houses are six stories high, something of that sort is, in my mind, absolutely necessary. There are various large areas where people cannot be properly approached without something of the sort; and if they do not get the greatest perfection, they get a very sound thing, and I think the customers are very well satisfied with it. There are a great number of questions which the paper raises that I should like to have gone into, but I will not detain you further. I may say, with regard to the lamp Mr. Swinton speaks of, two years ago such a lamp was shown to me by a certain gentleman; but, as he showed it me in confidence, I cannot say more. But it is perfectly feasible, though there appear to be some difficulties at present in making it a practicable commercial article.

Mr. Addenbrooke.

At present, in incandescent lamps, the length of filament is considered a disadvantage; but if it can be produced in a corrugated form, such as one or two makers are now introducing, I am inclined to think it may ultimately prove a positive advantage, as the very inartistic and unpleasing red-hot hair-pin look of the ordinary incandescent lamp is got rid of, and when well incandesced the corrugated filament looks like a solid globe of light.

The PRESIDENT: Mr. Byng says that it is the verdict of the *customer* that must determine the question of the advantage or disadvantage of the change from lower to higher voltage. I entirely agree with Mr. Byng in that opinion, and I have no doubt that the customer's verdict will turn on the point of cheapest supply. If higher voltage results in lower price, then higher voltage will be adopted.

The President.

Wherever a large area is supplied from one source, I think that the tendency of the adoption of higher voltage will be to lower the cost of the B.T.U., and therefore to lower the cost of electric light eventually.

Mr. Byng has pointed out the fact that at present the 200-volt

The President. lamps are not as efficient as 100-volt lamps. There is some truth in that statement; but if it is true that the most efficient of the 200-volt lamps now obtainable by the public are not quite as efficient as the most efficient of the lamps for lower voltage, it is also true that the best of the 200-volt lamps in the market are quite equal in efficiency to by far the larger proportion of the lower voltage lamps in general use. Then it is to be remembered that the highest degree of efficiency that has been reached (say 2½ watts per candle as a practical limit for a reasonably long life, and without much fall in efficiency towards the end of the period) has only been reached after long experience in the manufacture of the lower voltage class of lamp. Certainly the 100-volt lamps at the corresponding time after their manufacture began were not more efficient, or better in any way, than 200-volt lamps now are. I believe there is every reason to anticipate an improvement in the 200-volt lamp similar to that which has taken place in the 100-volt lamp.

I cannot agree with Mr. Byng in the assumption that an untreated carbon is necessarily inferior to a treated one. I allow that generally it is inferior, but it would be a great mistake to accept the idea that the primary carbon is not susceptible of further improvement, and that it cannot be made to equal the treated carbon.

I look forward with hope to the gradual and great improvement of 200 volt-lamps.

But, taking them as they are (I mean the best of them), they are sufficiently economical and efficient, considering all the circumstances of central electric supply, to fully justify their more extended use, and the hope that they will ultimately contribute to cheapness of supply.

The points chiefly in my mind in speaking of cheapness of supply are—

1st. The smallness of the works cost of the B.T.U.

2nd. The large additions to that cost consequent on *establishment charges* and the *cost of distribution* incidental to small output.

Establishment charges do not greatly increase with increase

of works output, therefore whatever tends to increase of output tends to lower total cost. The President.

Higher voltage facilitates a larger supply from one centre, and consequently tends to lower cost by spreading the nearly fixed charges over a larger business.

I say advisedly *tends to*, for so far I am not sure that the customer has reaped a very large crop of advantages from higher voltage; but if the costs of production and distribution, all told, are, as I have no doubt they are, reduced as a consequence of the larger extent of the supply from one centre, a reduction of the charge to the customer is certain to follow in the long run, and it will probably amount to more than the small difference in the present efficiency of high- and low-voltage lamps, so far as there is any difference; so that eventually I fully anticipate that the change will be in favour of the customer.

Mr. Mordey has called attention to the difference in the physical condition of treated and untreated carbon filaments. No doubt such a difference as he describes did, as Mr. Stearn also remarked, more or less exist in the earlier days of the lamp manufacture; and the porosity of the carbon was even held by a very high authority to be an advantage—and in one way it is an advantage—though the balance is all the other way. But Mr. Mordey would, I am sure, be surprised to see how greatly the character of the primary carbon of lamp filaments has in late years altered for the better: he would now find, if he examined the untreated filaments of the better made 200-volt lamps, that *there is no sign whatever of porosity*; the fracture is like the fracture of glass. As to the relation of specific gravity to conductivity of carbon, no doubt the carbon of higher specific gravity has—as a rule—higher conductivity, but in a much greater ratio. Electric arc-light carbon has approximately one-half the conductivity of graphite, but its specific gravity is very little less, and in some cases it is not any less.

There is not time to comment on the other points of Mr. Byng's paper, and there is the less need, as they have been already amply discussed. I will only add a word on the general question of the construction of fuses and switches for high voltage.

Mr. Hammond.

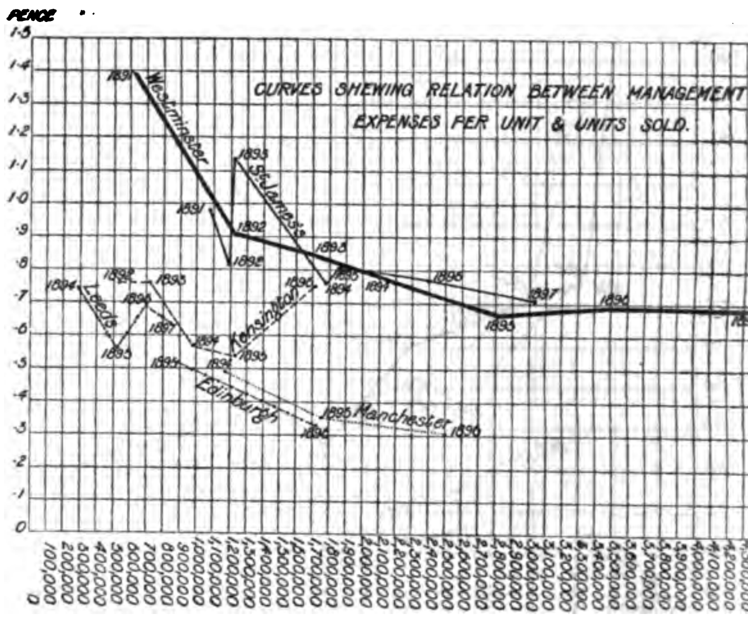
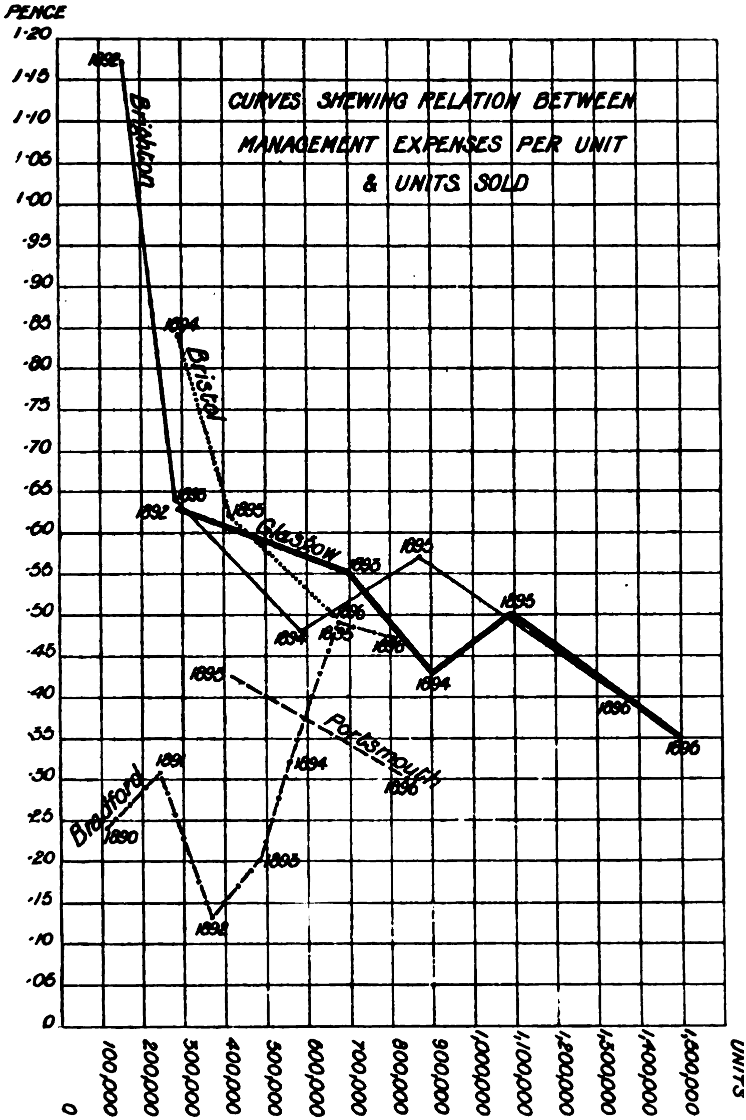


DIAGRAM X. (*a*).—MANAGEMENT.



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The President. In making a change such as we have been discussing it is of the very greatest moment that everything be done that can be done to ensure that the risk of fire shall not be sensibly increased—that suitable extra safeguards should be adopted to ensure it. As the nature of these safeguards is largely a matter for special study and for experiment, I think that an opportunity is presented to our younger members for taking the matter up in a scientific spirit and in a thorough manner, thereby doing an extremely useful piece of work, of which I am sure the members of the Institute would be glad to hear.

[*Communicated.*]—If time had permitted, I should, in the discussion of Mr. Byng's paper, have made a comment on Mr. Robertson's observations on the Wynne-Powell filament process, and what I should have said then I say now as an addition to the remarks I made.

Mr. Robertson spoke of the radical and great improvement in the quality of primary carbons that has resulted from the general adoption of that process. I do not question the accuracy of that statement, nor do I wish or intend to detract in the least from the merit due to Messrs. Wynne and Powell, and especially due to Mr. Powell, for having so successfully worked out the squirting process with the cellulose solution they employ—zinc-chloride cellulose solution. But I think I ought to point out that the improvement in question was not so much due to the use of a particular cellulose solution as to the use of a particular *process of filament formation*—a process consisting essentially of *the projection of a cellulose solution from a jet into a coagulating or setting liquid.* There are at least two other cellulose solutions which by this squirting process give filaments of equally good quality to those produced from the zinc-chloride cellulose solution; one of these is the solution of nitro-cellulose in acetic acid. What is essential is *the squirting process,* and that process was used and specified by me in the patent No. 5978 of 1883—a date earlier than the date of Messrs. Wynne and Powell's patent.

The point has the more importance as the same fundamental method of producing a filament has since been rather extensively exploited for the production of a substitute for silk. The exhibit

I made at the Inventions Exhibition of 1885 showed for, I believe, the first time an artificially produced filament that, amongst other uses, could be substituted in textile manufactures for natural filaments. There was shown at the Exhibition a specimen of artificial silk made by the process in question. The President.

Mr. E. KILBURN SCOTT [*communicated*]: It would be desirable if an extra large size of incandescent lamp and fitting was introduced for workshop use. A larger bulb would give more room for the 220-volt filaments, and the glass might then also be blown a little thicker. For special purposes, such as the lighting of mines, flour mills, &c. (where there is a certain element of danger), and in ironworks, &c., where heavy, rough work is carried on, the ordinary incandescent lamp and its fitting strike one as being just a trifle flimsy. I feel sure that those who use electricity in such situations would be prepared to pay a little more for a larger and *stronger* lamp and fitting. Mr. Scott.

It may be interesting to note that the magnetic blow-out principle, which has been of such incalculable value for preventing arcing in tram-car controllers,circuit-breakers, &c., is now being applied to quite small work, such as the switch and cut-out for the incandescent lamps on tram-cars. There should not be much difficulty in arranging a few turns of wire in an ordinary fuse in such a way that its magnetic polarity would disperse the arc. To show what can be done, it may be mentioned that the General Electric Company's (of America) combined switch and cut-out (costing 4s. 10d.), large enough for all the lighting on a tram-car, occupies only $2\frac{1}{4}$ inches $\times$ $3\frac{3}{8}$ inches $\times$ $1\frac{3}{4}$ inches, and the distance between the fuse terminals is only 1 inch.

Information would be welcome on the composition of the most suitable brass alloys for switch and fuse work; for, without going so far as to say that there is such a thing as a non-arcing metal, yet there is no doubt that some brass or copper alloys arc much more freely than others. Zinc and antimony are objectionable elements in a copper alloy from the point of view of conductance, but there is no doubt that under certain conditions they do tend to stop injurious arcing. The subject is one on which some further information would be useful.

Mr. Rose Mr. T. A. Rose [*communicated*]: In noticing the contrast that exists between unflashed and flashed filaments, Mr. Byng rightly remarks that there is "a far more rapid falling off of "candle-power during life with the former than with the latter." The amount of this reduction of candle-power, however, entirely depends on the quality of the carbon employed, and I certainly think that Mr. Byng's figure of 42 per cent. as the average loss of candle-power in 600 hours is far too high for the best unflashed filaments that can now be made. With further reference to this point, Mr. Robertson stated that all the filaments now manufactured, both in Europe and America, with only two exceptions, are made by the zinc-chloride process, and that, therefore, they are practically of the same quality. I fail to see on what grounds such a far-reaching statement can be made; and even if it be a fact that the filaments are made by the same general process, the quality of the carbons so obtained varies very considerably, and depends on the technical details of the process, and also on the system of carbonisation employed. This, I think, is proved by reference to Mr. Preece's memorable paper on "Electric Glow-Lamp Tests," read before the British Association in 1896, in which his tests on all the obtainable European makes of lamps were recorded. These different makes of lamps were found to vary enormously the percentage drop of candle-power in 1,000 hours, for lamps having an initial efficiency ranging from 3·5 to 4 watts per candle-power varied from 13·1 per cent. to 61·9 per cent. If this enormous difference is found in flashed filaments, it is equally certain that there is a large variation in the quality of the unflashed carbons employed, even if these are all made by the chloride of zinc process. Mr. Byng's statement that the present practice of lamp makers is to vary the efficiency with the voltage is quite new to me. In my own company we always make and supply lamps for whatever efficiency they are ordered, quite irrespective of the voltage. This, I am sure, is the only right method, although it may add somewhat to the cost of manufacture.

·kes. Mr. A. H. Dykes [*communicated*]: There can be no doubt that the greatest difficulty in connection with the change from

100 to 200-230 volts is with the lamps, and Mr. Byng's paper is valuable not only on account of the information he has himself brought forward, but also on account of that elicited from other lamp makers during the discussion. Mr. Dykes.

In discussing the general question of 200–230-volt supply, two distinct sets of conditions have to be borne in mind, and this accounts to a large extent for the very divergent views expressed on the subject.

In the case of already existing companies, more particularly in London residential districts, there is no doubt that, to begin with, the change involves many disadvantages to the consumer. From actual accounts which I have examined, I fear there can be no doubt that at the same price per unit the cost of current is at least 12–15 per cent. higher, due to the lower efficiency of the high-voltage lamps, and to the fact that 2½-, 5-, and 8-candle-power lamps are not available. There is also the trouble—a very real one in many large houses—of dealing with the large number of small-bulb and fancy lamps of low candle-power, and with lamps in horizontal positions. Has any maker yet been able to produce a 220-volt lamp suitable for use in a single candlestick? These are small matters, it is true, but in the opinion of consumers, especially of ladies—who, after all, have usually the last word to say on light in the household—they are very important.

These difficulties can in some cases be met by the offer of a substantial reduction in price, which the companies could well afford to make. A client of ours sent us a circular he had received in which he was asked to allow the change, the company offering to supply him with fresh lamps *free* (most of those he had were nearly new), and to give him good regulation (which they are supposed to do in any case), but could offer no reduction in price, on account of the expense to themselves involved in the change! He did not seem to consider the inducement sufficient.

In the case of new stations there is, I believe, little to be feared and much to be gained. Not having any previous accounts with which to compare their consumption, consumers do not complain, and the life of the lamps is gradually being improved;

Mr. Dykes. although, personally, I have not found them quite so perfect as Mr. Shoolbred, for one, appears to have done.

Turning to the question of switches, it must be remembered that length of break is not everything: the arrangement of the break and the material of the contacts are important factors. If, for instance, the switch be mounted, as it very often is, by careless wiremen, so that the contacts are below, the arc will flame up and bridge across. The same thing happens with fuses when one terminal is mounted vertically above the other. Copper contacts, again, are much less liable to flame than brass, in which metal the low melting point alloys volatilise out and feed the arc. Contacts also should have a fair amount of metal in them; a copper bridge-piece of ample section, with heavy brass block terminals, giving the best result in my own experience. The principle of the fuse introduced by the author seems distinctly good. For distribution fuse-boards the draw-fuse pattern seems preferable, especially with 200 volts. I have every sympathy with one of the public attempting to thread the fuse wire through the small holes in the side walls, and connect it up to a live fuse bar, especially if there is much leakage on the circuit.

I would most strongly urge the necessity for abolishing the use of flexible cord for wiring brackets and electroliers. Flexible cord on pendants and portable lamps should also be renewed occasionally—say every four years. Up till recently we specified vulcanised rubber flexible, but, owing to the large number of cases where the rubber has perished—often before it was actually in use—and its liability to crack, we have been obliged to return to pure rubber. I would also suggest 3/22's should be the smallest conductors, and no single wires should be used, on account of their liability to break if the wire is nicked. This implies that all terminals should be barrel or solid block, with pinch screws.

The saving in cost of wiring is, I think, greatly overrated. To begin with, the actual cost of the wire is only a small fraction of the total cost of the installation, and even then the section of the wires cannot be reduced all round. In running to switches and single lamps the same size of wire would be used with 200 as with 100 volts, and these small wires aggregate a much greater

length than the main cables, in which possibly some saving can be effected. The number of lights it is advisable, from the point of view of convenience, to place on each fuse, more than the current taken by each lamp, determines the number of sub-circuits. Mr. Dykes.

Mr. W. C. GOODCHILD [*communicated*]: Some remarks on the subject of lamp-holders from one who has the maintenance of several hundred may not be out of place. Mr. Goodchild.

I differ from Mr. Byng in his opinion that "the simplest holder ". . . is undoubtedly the Edison screw;" neither does the proportion of each now in use uphold his opinion.

In the first place, such holders are weak mechanically, except when used on a pendant cord and without a shade, which is seldom the case. When they are used on fittings in an inclined position, the covers are often causing "shorts" or "earths." The holes at the tops of shades (being generally for B.C. holders) are not made large enough to take the porcelain rings properly, and when a shade is replaced after cleaning, the cover may fall on the terminals and a "short" ensue the first time the lamp is switched on.

The spun brass screw is insufficient to support the weight of an inclined shade, and its screw-holes tear out from weakness, or the porcelain ring breaks.

In my opinion all shades should have open metal carriers, so that they can be removed for cleaning without disturbing the lamp. When no carrier is used the closed top of a shade confines the heat from the lamp, and soon loosens the plaster in the cap. The result is, many caps are screwed off the bulbs when being replaced after cleaning, especially if the screws are a good fit; if they are not, the cap will break contact on cooling, if 32-candle-power lamps are used, and have to be screwed up before lighting. Some four or five years ago I had to replace a dozen E.S. holders with B.C. ones for this reason.

One of the best all-round holders was that used in the early days with series lamps taking 7 amperes.

This had a small central screw in the holder, fitting into a corresponding socket in the lamp cap. The other pole was a concentric ring round the socket. This was strong mechanically,

Mr. Goodchild

and efficient electrically, even when used on 1,500-volt series circuits; and I see no reason why it could not be adapted for ordinary 110-volt or 220-volt lamps of the present day, and still be little larger than the E.S. holder.

Mr. Sydney Baynes.

Mr. SYDNEY BAYNES [*communicated*]: I regret that, owing to our protracted Committee meetings, which are synchronous with the meetings of the Institution, I have been unable personally to take part in this discussion.

The St. Pancras generating station in Camden Town, which was originally designed as a five-wire system, has been operated as a three-wire system, at 440 volts on the outers, with 220 at consumers' terminals, since the latter end of 1895, and was the first in this country working under these conditions. The installation has run smoothly and satisfactorily, and has presented not the slightest difficulty, accident, or inconvenience of any kind. Nothing is easier than to conjure up critical situations, disasters from fire, &c. This type of prophecy, "sounding notes of warning," was largely indulged in at the outset, but practice has shown that these alarms were quite uncalled for.

Mr. Geipel's remark that station engineers were not responsible for the change, shows that he is not conversant with the early history of the subject. Instead of the lamp makers urging it, as he states, in my capacity of station engineer I did the urging, writing frequently to the leading lamp makers, pointing out the vast importance of the change to the electrical industry, and begging them to combine with the station engineers for their mutual advantage. For a considerable period the chief trouble lay in procuring lamps in any adequate quantity, so that after changing over a number of consumers I had to ask them to burn 115-volt lamps, as before, until a further consignment could be obtained; and I can assure Mr. Geipel that during this time, working single-handed, and without any co-operation, I had a somewhat uphill task.

With regard to the alleged short-circuiting between the terminals inside the bulbs of the lamps, this may, and doubtless does, occur in inferior lamps, but there are some—notably the

Steam lamp—in which I have never had a solitary instance of such short-circuiting; it follows, therefore, that this defect is a detail in the manufacture which can, with the required experience, be avoided. Mr. Baynes.

The double-arc lamp was mentioned by me at the outset as an obvious method of using the open type, but the question of arc lamps applicable to a 220-volt supply is gradually solving itself in this way: In consequence of the trouble involved in the frequent carboning of the open-arc lamp—too often with poor results, due to the inexpertness of the *employés* who handle them—many firms have become so discouraged with their arcs that they discontinue their use and finally remove them; on the other hand, with the closed type, which require carboning at long intervals, they are able, at a reasonable expense, to enter into an arrangement with the contractor to re-carbon and keep the lamps in order, with results highly satisfactory both to the consumer and the supply company.

Mr. F. HARRISON [*communicated*]: In the main I agree with Mr. Byng's paper, but there are a few instances in which I differ from him. Mr. Harrison.

When speaking of unflashed carbon filaments of incandescent electric lamps it should be understood that there are many qualities, and I cannot agree with the author in condemning them generally, but rather follow Dr. S. P. Thompson's opinion that we are not yet at the end of our carbon experiments, whether unflashed or otherwise; for most decidedly the 200-volt lamp has improved very considerably, and many difficulties have been overcome during the two years of experiments. I would draw attention to the fact that in 1889 the Anglo-American Brush Company made a very good lamp with unflashed filaments at their works in Lambeth, owing to the injunction granted against them to restrain them from infringing the flashing patents held by the Edison & Swan Company.

Flashing may be carried on at from 1½ to 6 watts per candle-power, the difference in the deposits being very marked, whether flashed in gas or in vacuo; the specific resistance varying through a much larger range in one direction, and less in the other, than that stated in the paper.

Mr. Harrison.

I find that a lamp with an unflashed carbon filament requires to be the more carefully produced, especially while on the vacuum pumps, where the running up must be very slow. If this is done quickly, it results in the gases remaining in the bulb during a longer period while the filament is being incandesced, owing to the pump not carrying them away as fast as they are driven off from the filament; and, in nearly all cases it is here that the destruction of the untreated filament commences. To produce a good lamp with an unflashed filament, it must be subjected, while on the pump, to a running up to about 2 watts per candle-power. A lamp so made will not blacken, and will have a life of 1,000 hours with a very small variation in candle-power. I admit such a lamp is more difficult to make than one with flashed carbon, but, for all that, it is as good.

I find that the quantity of carbon (Crystal Electric Lamp Company's process) required to produce 16 candle-power at 100 volts at an efficiency of $3\frac{1}{2}$ watts per candle-power, unflashed, is 0·00405 gramme, 140 mm. long × 0·135 mm. in diameter; the surface being 59·374 sq. mm., or 3·711 sq. mm. per candle-power; having a specific resistance of 3,800 microhms per cb. cm., and a specific gravity of 2·02023. Flash this same filament in vacua with hydrocarbon vapour to 50 per cent. of its resistance, and we get a change as follows:—

——	Unflashed.	Flashed.	Difference.
Voltage	100	70	30
Candle-power at $3\frac{1}{2}$ watts ...	16	$13\frac{1}{2}$	$2\frac{1}{2}$
Diameter ... ... mm.	0·135	0·1575	0·0225
Length ... . ,,	140	140	*Nil*
Specific resistance microhms	3,800	2,720	1,080
Specific gravity	2 02023	1·70302	0·31721
Weight ... ... gramme	0·00405	0·00453	0·00048
Surface per candle-power, sq. mm.	3·711	5·115	1·404
Total surface ,,	59·374	69·0524	9·678
Resistance (cold) ... .. ohms	380	190	190

Thus it will be seen that, whilst it takes 0·00405 gramme of

unflashed carbon to give 16 candle-power at the said voltage and efficiency, it takes 0·0051 gramme of carbon flashed to 50 per cent. to give a like result. Mr. Harrison.

Now, on this basis, is it possible that equal work is being done by these two unequal quantities of carbon when heated to precisely the same temperature? I find, on testing, a difference of colour between them which, I take it, tends to disprove the general idea that efficiency is temperature in all forms of carbon; but with any one particular class of carbon I agree that it is so.

I do not agree that all unflashed carbon filaments are the more volatile, or that flashed carbon is more dense. Mr. Byng states that a microscopical examination of an unflashed filament after 100 hours' run showed a roughened surface. True; but more information is required before forming an opinion. What degree of vacuum was there in the lamp when the filament was taken out? I have a very strong opinion that the unflashed filament was in a very indifferent vacuum. This same effect can be produced in two hours with an unflashed or flashed filament. It simply means either a poorly constructed filament or bad exhaustion.

I carried out the following experiment:—A flashed filament and an unflashed one were put together in a bulb, each filament giving 16 candle-power at 100 volts and 3½ watts, and coupled them in parallel so that both should be under the same conditions in every respect, and overran the lamp for 50 hours on a continuous circuit of 130 volts. I then gradually ran them up. The unflashed carbon broke at 200 volts, and the flashed at 220 volts.

The unflashed filament is more expensive and difficult to make. The chief objectionable feature is that, when two are in series in one bulb, the efficiencies may differ slightly (in brilliancy or watts per candle-power). A small percentage of lamps of this kind will occur, and if allowed to go out on a varying circuit a short life will be the result. I use up this class for lighting our factory.

From a lamp manufacturer's point of view, with the present prices, I quite agree with Mr. Byng that a flashed filament is

Mr. Harrison. undoubtedly the most satisfactory, but disagree with his statement that two treated or flashed filaments cannot be sealed into an ordinary-sized bulb with safety. I have proved that this can be done, and, as a fact, after many tests and experiments, my company (Crystal Electric Lamp) have adopted this method for all sizes except 5-candle-power and under; but experiments in hand look very promising for even 5-candle-power high-voltage lamps being made with treated filaments. Of course, the greater the candle-power the easier this kind of lamp is to make.

I was certainly surprised to hear that it was the author's experience to have such a large percentage of immediate breakage on the lamps being put into circuit.

I find miniature fittings are the only kind that are more difficult to attach, owing to the number of leading-in wires and a possible risk of bad insulation.

The electrostatic was the greatest difficulty I had, but I have since overcome it.

Other matters in Mr. Byng's excellent paper I leave to those more experienced in their use.

Mr. Byng. Mr. Binswanger Byng, in reply, said: I am sorry it is so late, and that, therefore, by the request of our President, I have to condense my remarks. I think it would be more expeditious if I were to reply under the headings which I have laid down in my paper—viz., Lamps, Fuses, and Arc Lamps—rather than in the sequence of the speakers' remarks, especially as many of them do not call for a reply. With regard to Incandescent Lamps, it must have been gratifying to us that our President, Mr. Swan, and Mr. Stearn, the two true inventors and pioneers of incandescent lamps, have taken part in the discussion to-night. It is about 20 years ago, almost to the day, when Mr. Swan first put down that famous gas engine to test and experiment with the first carbon filament placed in a glass bulb exhausted of air. Mr. Stearn can also claim to be one of the earliest pioneers of 200-volt incandescent electric lamps. He, following his own motto—very like that valiant knight with whom we are familiar in connection with his advertisement—went immediately into the fray, and said he was not afraid to make 200-volt lamps, and his

criticisms to-night were upon the same lines. In many respects Mr. Stearn's speech brought forward a variety of valuable points. I agree with him that I am not afraid of making, not only 200-volt lamps, but good and efficient 200-volt lamps. If my paper is considered as a criticism on 200-volt lamps—and most speakers seem to have thought that I criticised them adversely—it was done with the object in view, as I explained at the beginning of the paper, of arriving at a higher perfection; and we can only improve if we know, recognise, and acknowledge any defects which may be at present before us. I think this object of my paper has been attained, and some good will result. Although some of Mr. Stearn's remarks may appear to be critical, really they are supplemental to mine, and, in fact, in the most important details confirm them. It is too late to-night to go into those few matters in which we are at variance. I shall elaborate upon them in writing. I wish to mention only that the main point on which there may be some difference between Mr. Stearn and myself refers to the question as to whether my criticism of unflashed filaments was correct or not, or whether I can make out my case that a flashed filament, under the circumstances which I have explained, and in 200-volt lamps, is much better and superior to an unflashed filament. The next point that I wish to discuss is that question which Mr. Stearn touched upon, whether lamp manufacturers should make lamps with a view of sending them out on the same voltage with varying efficiency. I wish to say that the case, as put by Mr. Stearn, was based on a misunderstanding—evidently a printer's error (on page 124) in putting the comma after the word "voltage," instead of after "efficiency." We are asked for different efficiencies at the same voltage, and if we have to make lamps on an economical and commercial basis it is certainly very advantageous to us if we can make them at varying efficiencies. I will only add, as regards lamps, that, although I may have criticised adversely 200-volt lamps, for the reason that I have already given, yet I have done so chiefly because I have examined samples of almost every 200-volt lamp that could be bought in London; and what I have stated is the outcome of my experience in brief. Of course we, at our works Mr. Byng.

Mr. Byng. at Hammersmith, have known these defects and acknowledged them, and we have to a great extent overcome them. We do not despair; in fact, we are certain that we can make lamps of 200 volts of the same efficiency as those of 100 volts. We are now making single-filament lamps which will work at 3½ watts per amyl acetate candle-power for 1,000 hours without losing more than 10 to 15 per cent. of their candle-power. As regards FUSES, I should have liked to expand the subject if circumstances permitted. Mr. Crompton, Mr. Wordingham, and Mr. Miller have spoken about fuses; and, although I am very pleased to have heard their remarks, especially those of Mr. Wordingham, yet I think I must stick to my guns, and say that, from the theoretical point of view, I believe my explanation of the behaviour of the plaster fuse is the correct one. Mr. Wordingham and Mr. Miller have tested them, and found them do the work which I claim in practice. If they do the work, I hold that to be the principal thing. Theory, however, is valuable, and it is extremely important to me and to everyone to know whether such fuses in theory behave in the manner which I have described, or in the almost opposite way which both Mr. Miller and Mr. Wordingham have described.

In respect of that part of my paper which refers to ARC LAMPS, I should like briefly to reply to the remarks of Messrs. Mordey, Crompton, and Professor Thompson. Mr. Mordey said that we must not forget that the efficiency of the work depends largely on, or is proportional to, the watts consumed. This is not an exact statement, as I will endeavour briefly to prove. An arc lamp in action must be looked upon as presenting two distinct phenomena—the crater in which the electrolytic action takes place, and the flame, which is a conductor. The crater gives us almost all the light, the flame gives us no illumination. The crater works with an unvarying E.M.F., to a large extent, whatever the length of the arc may be; and therefore, if we increase the length of the arc, and increase the watts or the E.M.F., the excess of E.M.F. beyond the normal and beyond the point of efficiency is used up entirely in the flame, and, as we get very little illumination by means of the flame, we are losing the extra amount of power (or watts) which we are putting into the arc lamp.

[*Communicated.*]—In accordance with what I have said in my verbal answer, I now add a few remarks which I was unable to make at the meeting, owing to the short time at my disposal. Mr. Bynq.

Incandescent Lamps.

Several speakers, and especially Mr. Grimshaw, have referred to my remark "that the consensus of opinion was that large "numbers of lamps were expected to short-circuit at once."

I referred to this as an expression of some users, and I must say that it is a somewhat exaggerated expression. I am sorry that it has been the main cause of creating an impression that I had condemned the quality of the present-day 200-volt lamps. I wish to say that it was far from my intention to do anything of the kind. My paper was expressly written with a view to showing that in certain particulars 200-volt lamps are capable of improvement; and, in order to induce manufacturers to take those improvements in hand, I had necessarily to speak of defects, and leave the good qualities out as superfluous. Hence the impression created. At the same time, the discussion itself clearly proves that there were two distinct currents of opinion in reference to the behaviour of most of the present-day 200-volt lamps; the one opinion being expressed mainly by central station engineers, and the other mainly by users or contractors, who have direct dealings with users. This, however, I believe, is a matter of the past, and I, in conjunction with other English manufacturers, may claim to have reduced this sudden short-circuit to an almost negligible quantity; and when the improvements I advocate are adopted, this defect will entirely disappear.

Mr. Stearn took some pains to point out all the virtues of unflashed (or untreated) carbons. I must say that the same virtues as are possessed by unflashed carbons are also possessed, together with additional ones, by flashed carbons.

He says that unflashed carbons are best for lamps classed as belonging to a transitional period. Now my paper was given with a view that such transitional period should speedily come to an end. He also admits the superiority of treated carbons in respect to the light curve. In saying this he really admits almost everything

Mr. Byng. that I pointed out as being the advantages attainable with a flashed filament. Here lies the crux of the matter, because in losing its candle-power during its life, the lamp also loses its efficiency. This is my last contention, and is therefore supported by what Mr. Stearn himself says.

The "Robertson" Lamp Company are now adapting flashed filaments to all types of high-voltage lamps, and I am well satisfied with the results, as they compare favourably with 100-volt lamps as regards retention of candle-power and efficiency; in fact, they are now identical with 100-volt lamps. These lamps also, having only one filament, cannot get entangled in transit, and therefore cannot have the defects of entanglement which Mr. Stearn pointed out that some two-filament lamps are liable to. Mr. Stearn also said that I had proposed to put the filaments further apart. This is not the case, since I only referred to the desirability of increasing the distance apart of the leading-in wires in the cap, and of thus obtaining better insulation for lamps, more especially when installed in damp places.

As for occluded gases in untreated filaments, this is a technical question; and if Mr. Stearn has no trouble *after* having heated the filament to white heat (which I can quite conceive), he will certainly get it *before* he arrives at this point, even with the most careful pumpers, as is suggested as being necessary by his further remark that "if *proper* precautions were taken in "exhausting the lamps——."

He also refers to the efficiency varying with the voltage, but I must repeat that he has entirely misunderstood me owing to a printer's error. Again I draw his attention to this, and if he will put the comma after the word "efficiency," instead of after "voltage," he will find that we are not at variance on this point, and I am assured on re-perusal that Mr. Stearn will see that I do not advocate varying the efficiency with the voltage.

With reference to my remark on combination filaments, Mr. Miller made some interesting statements about a new combination filament he had been trying. As this matter is of great interest, I should like Mr. Miller to give us some further details of the lines npon which these filaments are being made; and whether

they are oxides of the rarer earths, or are some new body or combination. What I have said as to combination filaments is based upon many experiments carried on for many years by Mr. Robertson and many other experts with whom I am acquainted. There are possibilities, I concede, of others being more successful —in fact, we have heard many rumours lately of such achievement. In view of the great importance of this matter, let us hope that the public will soon be enlightened. Mr. Byng.

Fuses.

The importance of the fuse question has been recognised by several speakers. Considering the millions of fuses now in operation, that they are really the weakest part of an electric light installation, and that they are to be constructed so as to be left largely under the control of the non-technical user, there is no doubt that special attention should be given to their construction, and that their behaviour when breaking should be investigated on a scientific basis; as, until we accurately know what is the reason of an arc being destructive, we can never make an instrument that is thoroughly reliable to prevent such destruction.

Mr. Wordingham, of Manchester, has made some interesting remarks upon fuses, in which he takes particular interest. He finds it necessary to apply a much higher test to fuses of 200-volt circuit than to those passed for 100-volt circuit. He acknowledges that his test is a very destructive one. The plaster-lined fuse box which I have described will stand this test. As Mr. Wordingham says, "it acts perfectly, and is the best fuse he has "seen." This is satisfactory, but I am not satisfied with the explanation which he advances as to the cause of the behaviour of this fuse. His impression is "that a good part of its working so "well is due to the air being driven out of the holes and blowing "the arc off the terminals."

Here I may mention that Mr. Miller also supports this view of the blowing effect through the two side holes, and he is of opinion that if these holes were stopped up, and the fuse subjected to short-circuit of 200 volts, the cover would be blown to pieces. Now I have made this experiment, and the fuse broke

Mr. Byng. the circuit without breaking the cover; in fact, the action of the fuse was the same in every respect as when the two side holes were left unstopped.

Judging from this experiment, and other reasons, I am of opinion that the blowing effect, if such exists, has nothing to do with the action of my plaster-lined cut-outs. To my mind the explanation is quite simple. It is a question mainly of preventing the arc from feeding upon the metallic vapours. The arc in itself is not destructive, or not sufficiently destructive to break the china or melt the terminals. It becomes destructive when it has spread over the conductive path created by the volatilisation of the tin wire. Only a fraction of this wire can volatilise in the plaster chamber, and the arc cannot spread to the terminal.

Another important matter, which it would be extremely useful to have thoroughly sifted, is whether the breaking of the china cover and other damage done to cut-outs by the breaking of the fuse is mainly due to sudden expansion of air. This view seems to be very prevalent, and both Mr. Crompton and Mr. Miller gave expression to it.

There is no doubt that some expansion of air occurs, but my experiments have tended to prove that this expansion is in no case sufficiently powerful to break any china or wood cover. I find it is entirely caused by excessive heat, and consequent cracking of the china. I am now engaged upon a further series of experiments on this question, which I hope to make public, as I consider that no perfect fuse based upon scientific principles can be made unless this question of destructive expansion of air is laid at rest.

Mr. Crompton also makes the remark that in his opinion the action of the plaster upon the arc may be due to chemical work done by the arc on the plaster or cement. This is a new idea, which I must confess never struck me; but I may say that I have used other material for lining, such as asbestos, mica, &c., with equally good results. Mr. Crompton's theory of the cooling effect upon the arc through chemical work being done upon plaster could hardly be extended, I think, to asbestos or mica.

Arc Lamps.

Mr. Byng.

I have only to reply to remarks made by Mr. Mordey and Mr. Crompton on the *cut-out* which I described, and which I said was necessary to use in lamps in series on the 200-volt circuit.

Mr. Mordey asserted that the old Brush cut-out answered all the purposes which I considered necessary. I must disagree with him. The conditions under which the Brush cut-out is used are altogether different to those with arc lamps in series on a 200-volts circuit. It was originally designed for a constant-current circuit, and, unless used in conjunction with an equivalent resistance, is useless on constant-potential circuits—especially on short series of four or five lamps, where the short-circuiting of one means a large increase of voltage across the remaining lamps. It is very evident that, under such conditions, either an equivalent resistance must be thrown in, or the circuit must be broken entirely. Whatever arrangement is used must be controlled either by the amount of current passing through the series of lamps or by the voltage across individual lamps. The first would be uncertain, especially when the lamps are switched on, when "pumping" will produce all effects that are produced during the working of the lamp. When the voltage across the lamp is used to control the working of the cut-out, it must be so arranged that it cannot possibly leave the mechanism of the lamp in such a way that the carbons are held apart, because when the circuit is broken entirely neither series nor shunt winding will be of use to pull the carbons together. Overcoming this difficulty is the chief novelty in my new cut-out.

Mr. Crompton thinks that I have made too long a description of supposed difficulties of working lamps in series on the 200-volt system. The difficulties I referred to are the possibilities of burning shunts and carbon holders. No one can gainsay that they do exist, and that we must provide against such emergencies; and Mr. Crompton's own instances prove that he himself has provided against them. He refers to the Liverpool Street Station installation, where four lamps burn in series on 200 volts. Judging from the appearance of these lamps in that station, and the large box on each, I have no doubt that every lamp contains

Mr. Byng. an equivalent resistance, with a necessary switch. Of course all this is provided to save the shunt, and therefore I am astonished to hear that some shunts still burn out during the year. I hold that this use of equivalent resistance is not always to be recommended, and I will quote from my paper: "Of course we "can instal a cut-out and equivalent resistance to each lamp. "But this expedient is very costly, and presents the further "difficulty of finding suitable room near the lamp," &c.

In the case of large installations in places like railway stations, such resistances may be neither too large nor too expensive. I contend that they are not suitable for many places like shops, &c., where a few lamps are wanted, and for these I have designed the cut-out and cut-in which I brought before the Institution in the course of my paper.

The PRESIDENT: I propose that the best thanks of the meeting be given to Mr. Binswanger Byng for his most useful paper.

The resolution was carried by acclamation.

The PRESIDENT: I have to announce that the scrutineers report the following candidates to have been duly elected:—

Associates:

George Balfour.	Roland Marshall.
Edward Hobart Burgess.	Lieut. C. M. Playfair, R.A.
Thomas Philip Edward Butt.	M. Railing.
William Rowland Edwards.	Thomas Edward Ritchie.
John Edward Elliott.	B. Surdar Singh.
Harry Percy Girling.	Herbert W. Watts.
Richard Lund.	James Michael Graham Wilson.

Students:

James John Chapman.	William Gilmour Laird.
John Francis Henderson.	William Allen Turquand.
Francis Henry Hutton, B.A.	Edgar Benson Ward.

The meeting then adjourned.

ORIGINAL COMMUNICATION.

SPARKLESS REVERSAL IN DYNAMOS.

By H. N. ALLEN, B.Sc.

INTRODUCTION.

Since the output of a large dynamo is limited by the occurrence of sparking, it is a matter of considerable practical importance to determine the conditions on which this depends. If the armature coil were entirely surrounded by air, or some other non-conducting material, the energy wasted in each spark would be proportional to $L\,C^2$, where L is the inductance of the coil, and C the change of current which takes place during the spark. The inductance of an armature coil is thus a quantity of some practical importance, but, as it is small, it is not easy to determine experimentally. The approximate theoretical value can be determined by the method given by Swinburne in his paper on "The Theory of Armature Reaction in Dynamos and "Motors," read before the Institute of Electrical Engineers in 1890 (*Journal*, vol. xix., p. 90). It is, however, difficult to form a clear mental conception of the varying distribution of the tubes of magnetic induction when the current varies rapidly; and, as the matter did not appear to have been investigated experimentally, it was decided to devote some attention to it, with the view of finding some of the factors on which the inductance depends, and to see how nearly the results obtained by experiment would agree with the theory.

Throughout the investigation the writer was indebted to Professor W. E. Ayrton, F.R.S., in whose laboratory the experiments were made, and to Messrs. T. Mather and A. H. Allen, his assistants, for their sympathetic interest and help.

Two methods were made use of to determine the inductance. In the first, the impedance of the coil was measured by observing the virtual volts between the terminals when an alternating

current of measured magnitude and frequency was sent through it. The resistance of the coil having been determined, its reactance and inductance could be calculated. The second method was to make use of the secohmmeter, and to balance the inductance of the coil, which was placed in one of the arms of a Wheatstone's bridge, by means of an adjustable standard of inductance in the neighbouring arm.

It was not considered desirable to disconnect one of the armature coils from the commutator for these experiments, but the inductance of a single coil of a drum armature can be determined fairly accurately with the secohmmeter without doing this, by making connections with two contiguous commutator segments. With a simple closed-coil drum armature this gives a circuit, consisting of a single armature section, shunted by all the other sections in series. The impedance of the latter branch is so great, as compared with that of the former, that the impedance of the combination will be nearly equal to that of a single coil. It was found that, with the highest frequency which could be made use of, the resistance of an armature coil was too large, as compared with the reactance, to give very satisfactory results by the alternating-current method. The experiments were therefore conducted with artificial armature coils, of thicker wire, wound round the armature core. In the first experiments they were outside the ordinary winding, so that there was a space between them and the iron core. Afterwards a special cylindrical core was constructed of thin soft iron ring discs, threaded on a wooden spindle, so that the wires could be brought close up to the iron. The dimensions of this core were as nearly as possible those of the actual armature core of the small series-wound Immisch motor which was experimented on.

Measurement of Inductance with Alternating Currents.

The virtual volts between the terminals of the armature coil when an alternating current flowed through it were measured by a reflecting electro-dynamometer, with its fixed and moving coils in series. The moving coil was suspended by means of a phosphor-bronze strip, the other connection being made by a

spiral of strip at the bottom. It was damped by means of a platinum vane, which dipped into a mixture of paraffin and engine oil of suitable consistency. Originally this vane dipped in mercury, and the lower connection was made in this way. This, however, proved very unreliable: the surface of the mercury rapidly became dirty, and the zero of the instrument most irregular. The inductance of the voltmeter was so small that the impedance did not differ from the resistance by a measurable amount at the highest frequency used in the experiments.

Inductance with the Coil in the Median Plane.

Table I. shows the results of measures made on five different coils. The coil was in each case in the median plane of the motor, which is marked A A in Fig. 1, where A B A B represents

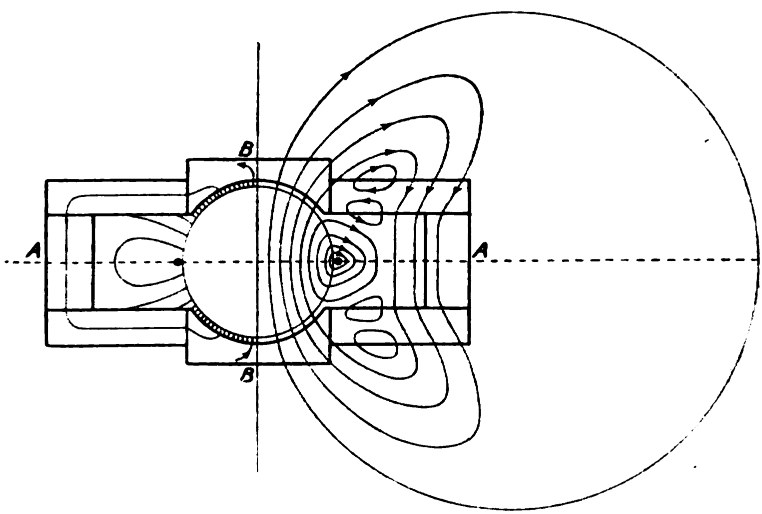

Fig. 1.—Left: Magnetic Field due to Continuous Current. Right: Magnetic Field due to Alternating Current.

the motor frame. The coils marked A, B, and C, in Table I., were of No. 12 wire, A being more loosely wound than B. Coil D was of square copper rod 0·2 inch thick; E was made of the same rod hammered into a rectangular section, the sides being 0·23 inch by 0·165 inch. Coils D and E were used on the model armature core, so that the conductor was close to the iron.

Table I.

Coil.	Turns.	Condition.	R.P.M.	Inductance. Henrys.	Inductance divided by Square of Number of Turns.
A	6	Field open	1,500	0·0000493	0·00000137
„	6	„	1,100	0·0000537	0·00000149
„	6	„	700	0·0000648	0·00000180
„	6	Field closed	1,500	0·0000278	0.00000077
„	6	„	1,100	0·0000280	0·00000078
„	6	„	700	0·0000297	0·00000083
B	6	Field open	1,500	0·0000563	0·00000156
„	6	„	1,100	0·0000622	0·00000173
„	6	„	700	0·0000699	0·00000194
„	6	Field closed	1,500	0·0000333	0·00000093
„	6	„	1,100	0·0000339	0·00000094
„	6	„	700	0·0000297	0·00000082
„	6	On core	1,500	0·0000309	0·00000086
„	6	„	1,100	0·0000333	0·00000092
„	6	„	700	0·0000338	0·00000094
„	6	No iron	1,500	0·0000201	0·00000056
„	6	„	1,100	0·0000182	0·00000051
C	11	Field open	1,500	0·000151	0·00000125
„	11	Field closed	1,500	0·000076	0·00000063
„	11	On core	1,500	0·000077	0·00000063
„	11	No iron	1,540	0·000054	0·00000045
D (114 amperes)	1	Field open	1,600	...	0·00000158
„	1	Field closed	1,600	...	0 00000111
„	1	On core	1,600	...	0 00000095
„	1	No iron	1,600	...	0·00000072
D (137 amperes)	1	Field open	1,600	...	0 00000157
„	1	Field closed	1,600	...	0 00000106
„	1	On core	1,600	...	0 00000093
„	1	No iron	1,600	...	0·00000062
E (163 amperes)	1	Field open	1,600	...	0·00000159
„	1	Closed by 0·1 ohm ...	1,600	...	0·00000102
„	1	Closed by thick wire	1,600	...	0·00000104

1,500 R.P.M. correspond to 200 alternations per second.

In order that the values in the last column, for different coils under similar circumstances, may be compared together, they are tabulated in a different form in Table II. for speeds of 1,500 and over.

Table II.

Coil.	Field Open.	Field Closed.	On Core.	No Iron.
A	0·00000137	0·00000077	...	...
B	0·00000156	0·00000093	0·00000086	0·00000056
C	0·00000125	0·00000063	0·00000063	0·00000045
D (114 amperes)	0·00000158	0·00000111	0·00000095	0·00000072
D (137 amperes)	0·00000157	0 00000106	0·00000093	0·00000062
E (163 amperes)	0·00000159	0·00000104	...	...

As will be seen, the observations were made with three of the coils, under four different conditions: 1, The coils on the field magnets were left open; 2, they were short-circuited by a thick wire; 3, the core, with the coil on it, was removed from the field; 4, the coil was removed from the core, so that there was no iron near it.

The second value of the inductance of coil D, without iron, is probably the more correct, as with the larger current the deflection of the voltmeter becomes much greater. The reason why the values for A and C are smaller than the rest is that A being loosely wound, and C consisting of a greater number of turns, a number of lines of force escape being linked with all the turns.

Effect of Frequency on the Apparent Inductance.

If the values obtained with open field circuit at different frequencies are examined, it will be seen that, as the frequency diminishes, the observed inductance increases. At very low frequencies it would no doubt be the same as the true inductance of the coil, which can be obtained by calculation from the reluctance of the magnetic circuit, or by finding the flux through the coil experimentally when a continuous current flows in it. For a coil of six turns the true inductance was found to be about 0·0000835. When the field circuit was closed, the lowest available frequency did not show any increase in the observed inductance. When the frequency is low the impedance and resistance are nearly equal, so that the inductance cannot be obtained with much accuracy. No doubt for very low frequencies the inductance in this case also would approach the value 0·0000835 given above.

Effect of the Short-Circuited Field on the Inductance.

When the field circuit is closed, it acts as the secondary of a transformer. A current is induced in it which may almost entirely neutralise the tendency of the current in the armature coil, or primary, to produce a varying flux through the field magnet. Table III. shows the effect of closing the field circuit through different resistances. The first column gives the resistance in ohms which was connected across the terminals of the field, and the second the volts at the terminals of the armature coils of 11 convolutions, in which a current of 13·7 amperes was flowing. Frequency, 128. Resistance of field coils, 0·143 ohm.

Table III.

Resistance in Ohms used to close the Field Circuit.		Volts at the Terminals of Armature Coil C.
0	.	1·03
0·4	...	1·07
0·8	...	1·11
1·2	..	1·13
1·6	...	1·15
2·0	...	1·17
2·4	...	1·19
6·0	...	1·34
10·0	...	1·48
20·0	...	1·64
30·0	.	1·72
100·0		1·83
Field circuit open	...	1·92

An increase of inductance in the field circuit produced a similar effect to that produced by increased resistance: the volts at the terminals of the armature, required to send a given current through it, were increased. This point was tested by including the secondary of a large transformer in the field circuit. This hardly affected the resistance at all, but the increase of inductance diminished the induced current in the field coils, and increased the apparent inductance of the armature coil, so that the volts rose from 1·1 to 1·15, the deflection of the voltmeter changing from 37 to 42.

Effect of a Current in the Field Coils.

A strong current through the field coils diminished the inductance slightly. The permeability of the iron was diminished, and thus the flux through the armature coil was less. In one experiment 20 amperes sent through the field coils lowered the volts on the armature coil terminals from 1·11 to 1·07, though the resistance in the field circuit was in reality a little larger in the second case, since that of the 12 storage cells used was added, and the field coils were heated by the current.

Effect of the Position of the Coil on the Armature.

Table IV. gives the results of experiments on the inductance of coil E in different positions with respect to the poles. The same current of 163 amperes, with a frequency of 213, was used throughout. Several of the values are the means of a number of observations.

Table IV.

Position of Coil.	Volts on the Terminals of the Armature Coil.			Apparent Inductance. Henrys.		
Median plane	0·371	0·256	0·260	0·00000159	0·00000102	0·00000104
Just outside pole-tips ..	...	0·288	...	...	0·00000119	...
Just under pole-tips ...	0·400	0·331	0·343	0·00000173	0·00000140	0·00000146
$\frac{1}{4}$ in. in from pole-tips .	...	...	0·406	...	...	0·00000178
At right angles to the median plane ...	0·594	0·560	0·584	0·00000266	0·00000251	0·00000261
Resistance in field circuit	∞	0·1 ω	0 ω	∞	0·1 ω	0 ω

It will be seen that when the coil is at right angles to the median plane, in the position marked B B in Fig. 1, the apparent inductance is not appreciably changed by short-circuiting the field, and that the value obtained is not very different from the number 0·00000232 calculated, as is described further on in this paper, from the flux through the air gap produced by a steady current in the armature coil. In obtaining this value a considerable amount of magnetic leakage is neglected. As the coil is moved from the plane B B towards the median plane, A A, the volts on the terminals and the apparent inductance diminish.

This diminution is greatest when the field coils are short-circuited, since the specific resistance of the copper coils is less than that of the iron masses of the field-magnet limbs, and so larger currents are induced in the former than in the latter. It is, of course, these induced currents which tend to check the variation of flux through the field magnet.

Flux through the Magnet Yokes.

A special series of experiments was made to investigate this choking action of the short-circuited field coils on the magnetic flux.

A coil of 12 turns was wound on one of the field-magnet yokes, and was connected to the reflecting voltmeter when the current was flowing in the armature coil in the median plane, the field coils being short-circuited. The result was to show that about 6·5 per cent. of the total flux produced passed through the field yokes. The rest, or 93·5 per cent. of the alternating flux, passed through air in going from one surface of the armature core to the other. Now with a continuous current of the same virtual value the leakage field—that is, the portion of the flux which does not pass through the field magnets—is estimated as only 55, the above total virtual alternating flux through the armature coil being taken as 100. With the alternating current, then, a larger flux passes through the air about the motor, without going through the yoke, than is the case with a continuous current. Fig. 1 is intended to illustrate this. On the left is indicated the field produced by a continuous current in the armature coil, with a large flux round through the field magnets and a small leakage field. The right half of the figure gives some idea of the configuration of the field with an alternating current when the field coils are short-circuited. The effect of the induced current in the field is somewhat exaggerated, so that there is on the whole a flux through the field coils in the opposite direction to that which would be produced by the current in the armature coil. It will be seen, on comparing this with Fig. 3, which represents the field produced by the same continuous current in the coil, when it is on the iron

core outside the field (the thickness of the conductor is greater), that the form of the magnetic field is in a general way similar in the two cases. With the alternating current it is a little distorted; but the effect of this distortion on the magnetic reluctance between the top and bottom of the core is rather more than balanced by the fact that some of the tubes pass for a considerable distance through iron, instead of through air: the apparent inductance is thus a little larger in this case.

Table V. gives the volts observed when two coils, of 12 turns each, were wound on the two field yokes, and connected in series with the voltmeter, an alternating current of constant value being sent through the armature coil.

Table V.

Position of Coil.	Volts induced in Yoke Coils.	
Median plane	1·5	0·166
Just outside pole-tips	...	0·121
Just under pole-tips	1·26	0·111
At right angles to the median plane	0·074	0
Resistance in field circuit	∞	0·1 ohm

The numbers in the first line show the diminution in flux when the field is short-circuited. The vertical columns show that, as the coil is moved under the pole-tips, more and more of the flux passes round through the pole-pieces, without going through the magnet limbs and yokes. When the coil is exactly at right angles to the median plane there will be no flux through the yoke coils.

Inductance of a Ring Winding.

In order to compare the inductance of a ring winding with that of a drum, two bent pieces of copper bar were soldered to the drum coil D. These bars passed through the central hole of the core, the wooden spindle being cut away to allow this, and were of such a shape that they lay close to the iron, from which

they were insulated by tape. In Fig. 2, A B C D E F is the rectangular drum coil. The ends C D and A F were bent upwards, so as to clear the wooden spindle, and allow the coil to be placed on the core, which is shown in section in the figure, in a plane passing through the axis. The two extra bars are indicated by G H B and K L E, and were soldered on at B and E. The terminals of the drum coil are A and F, and of the ring winding G and K.

The ring winding G H B C D E L K represents two coils, of one turn each, on opposite sides of the armature. In an actual dynamo two such coils might be commutated at the same instant, the currents flowing in them being in the same relative directions as in this experimental winding. The following are the values

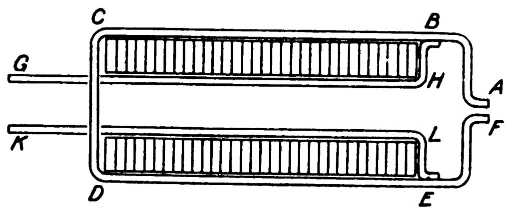

FIG. 2.
The terminals of the drum coil are A and F, and of the ring winding G and K.

obtained for the inductance; the corresponding value for the drum coil, and the difference, being given in each case:—

Table VI.

Condition of Experiment.	Inductance of Ring Winding.	Inductance of Drum Winding.	Difference.
Armature out of field	0·00000139	0·00000093	0·00000046
Field closed by thick wire ...	0·00000145	0·00000104	0·00000041
Field closed by 0·1 ohm ..	0·00000150	0·00000102	0·00000048
Field open	0·00000203	0·00000159	0·00000044

To get some idea of the effect of the rods K L and G H alone—that is to say, of the field in the interior of the armature core—the terminals A and F were soldered together, and the inductance of the circuit G H B A F E L K was determined.

The points B and E were, of course, also connected by B C D E, but the inductance of this is great as compared with that of B A E, so that very little current will flow this way. The value obtained for the inductance was 0·00000072, which is rather larger than the average difference between the inductances of the ring and drum windings. The inductance of B A E is included in this measure, as it is also in the inductance of the drum. If twice the inductance of B A E is subtracted from 0·00000072, the remainder will probably not differ much from 0·00000045, the average difference in Table VI.

The inductance of the ring coil is, in this case, about one and a half times that of the drum. It must be remembered, however, that the core had not the proportions which are usual in a ring armature. The length was 8 inches, the outside diameter 3¾ inches, and the inside diameter 2. In this type of armature the diameter and the length are generally more nearly equal.

Measurement of Inductance with the Secohmmeter.

The inductance of coil C was measured with the secohmmeter, and the following values were obtained. The results of the alternating-current experiments are tabulated in the last column.

Table VII.

Condition of Experiment.	Inductance with Secohmmeter.	Inductance with Alternating Current.
Field open	0·000159 (slow), 0·000132 (fast)	0·0000151
Field closed	0·000082 (slow), 0·000070 (fast)	0·000076
On core	0·000070	0·000077
No iron	0·000051	0·000054

It follows that a very fair approximation to the value of the apparent inductance, at frequencies of about 200, can be obtained by running the secohmmeter at a medium speed. The actual speed of the secohmmeter was not measured, but it was run as slow and as fast as possible by means of a small motor.

The inductance of a number of actual armature coils of different types might easily be measured with the secohmmeter,

as, for example, ring and drum armatures with ordinary winding and with cord winding, with smooth cores and toothed cores of different forms.

The inductance of the whole armature between the brushes of the Immisch motor was found by the secohmmeter to be 0·0023 henrys when the field circuit was closed. The armature was wound with 24 sections of six turns each. The inductance of one of the sections was found to be 0·000034 when it was in the median plane. There does not appear to be any way of deriving one of these numbers from the other.

Power Absorbed in the Circuit.

Experiments made by the three-voltmeter method to determine the power absorbed by an armature coil in which an alternating current was flowing were not very satisfactory. This was partly due to uncertainty with regard to the resistance of the hot copper conductor at the instant at which the measurement was made, and partly to difficulty in obtaining a small non-inductive resistance of sufficient carrying capacity. The observations made with the drum-wound coil showed that when the field was short-circuited very little power was required to send an alternating current through the armature coil in the median plane, beyond that required to overcome resistance in the coil itself. The total power, as measured, was 21 watts, of which 19·5 were expended in the primary, or armature coil, and 0·5 in the secondary, or field-magnet winding; leaving an apparent loss of 1 watt in the iron. The iron loss will certainly be small in this case, since very few tubes pass round through the field magnets. When the field circuit was open, on the other hand, the total power absorbed, with the same current, was found to be 28·5 watts; and, since 19·5 watts were lost in the copper of the coil, 9 were lost in the iron. When the coil was at right angles it appeared that the total power required is 36 watts, making 16·5 watts lost in the iron. In this position of the coil the induction tubes pass round mainly through the pole-pieces, and not through the field-magnet limbs and yokes. The total flux is much larger, so that the induction density in the core is increased; and, as hysteresis losses vary as the 1·6th power

of the induction density, it is possible that, in spite of the smaller volume of iron affected, the loss of power may be greater.

Power Absorbed in Ring Winding.

Some previous experiments made with the ring winding were unsatisfactory, as the resistance used in series with the armature coil had some inductance. The effect of neglecting this in the calculations was to increase the apparent loss of power. Table VIII. shows the results obtained for the drum and ring windings. In the second and fourth columns of figures are given the results of subtracting the watts needed to send the current through the copper of the coils from the total watts.

Table VIII.

Condition of Experiment.	Drum Winding.		Ring Winding.	
	Total Watts.	Watts in Iron and Field.	Total Watts.	Watts in Iron and Field.
Open circuit	28·5	9·0	45	17
Field short-circuited by 0·1 ohm	30·0	1·5	35	7
Field short-circuited by a thick wire	21·0	1·5	32	4
Armature out of field	...	...	31	3

The watts lost in the copper of the armature coil are 19·5 for the drum, and 28 for the ring. The effect of the inside conductors will be to increase the induction density in the core, and thus to produce an increased hysteresis loss.

When the field is short-circuited, the loss outside the coil itself is small. In an actual dynamo the resistance of an armature coil is often much larger than in these experimental coils, and the proportion of the loss due to resistance to the remaining losses, when an alternating current is sent through it, will be even greater than that observed. It seems to follow that when, as in the case of a dynamo that is sparking, a rapid variation of the flux in a coil takes place, a very small proportion of the resulting loss of energy is caused by hysteresis or eddy-currents in the iron, or induced currents in the field

circuit. The energy is changed into heat mainly in the armature coil itself, in the brushes, and in the spark. It must be remembered that the field circuit of a shunt-wound dynamo, or motor, is always short-circuited through the armature.

Calculation of the Inductance of a Drum Armature Coil.

The next section of the paper shows how the value of the true inductance of the coil can be easily found approximately from the dimensions, in the different cases experimented on, and how the apparent inductance with an alternating current depends on the true inductance.

1. *Coil removed from Core.*

The inductance of a long, narrow rectangular coil, without iron, can be calculated by means of Maxwell's formula for parallel cylindrical conductors, conveying equal currents in opposite directions, with sufficient accuracy for present purposes. The formula is $L = l\ (2 \log_{\epsilon} \frac{d^2}{a^2} + 1)$, where d is the distance between the axes of the two conductors, a the radius of each wire, and l the length. Assuming $a = 0{\cdot}015\ d$, we obtain $L = 17{\cdot}4\ l$ for the inductance of a long rectangular coil, of one turn, the length of which is l, and which is not in the neighbourhood of iron The action of the ends of the coil is here neglected. This ratio of a to d is about that which often obtains in practice. A large change in the ratio does not greatly change the value of L.

2. *Effect of the Iron Core.*

The following considerations will help in understanding the action of the iron core. In Fig. 3 are shown the magnetic equipotential surfaces, and the boundaries between the tubes of magnetic induction, due to two such wires as are considered above. The latter are indicated by full, and the former by dotted, lines. The diagram consists of two sets of circles, cutting one another orthogonally. The sections of the two wires are represented by the two circles A B C and D E F. A certain number of tubes of induction are indicated as existing within the section of the

wire. These are in reality not strictly circular, but the difference in the two diameters is hardly large enough to appear in a small diagram. There is no magnetic potential within the wires, and so no equipotential surfaces can be shown there.

It is possible to construct the tubes and surfaces in such a way that each of the cells into which they divide up the space about the wires represents a unit of energy, stored up in the medium, and ready to be given up to the circuit when the current diminishes. A certain amount of energy is also stored in the wire itself, but this cannot be represented in the same way; if the wires are small enough, and far enough apart, it may be neglected. The total amount of the stored up energy is in any case $E = \frac{1}{2} L C^2$, where C is the current flowing. This equation gives a more accurate definition of the inductance, L, than the ordinary one, which makes it equal to the total number of tubes linked with the circuit when unit current flows through the wires. If only the tubes outside the wires are taken, the value obtained is too small, while if all the tubes are counted the result is too large. In the majority of practical cases, as in the present one, the difference can be neglected.

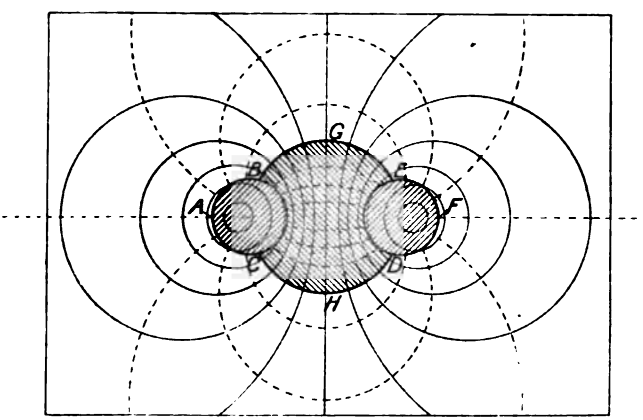

FIG. 3. - Magnetic Field due to Two Cylindrical Wires.

Let the circle B G E H, in Fig. 3, represent the section of a cylinder, grooved at the sides to receive the wires, and filled with some material of very high permeability. The wires in an actual armature coil will of course be very much smaller, as compared

with the armature, than those shown in the drawing. The magnetic potential will be very nearly the same throughout the permeable cylinder, and there will be practically no equipotential surfaces inside it, to divide up the induction tubes into energy cells.

Now before the introduction of the permeable material the cylinder contained half of the equipotential surfaces, and practically half of the energy cells. Even with the thick wires drawn, there are 48 cells inside the cylinder to 57 outside. On introducing the cylinder half the magneto-motive force—that is, half the current—will produce the same flux as before. There will be half as many equipotential surfaces, but they will all be outside the iron, and the outside portions of the diagram will remain unchanged. The number of energy cells with current ½ A. and using the permeable cylinder, is half the number with current A, without the permeable cylinder. Doubling the current doubles the number of tubes of force, and also the number of equipotential surfaces, so that each energy cell is divided into four new ones. Thus current A, with the permeable cylinder, gives twice as many energy cells as without it. Thus the inductance is doubled by the action of the permeable cylinder. This can also be seen from the fact that the number of induction tubes is doubled. It follows from this that the inductance of a coil of one turn, on a drum armature, outside the field, is approximately, neglecting the action of the ends,

$$\mathrm{L} = 2\,l\,(2 \log_\epsilon \frac{d^2}{a^2} + 1);$$

or if, as before, A = 0·015 d, the inductance will be L = 35 l. For such a coil on the armature of the Immisch motor this gives 0·00000071, while the observed value for coil D, including the ends, was 0·00000093. The conductor was of square section, and considerably thicker than is assumed above.

3. *True Inductance on Core, in Field.*

When the armature is in position in the machine, the true inductance of a coil will be twice the energy stored up when unit current flows in the coil. With a coil of one turn this is practi-

cally equal to the magnetic flux through the coil. The flux can be calculated from the dimensions of the magnetic circuit. As the greater part of the flux will pass through the air gap between the pole-faces and the core, and as the reluctance of the air gap is in most machines rather high, as compared with that of the rest of the magnetic circuit, the true inductance will be nearly the same in all positions of the coil, and its approximate value can be obtained from the dimensions of the pole-faces and air gap. The total flux equals 4π times the number of turns in the coil, n, times the area of the pole-face s, divided by the length of the double air gap, g, $= \frac{4 \pi n s}{g}$, and the true inductance is

$$L = \frac{4 \pi n^2 s}{g}.$$

With a polar angle of 135° and the air gap one-tenth of the diameter of the core, $L = 15 \pi^2 n^2 l$. This is about four and a half times the value, found above, for the inductance of the coil on the armature core outside the field, n being equal to unity. A certain amount should be added to this for tubes which pass round through the air without penetrating the pole-faces. Neglecting this leakage, the inductance of a single turn on the armature of the Immisch motor was found to be 0·00000227.

Experimental Determination of the True Inductance.

In order to measure the true inductance of a coil, in the median plane, on the armature of the motor, the flux through the pole-piece was measured when a continuous current was flowing in the coil. A test coil was used, wound on a suitable frame, and connected with a slow-swinging, undamped D'Arsonval galvanometer. The test coil enclosed all the tubes which passed through one of the pole-faces of the motor, and could be pulled out of the field in a very small fraction of a second. A standard mutual inductance was made use of to find the number of tubes corresponding to the observed swing of the galvanometer. The secondary was put in series with the galvanometer and test coil. Since the mutual inductance was 0·01 henrys, a change of 1 ampere in the primary would give the same electro-motive force in the secondary that would be

produced by a change of one million lines in a coil of one turn. Thus, by finding the current in the primary which would give, on break, the same galvanometer swing as was produced by the test coil, the number of tubes cut could be determined. In this way, neglecting magnetic leakage as before, the inductance of an armature coil with one convolution was found to be 0·00000232.

4. *Apparent Inductance.*

The ratio of the virtual potential difference between the terminals of the coil, to the current flowing through it, may be called the apparent impedance of the coil, $I_a = \frac{E}{C}$. This may be resolved into two components, at right angles to one another—the resistance and the apparent reactance. If the apparent reactance is divided by p, which is 2π times the frequency, the apparent inductance is obtained, $= L_a$. $I_a = \sqrt{R^2 + p^2 L_a^2}$. This is the method made use of in the observations with alternating currents recorded in the preceding pages. Since the true inductance of the coil depends mainly on the dimensions of the air gap, it seems that the apparatus, when the armature coil is in the median plane, can be treated to some extent like a transformer with an air core; the iron of the field magnets acting like a secondary with a high resistance in its circuit, while the field-magnet coils, when short-circuited, form a secondary of very low resistance.

In a transformer with an air core and a short-circuited low-resistance secondary, the current in the primary, at any instant, is, if p is large,

$$c = \frac{E L' \sin(pt - \epsilon)}{\sqrt{(L L' - M^2)^2 p^2 + L'^2 R^2}}.$$

or $$c = C \sin(pt - \epsilon),$$

where
C = maximum current in primary,
E = maximum E.M.F. in primary,
L = inductance of primary,
L' = inductance of secondary,
M = mutual inductance of the two coils,
R = resistance of the primary.

The current in the secondary is—

$$c' = \frac{M\,E \sin(pt - \epsilon - \pi)}{\sqrt{(L\,L' - M^2)^2\,p^2 + L'^2\,R^2}}.$$

It lags behind the current in the primary by half a period, and exactly opposes the tendency of the primary current to produce a flux through the secondary.

The apparent impedance is

$$I_a = \frac{E}{C} = \sqrt{R^2 + \frac{(L\,L' - M^2)^2\,p^2}{L'^2}}.$$

Thus the apparent reactance is $\frac{(L\,L' - M^2)^2\,p^2}{L'^2}$, and the apparent inductance $L_a = \frac{L\,L' - M^2}{L'}$.

The maximum magnetic flux through the primary is $\frac{C}{n}\left(\frac{L\,L' - M^2}{L'}\right) = \frac{C}{n}\,L_a$, n being the number of turns in the primary. Thus in this case the apparent inductance is measured by the number of tubes that pass through the coil with unit current, multiplied by the number of turns.

It is also true that the magnetic flux, N, at any instant, is equal to $\frac{c}{n}\,L_a$; so that if n times the ratio of the instantaneous flux to the instantaneous current is called the instantaneous inductance, L_i, it follows that in this case $L_a = L_i$.

The maximum magnetic flux through the secondary is $\frac{C\,M\,R'}{n'\sqrt{L'^2\,p^2 + R'^2}}$, where n' is the number of turns in the secondary, and R' is its resistance. If R' is small this quantity will be small. If the transformer is constructed in such a manner that $L = L'$, the apparent inductance is $L_a = \frac{L^2 - M^2}{L}$. Thus it appears that if $L = M$—that is, if there is no magnetic leakage—the apparent inductance will be zero.

Calculation of Magnetic Leakage.

Suppose the armature coil is in the median plane, and consists of one turn. If the copper in the field coils is used to form a

closed coil, of one turn, about each yoke, the effect of the short-circuited field on the primary will not be altered. Suppose also that the inductance of this field coil is the same as that of the armature, as will be the case if magnetic leakage can be neglected. The observed steady flux through the poles, with unit continuous current through the armature coil, may be taken as the mutual inductance of the two coils, $M = 0{\cdot}00000232$.

The apparent inductance with the field short-circuited by 0·1 ohm, is 0·00000102 henrys. Hence, since $L_a = \dfrac{L^2 - M^2}{L}$, it follows that $L = \dfrac{L_a}{2} + \sqrt{M^2 + \left(\dfrac{L_a}{2}\right)^2} = 0{\cdot}00000288$ henrys. The difference between this and the mutual inductance gives the magnetic leakage about the ends of the coil and between the pole-tips. This difference is 0·00000056, which appears rather high, as the whole inductance of the armature coil, without iron, was found in one experiment to be only 0·00000062 henrys.

Another method used to obtain some idea of the leakage was to measure the current induced in the field coil. The number of turns in the field coil could not be determined with any great accuracy without unwinding them, but it appeared that 163 ampere-turns in the armature coil produced 146 ampere-turns in the short-circuited field coil. This makes $L = 0{\cdot}00000259$ henrys, and the magnetic leakage, measured in henrys, 0·00000027, or about half the value previously obtained. No doubt the assumption that $L = L'$ if the number of turns is the same is not quite correct. In any case the two values are of the same order.

EXPERIMENTAL INVESTIGATION OF THE FIELD PRODUCED BY AN ALTERNATING CURRENT IN AN ARMATURE COIL.

A narrow rectangular coil of five turns was wound of No. 16 wire, which could be laid on the armature parallel to the axis. An alternating current of 27·7 amperes was sent through coil C, of 11 turns, wound like an ordinary drum winding on the armature, which was removed from the field, the 11 wires on each side being bound together into a solid bunch. When the

narrow coil lay on the armature, as near as possible to one side of C, 0·172 volt was induced in it, while when it was moved to the position on the armature where it was equally distant from the two sides of C, 0·145 volt was induced. This shows that there is no very great difference in the number of tubes per square centimetre proceeding from different parts of the surface of the armature. This tends to show that the field, in this case, is like that shown in Fig. 3.

It would be interesting to continue the experiment with the armature in place in the short-circuited field, and see how far the magnetic distribution sketched on the right side of Fig. 1 is correct.

Variation of Flux through the Pole-Pieces in a Running Machine.

A coil may be wound round the armature, separated from it so that the armature is free to turn; or the coil may be wound on the pole-piece. Any alternating variation of magnetic flux when the machine is running will induce an electro-motive force in this coil, which can be measured by the alternating-current voltmeter. This experiment was tried with the Immisch motor, and with a Goolden drum-armature dynamo. The variation of flux was found in some cases to be considerable, but by properly adjusting the brushes it was found possible to reduce it to a very small quantity, the sparking becoming very small at the same time. This might well prove a valuable method for estimating the intensity of sparking in a given machine.

Measurement of the Reversing Field.

In the theory of commutation, as developed by Swinburne in his paper on "The Theory of Armature Reaction in Dynamos and "Motors" (*Journal of the Institution of Electrical Engineers*, vol. xix., p. 90, 1890), a most important factor is the magnitude of the reversing field in which the armature coil moves during the period of commutation. It was decided to try to measure this field in a running dynamo. A Manchester machine, by Mather & Platt, was made use of. It had a ring armature, and was designed

to give 100 volts and 35 amperes at 1,600 R.P.M. The field magnets were compound-wound. In order to be able to run above the sparking limit without using an excessive current, the speed was reduced to 1,180 R.P.M.; thus diminishing the terminal voltage, the current in the shunt coil, and, as a result, the flux through the armature. The result was that a current only a little larger than the normal working current made sparkless commutation impossible. The brushes were placed in such a position that a coil, during commutation, moved from a place just outside the pole-tip to one well under it, and it was found possible so to adjust the current that sparking almost ceased. Any change in the current, whether it were an increase or a decrease, increased the sparking, and, with the proper value, the sparks were very minute indeed. A coil of four turns, of very thin wire, was wound like a ring coil on the armature, between two of the sections. One end of this was soldered to the armature spider, and was thus connected to the body of the machine, the other to an L-shaped brass contact piece, which was insulated from the shaft by an ebonite ring, to which it was fastened. A radial brush, of steel watch-spring, touched the projecting part of this contact piece once in each revolution. The position of the brush could be varied by means of a suitable rocker, on which an arbitrary scale was cut, so that its position could be recorded for every observation.

A wire led from the brush to one of the quadrant pairs of an electrometer, the other pair being connected to the instrument case and to the dynamo frame. The current given by the dynamo throughout the experiment was 37 amperes, which gave minimum sparking.

Results of this Experiment.

The results are shown in the form of a diagram, in Curve I., Fig. 4. Ordinates represent the volts induced in the test coil, being taken as positive when coinciding in direction with the current which flows in the armature coil after reversal. Abscissæ represent the positions of the test coil with respect to the poles, in terms of the scale cut on the rocker of the test brush. A

shows the position of the coil when commutation commences, B when commutation ends; the pole-tips are indicated by C and D. The diagram shows that the field in which the coil moves during commutation varies from a value which produces about 1 volt in the moving test coil, in the negative direction—that is, in the direction of the initial current in the armature coil (indicated by EF in the diagram)—to a value which produces about three-tenths of a volt in the positive direction in the coil (indicated by G H). If the

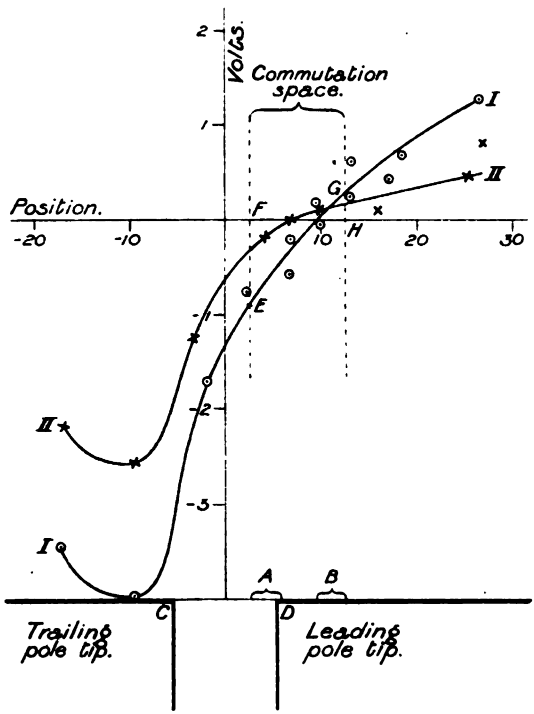

FIG. 4.—Volts in an Experimental Coil wound on a Dynamo Armature.

armature had been drum-wound, such observations would have indicated that the average value of the reversing field during the whole period of reversal was *negative*, but small. Since, however, there is a leakage field inside the ring, and the test coil cuts this also, what is proved is that the field entering the armature on the outside, and that leaving it on the inside, at the place where commutation occurs, are nearly equal. It may be noted that

at the speed at which the dynamo was run 0·4 volt would be induced in the test coil if the outside wires cut a field of about 500 tubes per square centimetre. According to the ordinary theory the induction density should have at least this value throughout the whole commutation space.

SECOND EXPERIMENTAL DETERMINATION.

Curve II., Fig. 4, shows the result of another set of observations, in which the speed was 880 R.P.M. The non-sparking current, with the brushes in the same position, was 26 amperes. The points observed are marked by crosses, while those belonging to Curve I. are surrounded by circles. The two curves are very similar, the amplitude of II. being less than that of I., on account of the lower speed.

MEASUREMENT OF FIELD WITHOUT ARMATURE REACTION.

In order to make sure that the electrometer was working properly, an experiment was made without current in the external circuit. The volts in the test coil were found to be nearly the same under the two pole-tips. From the numbers obtained the induction density under the poles was calculated, and found to be 1,990. From the observed volts on the dynamo terminals, the known number of armature conductors, the dimensions of the pole-faces, and the speed of the machine, the value $B = 1{,}930$ was obtained.

THEORY OF THE REVERSING FIELD.

Consider the case of a drum armature, so wound that two neighbouring bars belong to the two coils which undergo commutation at the same time on opposite sides of the commutator. For the sake of simplicity, imagine these pairs of coils to be combined into single coils, each of one turn, and conveying the full dynamo current. Let the brushes be just wide enough to cover one commutator segment and overlap the insulation, so as to touch the next one. Then, if at the beginning of commutation the conductor E is at A in Fig. 5, and is conveying the full current upward, it will be at D at the end of commutation, and will

convey the full current downward. The conductor just behind it will have moved up to A, and will still convey its current upward. Both cross and back turns are now exactly what they were at the commencement of commutation; and C. C. Hawkins, M.A., A.I.E.E., in a paper on "Armature Reaction and the Theory of Com-"mutation" (*The Electrician*, vol. xxxiv., April 30th to June 11th, 1897), shows that they remain constant throughout the whole period of commutation. The resistance of an armature coil in a modern dynamo is so small that only a very small electro-motive force is required to send the current through it. This electro-motive force will be supplied by cutting a very weak field; and, if the current dies away to zero and rises again to its maximum

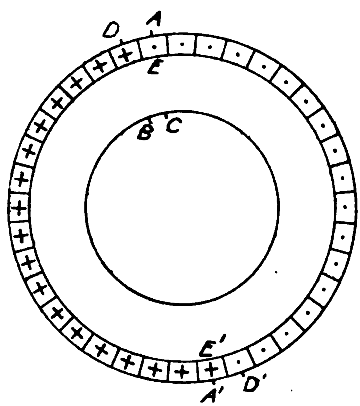

FIG. 5.—Section of Drum Armature.

value according to a straight-line law, a negative electro-motive force and a negative field will be required for this purpose during the first half of the commutation. For the present the resistance may be neglected, and the effect of moving a coil of infinite conductivity in a magnetic field considered. A current is induced in such a coil which keeps the flux through it constant. It follows that, as the armature coil moves from A to D, the flux through it remains nearly constant, the current being exactly reversed when it reaches D, in the case of sparkless commutation. In other words, the flux, at any instant, through the coil at A is equal to that through the coil at D. For the latter coil has just

come from A, the flux through it is the same as when it was at A, and it is clear that whatever coil may be at A the flux through it will be the same. Now, in order that the flux through the coil at A may be equal to that through the coil at D, it is necessary that the resultant magnetic field between A and D, and between A′ and D′, should be zero. As a matter of fact, a small negative field will be required near A, and a small positive field near D, to keep the current running in the coil against the resistance of the copper; but 40 or 50 tubes per square centimetre will probably be sufficient for this, if the brush makes good contact.

As it has been stated that a very much larger reversing field than this is required, it will be well to examine the matter more closely, and find out what the difference is due to. Consider first the field due to the armature alone. Both the cross turns and the back turns tend to produce a field the direction of which is into the core at A D, and out from it at A′ D′. Let B′ be the resulting induction density in the air outside the core, and a be the area traced out by the axis of the bar in moving from A to D. Then $B' \times a$ is the flux into A D, and $B' \times a$ is the flux out of A′ D′. It follows from this that the change of flux through the coil E, in passing from A to D, is $B' \times 2a$. Now the current in the other armature coils is unchanged by the motion of the coil E from A to D, and no part of the change of flux through E is due to them; also, the true inductance of coil E is hardly changed by change of position, so that B′ (A D + A′ D′) is the change of flux in coil E due to the change in the current from the value $-$ C to $+$ C. Thus $2\,L\,C = B' \times 2\,a$. Now, in order that the current may be reversed in the manner indicated, the flux due to the field magnets, in through A′ D′, and out through A D, must, the resistance of the coil being neglected, exactly neutralise the flux due to the armature. If B is the field intensity produced by the field magnet, $B \times 2\,a + B' \times 2\,a = 0$. Thus $B = -B'$.

If, however, the fact that the change of flux through the armature coil, due to the change of current, and the back field through A D and A′ D′, due to the current in the entire armature, are simply different ways of looking at the same thing, is lost sight of, an erroneous result will be obtained: the value of B will

be taken twice as high as it should be. If, however, L_a, the apparent inductance, is used instead of L, the true inductance, and if L_a is one-fourth of L, the component of the reversing field due to the field magnet will only have to be increased 25 per cent. beyond its true value. So that, if the field due to the armature is 4,000 tubes per square centimetre, that due to the field magnets will be taken as 5,000, so as to leave a resultant field of 1,000, which is wrongly supposed to be necessary to neutralise the change of flux through the coil, due to the change in current. In a previous section of this paper the ratio of L to L_a was found to be about 4·5 by calculation, or 2·8 by observation.

It is clear that the result of an error such as that just mentioned would be to cause an underestimate of the possible output of a machine as limited by sparking. For, if the field due to the field magnets is 5,000, it would appear necessary to keep the current so low that the back field due to the armature should be not greater than 4,000, whereas in reality it might rise almost to 5,000 without making sparkless commutation impossible.

ENERGY LOST IN A SPARK.

Though a knowledge of the apparent inductance proves to be of no value in determining the magnitude of the reversing field, it is needed to calculate the energy lost when sparking takes place. If C_1 is the initial value of the current in the coil at the instant when the tip of the brush breaks contact with the commutator sector, and C_2 its final value when the spark has died away, the current through the spark at any instant will be $C_2 - C$, where C is the current in the coil. The potential difference between the ends of the coil is $L_a \frac{dC}{dt}$, and the energy lost in the spark in the time dt is $(C_2 - C) L_a\, d\, C$. The total energy lost in the spark is

$$\int_{C_1}^{C_2} (C_2 - C)\ L_a\ d\ C = C_2\, L_a\, (C_2 - C_1) - \tfrac{1}{2}\ L_a\, (C_2^2 - C_1^2);$$

and this is equal to $\frac{1}{2} L_a (C_2 - C_1)^2$, or half the apparent inductance

times the square of the change of current. This result, however, is only obtained when the resistance of the armature coil is neglected. If this is taken into account, the energy lost in the spark will be found to be greater; one assumption as to the way in which the resistance of the spark varies making the energy lost $L_a (C_2 - C_1)^2$.

Suppose a dynamo is so designed that it will just run sparklessly at full load, the forward lead being so large that the coil undergoing commutation is right under the pole-tip. If the current is switched off without altering the excitation, or the lead of the brushes, the machine will spark, and the energy of each spark will be proportional to

$$\frac{L_a B^2 l^2 d^2}{L^2 S}, \text{ or to}$$

$$\frac{L_a}{L} \frac{l\, d\, q}{S^2\, \phi}, \text{ or to}$$

$$\frac{L_a}{L} \frac{B\, V\, r\, n\, q}{\phi^2};$$

with a multipolar dynamo the last expression becomes proportional to

$$\frac{L_a}{L} \frac{B\, V\, r\, n\, q}{\phi^2\, p}.$$

l = length of armature.

d = diameter of armature.

S = number of commutator segments.

q = effective length of magnetic circuit.

ϕ = polar angle.

r = revolutions per second.

n = number of complete turns to a coil.

L_a and L are the apparent and real inductances of one turn.

p = number of poles.

V = volts per commutator part.

The question of the sparking of a machine when loaded, if the brushes are kept in the neutral plane, is of more practical importance, but is more difficult to treat theoretically.

Constancy of Flux in Short-Circuited Coil.

That the flux through a short-circuited armature coil in a

dynamo that is not sparking remains nearly constant appears as follows :—

Let a closed coil the resistance of which is R and the inductance L be moved in a magnetic field. At time 0 let the flux through it due to the outside field be F_1, and the current be C_1, so that the total flux through it is $F_1 + L C_1$. At time T let the flux due to the outside field be F_2, and the current be C_2, and at some intermediate time t let the flux be F and the current C. Then the total flux through the coil at time t is $F + L C$.

The electro-motive force acting at this instant to overcome resistance is $-\left(\frac{dF}{dt} + L\frac{dC}{dt}\right)$, and this is equal to R C. Assume that the coil is moved in the field in such a way that the current is a linear function of the time, or $C = C_1 + \frac{C_2 - C_1}{T} t$.

$$\text{Then } dF + L\frac{C_2 - C_1}{T} dt = -R\, C_1\, dt - \frac{R(C_2 - C_1)}{T} t\, dt.$$

$$\therefore F_1 - F_2 = +\int_0^T \left\{ L\frac{C_2 - C_1}{T} + R\, C_1 + \frac{R(C_2 - C_1)}{T} t \right\} dt.$$

$$= \quad L(C_2 - C_1) + R\, C_1\, T + \tfrac{1}{2} R(C_2 - C_1) T.$$

Suppose $C_1 = 0$ and $F_2 = 0$, then

$$F_1 = L\, C_2 + \tfrac{1}{2} R\, C_2\, T.$$

The initial total flux through the coil is F_1, the final is $L\, C_2$; the difference between them is $\frac{1}{2}$ R C_2 T. This difference will be very small if R is small, or if T is small. In the case of a coil of a dynamo armature during reversal, both these quantities are small, so that the flux through the coil must remain practically constant during the whole period of reversal. It will be noticed that R C_2 is the electro-motive force required to keep the current C_2 running in the coil, so that the difference between the initial and final flux is half the change of flux that would be required to keep the current C_2 flowing in the coil for the time T.

Summary.

The results obtained may be recapitulated as follows :—

1. The apparent inductance of an armature coil, in the

median plane, is nearly the same, whether determined by the secohmmeter or by the alternating-current method.

2. Under ordinary working conditions, the shunt winding of a dynamo or motor being short-circuited by the armature acts like the short-circuited secondary of a transformer with considerable magnetic leakage.

3. The result is that the apparent inductance, under these conditions, is about the same as that of the coil on the core when removed from the field, which can be determined approximately by calculation.

4. If the theory is correct which makes the apparent inductance of an armature coil equal to $\frac{L L' - M^2}{L}$, and if it is possible to reduce the magnetic leakage, the apparent inductance can be reduced by this means, thus diminishing the energy lost in sparking when the brushes are not exactly adjusted.

5. The apparent inductance when the coil is at right angles to the median plane does not differ very much from the true inductance, as determined by measuring the flux with a continuous current.

6. In finding the apparent inductance of a ring armature, the external field is regarded as being the same as in the case of a drum. The field in the interior of the core has also to be taken into account.

7. The magnitude of the reversing field is shown by experiment, and by theory, to be very small.

8. The variation in flux through the armature when sparking occurs can be observed by means of a fixed coil, wound outside the armature, or upon the pole-piece, and connected to an alternating-current voltmeter. It is shown that this variation is small in a machine which is running with little sparking.

PRESENTATION OF TESTIMONIAL TO MR. FRANCIS H. WEBB.

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REPORT OF THE HONORARY SECRETARY OF THE TESTIMONIAL (MR. HENRY EDMUNDS), *read on the occasion of the Presentation, at the Hotel Métropole, on February* 21*st*, 1898.

The proposal to offer a testimonial to Mr. Francis H. Webb, in recognition of his long services as Secretary of the Institution of Electrical Engineers, first assumed practical shape early in December, 1897. It then appeared desirable to make the presentation upon a day as near as possible to the date of his retirement, namely, the 12th February, 1898. A Provisional General Committee and an Executive Committee were therefore formed; the latter consisting of Dr. John Hopkinson, Mr. Edward Manville, and Mr. Alexander Siemens, with Sir Henry Mance as Chairman, and Mr. Henry Edmunds as Honorary Secretary.

After due consideration, it was decided that an illuminated address, with an album containing the autograph signatures of contributors, should be presented to Mr. Webb; that a diamond brooch should be given to Mrs. Webb; and that the balance of the money subscribed, after paying all necessary expenses, should be presented to Mr. Webb.

I accordingly wrote to Mr. Webb, informing him that he had probably heard that a number of his friends, being desirous of recognising in a substantial manner his services to the Institution of Electrical Engineers, had formed themselves into a Committee, which had communicated with the various Members, Associates, and Students of the Institution, informing them of the objects of the Committee; and this had resulted in a very liberal response. Also, that it was further proposed to give a dinner at the Whitehall Rooms on the 21st February; and that after the dinner a reception would be held, and an album containing the autographs of many of his friends, a sum of money, and a gift for Mrs. Webb, would be presented. I informed him that the amount of money would exceed £600; but that, at the time of writing, I did not know the exact amount, as additions were (and still are) being constantly made; and I asked Mr. Webb if he would kindly

inform me, on behalf of the Committee, what his wishes were as to the mode of payment of this money, or other disposition of the same.

In reply to this, I received the following letter:—

> "I am greatly obliged to the Committee for their "kind thought in consulting my wishes as to the form in "which the money that has so generously been subscribed "should be presented to me; and I venture to suggest "that £600 should be invested in the names of trustees in "such manner that the income arising therefrom should "be secured to my wife and myself during our joint lives, "and to the survivor, but with power to me to dispose of "the capital, subject to the life interest as above referred to.
>
> "If some such arrangement could be carried out, "perhaps two members of the Committee would kindly "consent to act as trustees; and I should, of course, be "prepared to sign any documents that might be necessary."

The Committee, after receiving this letter, decided that the trustees should be Mr. Edward Manville and Mr. Henry Edmunds, acting in conjunction with Mr. Webb. A trust is being formed, and the money will shortly be invested.

The total sum subscribed amounts to nearly £750; so that, in addition to the £600 for which we have the Bank certificate to hand to Mr. Webb to-night, there is a balance covering all the out-of-pocket expenses for correspondence, printing, postages, the preparation of the album and the illuminated address, the present purchased for Mrs. Webb, and also a silver case to contain the certificate. But no part of the subscriptions has been applied either for the dinner this evening, or to the reception following the same; and we still hope, as additional subscriptions come in, to be able to add a further small amount in cash to the £600 which will be given to Mr. Webb.

Independently of the financial side of the question, it cannot fail to be most gratifying to Mr. Webb to know that we have received from Members, Associates, and Students upwards of 1,000 signatures for the autograph album, not merely from all parts of

the United Kingdom, but practically from every country upon the face of the globe.

As you may well imagine, the amount of correspondence in connection with this testimonial has been very considerable; and in this matter the Committee has been most ably seconded by Mr. McMillan, our new Secretary. We have had offers of help from so many old friends of Mr. Webb that it would seem that our only real difficulty in this undertaking has been in limiting the number of friends who wished to be officially connected with the Committee. However, as small compact bodies are believed to work more rapidly and expeditiously than larger ones, the bulk of the work has fallen upon the Executive Committee; and, as a member of the Executive Committee, I feel we are well rewarded by the way in which our appeal has been responded to by the Members, Associates, and Students of the Institution of Electrical Engineers, thus ensuring the gratifying success of the undertaking.

[*Copy.*]

ADDRESS TO MR. FRANCIS HUGHES WEBB, SECRETARY OF THE INSTITUTION OF ELECTRICAL ENGINEERS, 1878–1898.

21*st February*, 1898.

DEAR MR. WEBB,

Upon the occasion of your retirement from the Secretaryship of the Institution of Electrical Engineers, we, the undersigned, Members, Foreign Members, Associates, and Students of the Institution, desire to give expression to the deep sense that we entertain of personal and corporate indebtedness to you.

For 20 years you have devoted the whole of your untiring energy to that office, with a single mind to the interests of the Institution; and the increase in the number of members during these years from 800 to nearly 3,000, coupled with the soundness of the financial position to which the Institution has attained in the same period, sufficiently attest the value and the success of your labours.

In conducting the work of the office your uniform and unfailing courtesy, tact, sympathy, and kindliness have endeared you to all with whom you have been brought in contact. And now, in presenting you with this address, together with a sum of money amounting to upwards of £600, and a diamond brooch, which we beg Mrs. Webb to accept as a token of our esteem, we earnestly hope that for many years you may be spared with her to enjoy the rest that you have earned so well; and we trust that the Institution may long be permitted to have the benefit of your experience, which has proved so valuable to it in the past.

We beg also that you will regard the autograph signatures in this album as those of true and sincere friends.

We are, dear Mr. Webb,
On behalf of the Subscribers,
Yours very truly,

H. C. MANCE,
JOHN HOPKINSON,
E. MANVILLE,
A. SIEMENS,
HENRY EDMUNDS (*Hon. Sec.*),
Executive Committee.

NOTICE.

1. The Institution's Library is open to members of all Scientific Bodies, and (on application to the Secretary) to the Public generally.

2. The Library is open (except from the 14th August to the 16th September) daily between the hours of 11.0 a.m. and 8.0 p.m., except on Thursdays, and on Saturdays, when it closes at 2.0 p.m.

An Index, compiled by the late Librarian, to the first ten volumes of the Journal (years 1872–81), and an Index, compiled under the direction of the late Secretary, to the second ten volumes (years 1882–91), can be had on application to the Secretary, or to Messrs. E. and F. N. Spon, 125, Strand, W.C. Price Two Shillings and Sixpence each.

// JOURNAL

OF THE

Institution of Electrical Engineers.

Founded 1871. *Incorporated* 1883.

VOL. XXVII. 1898. No. 134.

The Three Hundred and Thirteenth Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday, March 24th, 1898—Mr. JOSEPH SWAN, F.R.S., President, in the Chair.

The minutes of the Ordinary General Meeting held on March 10th were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council:—

From the class of Associates to that of Members—

William Charles Cloete Hawtayne.
J. W. Meares.

From the class of Students to that of Associates—

Robert Fleetwood Fuller. | Owden V. Greeves.
O. M. C. Heyl.

Mr. H. C. Gunton and Mr. E. H. Cozens Hardy were appointed scrutineers of the ballot.

Donations to the Library were announced as having been received since the last meeting from Lady Cook; Messrs. Crosby, Lockwood, & Co., the publishers of *The Electrician;* Messrs. Macmillan & Co.; Mr. A. A. Campbell Swinton, Member; and Mr. E. K. Scott, Associate; to all of whom the thanks of the meeting were unanimously accorded.

The PRESIDENT: I have much pleasure in calling upon Mr. Hammond to read an abstract of his paper. The paper is a very voluminous one on a very important subject; it is quite impossible to read it in its entirety.

THE COST OF GENERATION AND DISTRIBUTION OF ELECTRICAL ENERGY.

By ROBERT HAMMOND, Member.

Mr. Hammond

1. Section 9 of the Electric Lighting Act, 1882 (45 and 46 Vict., chap. 56), runs:—

"ELECTRIC LIGHTING ACT, 1882."

"*Accounts*—9.—The Undertakers shall on or "before the 25th day of March in every year fill up "an annual statement of accounts of the undertaking, "made up to the 31st day of December then next "preceding; and such statement shall be in such "form, and shall contain such particulars, and shall be "published in such a manner, as may from time to "time be prescribed in that behalf by the Board of "Trade.

"The Undertakers shall keep copies of such "annual statement at their office, and sell the same "to any applicant at a price not exceeding one "shilling a copy.

"In case the Undertakers make default in "complying with the provisions of this section, they "shall be liable to a penalty not exceeding forty "shillings for each day during which such default "continues."

2. I set out in Appendix I. the form of accounts prescribed by the Board of Trade in cases where the undertakers are local authorities. Mr. Hammond.

As regards the manner of publication, the Board of Trade prescribe that copies of the accounts shall, in the case of local authorities, be forwarded to the Board of Trade and to the Local Government Board (in Scotland to the Secretary for Scotland), and in the case of companies to the Board of Trade and to the local authority.

On reference to the form it will be seen that the undertakers have to state not only the amount of the revenue and the working expenses of the undertakings, but to analyse the expenditure under numerous headings, which may generally be summarised as under:—

1. Coals or other fuel.
2. Oil, waste, water, and stores.
3. Wages on generation and distribution.
4. Repairs and maintenance.
5. Rents, rates, and taxes.
6. Management — *i.e.*, salaries, stationery and printing, general establishment charges, law expenses, insurance, &c.

There is also a statement (No. VIII.) from which, if it be faithfully and fully filled in, it is possible with ease to arrive at the cost of, as well as the average revenue arising from, the "Units sold," to discover the load-factor, and to determine the percentage of "Quantity not accounted for."

3. Where companies are undertakers, an almost identical form to that for local authorities is prescribed. The points of difference are:—

(1) In the companies' form the item "Proportion of Salaries "of Engineers" appears under the headings of "Generation" and "Distribution" (*i.e.*, *Works Costs*); whereas in the case of local authorities the item has to be included under *Management*.

Mr. Hammond

(2) Provision is made in the companies' form, under "Management," for remuneration of directors and auditors.

(3) The companies' form provides in the revenue account a heading for "Depreciation."

In the case of companies the returns have to be audited by an auditor appointed by the Board of Trade.

With regard to (1) it is, I think, to be regretted that the two forms are not identical, because, without readjustment of the item of salaries, no trustworthy comparison can be made between the "works costs" of undertakings of local authorities and those of companies.

Such readjustment has been made by me throughout this paper in the costs of 1896 and 1897, the *whole* of the salaries of engineers being placed under "Management," as in the returns of the local authorities.

Manifestly the proper plan would be to place the salaries, especially those of assistants, under the headings of "Generation" and "Distribution" (*Works Costs*). To secure uniformity it might be prescribed by the Board of Trade that the salaries of all assistants, other than those directly concerned with "Management," should be debited to "Generation" and "Distribution," and that one-half of the salary of the managing engineer should be so allocated.

4. The undertakers are under an obligation to file their accounts, made up to the previous December 31st, on or before March 25th in every year; but this obligation is not strictly complied with, especially by local authorities—mainly because, in most cases, their financial year does not end till March 31st, and in some cases not till May 31st.

It may indeed be said that after the rush of the law-abiding, at the beginning of each year, the returns from the law breakers drop in most casually over the remainder of the twelve months, and it is therefore impossible to compile a complete record of the

costs for any year till the end of the following year; and even then one has to mourn the stiff-neckedness of certain individuals who risk the penalty of 40s. per day, not only by not filing accounts within the specified time, but by withholding some of the items demanded by the Board of Trade form. Mr. Hammond

5. On the whole, however, those holding powers under the Electric Lighting Acts have fully complied with their statutory obligations.

The extent to which the data have been growing may be seen by the following table:—

Year.	No. of Electricity Works in operation under statutory powers, or for which statutory powers were subsequently secured.	Year.	No. of Electricity Works in operation under statutory powers, or for which statutory powers were subsequently secured.
1882	2	1890	17
1883	3	1891	31
1884	3	1892	39
1885	4	1893	62
1886	4	1894	81
1887	6	1895	93
1888	7	1896	112
1889	14	1897	121

The Board of Trade have made no publication of these returns, and the only course open to the inquirer is to apply to the various undertakers for copies of the same.

6. Having held for many years the view that the general use of electrical energy for purposes other than lighting would result from low costs of production, the analysis of these returns has, from their first publication, proved a great fascination to me, and at the beginning of 1892 I started the idea of publishing analyses weekly. Hence their appearance in *Lightning*, and the creation of "Chesterfield Junr."

As no paper on the subject has been laid before the Institution

Mr. Hammond. since Mr. R. E. Crompton's "Cost of Electrical Energy," read on April 26th, 1894, it has struck me that a fairly complete analysis of the returns, as far as "costs" are concerned, might prove useful to the members, and possibly have a convincing effect, upon those who have not hitherto examined the data, as to the gradually decreasing cost of the production and distribution of electrical energy from central electricity supply works in the United Kingdom.

7. Before embarking on that analysis, I draw attention to the fact that the existence of such data, as far as I am aware, is unique in the history of English industries, if not in that of the industries of the world.

The nearest parallel to it in this country, that I can recall, is the quarterly and duly audited returns of the selling price of finished iron, published by the Board of Conciliation and Arbitration for the Manufactured Iron and Steel Trade of the North of England, and accepted by both masters and men as the basis of wages for the ensuing three months.

The finished iron makers, however, though heartily co-operating in the publication of these data, would, I imagine, stand aghast at the idea of disclosing the costs of all the items that go to make the finished article—*i.e.*, the pig iron, coals, fettling, repairs, wages, depreciation, and, we may now add, electricity.

Similarly, I fancy that Colonel Makins, the chairman of the Gas Light and Coke Company, would shrink from such a rigid analysis of his cost sheets as that set out in the form prescribed by the Board of Trade for electricity undertakings.

8. Some, indeed, have been puzzled to understand why so socialistic a condition was thrust upon our industry.

It must be remembered that 20 years ago, when we were agitating for statutory powers for electric lighting, there was a strong anti-monopoly feeling prevalent. The Legislature would accord only a 21 years' tenure, with an option at the end

of that period on the part of the local authority to buy up the undertaking at a break-up value. Meanwhile the local authority had to know exactly how the business was progressing. Hence clause 9, as to filing accounts, in the Act of 1882. Mr. Hammond.

When we secured in 1888 an extension of the tenure to 42 years, the onerous clause was allowed to remain.

9. The English are notably a patient race. The clause was accepted without a murmur as part of the nature of things, and it has unexpectedly operated most markedly in assisting the reduction of costs of production and in the lowering of prices to consumers.

The publication of analyses of the current year's returns, in the electrical papers, has acted like an irritant upon the engineers of those works whose costs were high, and as a stimulus to still greater economies on the part of those whose costs were low.

10. My task is to summarise the returns of each undertaking in the United Kingdom, as far as I have been able to obtain them.

They go back to 1890, and, in almost every case, end with the 1896 return, but where the 1897 return has been obtainable it is included.

It is, unfortunately, out of my power to include the costs of production in works established solely for the supply of electrical energy to tramways, these works being under no statutory obligation to file such returns. The figures tabulated by me refer to those undertakings that are operating under the Electric Lighting Acts, 1882–1888, having, of course, electric lighting for their main object, and only being assisted to a very slight extent by a demand for electrical energy for other purposes than lighting; though in the Provisional Orders under which they are operating it is made clear that they are constituted in their districts the suppliers of electrical energy for all purposes that may be demanded.

Mr. Hammond.

11. Table I. sets out, for all the undertakings in the kingdom, except the very smallest, the following data :—

(*a*) Units sold (as defined by Board of Trade form).

(*b*) Coals used per unit sold.

(*c*) Average price of coal throughout the year.

Costs *per unit sold* of—

(*d*) Coals or other fuel;

(*e*) Oil, waste, water, and stores;

(*f*) Wages (on generation and distribution);

(*g*) Repairs and maintenance;

(*h*) Total of (*d*), (*e*), (*f*), (*g*)—*i.e.*, *Works Costs*;

(*i*) Rent, rates, and taxes;

(*j*) Management—*i.e.*, salaries, stationery and printing, general establishment charges, law expenses, insurance, &c.

(*k*) *Total Costs.*

Mr. Hammond.

Table I.—Costs per Unit Sold.
Provincial Undertakings.

—	Pressure.	Year.	Year of Operation.	Units Sold (Kilowatt-(Hours).	Coal used per Unit.	Approx. Price of Coal per Ton.	Cost per Unit in Pence. Coal.	Oil, Waste, and Stores.	Wages.	Repairs and Maintenance.	Works Cost.	Rent, Rates, and Taxes.	Management Expenses.	Total Costs.
					Lbs.	s. d.	d.	d.	d.	d.	d.	d.	d.	d.
Aberdeen Corporation	L.	1895	1st	138,872	...	...	0·33	0·09	0·46	0·42	1·30	0·36	0·70	2·36
,, ,, ...	,,	1896	2nd	210,185	10·94	4 4	0·25	0·08	0·59	0·76	1·68	0·40	0·56	2·64
Aberystwith Company	H.	1896	2nd	36,681	...	...	1·71	0·35	1·47	0·34	3·87	...	1·56	5·43
Ayr Corporation...	H.	1896	1st	124,924	32·2	5 4	0·92	0·20	1·15	0·37	2·64	0·13	0·84	3·61
Bedford Corporation	H.	1895	1st	45,500	...	...	2·10	0·37	1·76	0·03	4·26	0·32	0·97	5·55
,, ,, ...	,,	1896	2nd	158,236	...	...	1·35	0·15	0·92	0·12	2 54	0·19	0·40	3·13
,, ,, ...	,,	1897	3rd	255,990	...	...	1·24	0·13	0·67	0·20	2·24	0·18	0·33	2·70
Belfast Corporation (*managed by Gas Committee*)	L.	1895	1st	82,771	...	...	1·45	0·23	1·29	0·49	3·46	0·68	1·28	5·42
,, ,,	,,	1896	2nd	149,721	..	...	1·33	0·19	0·87	0·53	2·92	0·87	0·72	4·01
Birmingham Company	H.& L.	1892	1st	214,088	...	...	0·92	0·25	1·18	0·26	2·61	0·10	1·48	4·19
,, ,, ...	,,	1893	2nd	352,096	...	...	0·88	0·18	0·85	0·43	2·34	0·84	0 87	3·55
,, ,, ...	,,	1894	3rd	396,952	...	...	0·98	0·17	0·60	0·32	2·07	0·41	1·06	3·54
,, ,, ...	,,	1895	4th	490,646	...	...	0·72	0·21	0·54	0·28	1 75	0·42	0·94	3·11
,, ,, ...	,,	1896	5th	756,428	...	...	0·71	0·19	0·56	0·22	1·68	0·28	0·82	2·78
Blackburn Corporation... ...	L.	1896	1st	157,000	11·7	6 3	0·39	0·18	0·58	0·11	1·26	0·27	1·07	2·60

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Table I.—Costs per Unit Sold—*continued.*

Provincial Undertakings—continued.

—	Pressure	Year.	Year of Operation.	Units Sold (Kilowatt-Hours).	Coal used per Unit.	Approx. Price of Coal per Ton.	Cost per Unit in Pence. Coal.	Oil, Waste, and Stores.	Wages.	Repairs and Maintenance.	Works Cost.	Rent, Rates, and Taxes.	Management Expenses.	Total Costs.
					Lbs.	s. d.	d.	d.	d.	d.	d.	d.	d.	d.
Blackpool Corporation	H.	1894	1st	217,085	...	...	1·19	0·56	1·10	0·28	3·13	0·16	0·54	3·83
,, ,, ...	,,	1895	2nd	356,129	...	...	1·15	0·48	0·75	0·40	2·78	0·10	0·43	3·31
,, ,, ...	,,	1896	3rd	429,669	...	...	1·21	0·40	0·68	0 48	2·77	0·11	0 53	3·41
Bolton Corporation	H.	1895	1st	94,914	...	...	0·56	0·23	1 80	0·21	2·80	0·46	0·92	4·18
,, ,, ...	,,	1896	2nd	186,956	...	...	0·40	0·26	1·21	0·11	1·98	0·29	0·54	2·81
Bournemouth Company ...	H.	1892	3rd	132,343	...	..	1·93	0·29	1·34	0·69	4·25	0·32	1 46	6·03
,, ,, ...	,,	1893	4th	159,016	...	...	1·87	0·29	1·13	0·82	4·11	0·27	1·25	5·63
,, ,, ...	,,	1894	5th	214,374	...	...	2·00	0·43	1·15	1·09	4·67	0·35	0·89	5·91
,, ,, ...	,,	1895	6th	243,816	...	...	1·85	0·47	1·22	0 43	3·97	0·24	0·86	5·07
,, ,, ...	,,	1896	7th	281,310	...	...	1·43	0·41	0·74	1·11	3·69	0·10	1·23	5·02
Bradford Corporation	L.	1890	1st	107,907	...	...	1·09	0·15	1·73	0·67	3·64	0·70	0·24	4·60
,, ,, ...	,,	1891	2nd	239,361	...	...	0·63	0·07	0·93	0·70	2·34	0·30	0·31	2·97
,, ,, ...	,,	1892	3rd	365,411	...	...	0·46	0 06	1·11	0·54	2·18	0·25	0·13	2 57
,, ,, ...	,,	1893	4th	480,614	...	...	0·66	0·06	1·05	0·38	2·15	0·22	0·20	2·57
,, ,, ...	,,	1894	5th	554,633	...	...	0·58	0·16	0·70	0 58	2·02	0·20	0·32	2·54
,, ,, ...	,,	1895	6th	673,699	...	...	0·56	0·16	0·52	0·24	1·48	0·27	0·49	2·24
,, ,, ...	,,	1896	7th	813,628	9·6	7 9	0·40	0·11	0·42	0 10	1·03	0·31	0·47	1·81

Mr. Hammond.

Brighton Corporation	H.& L.	1892	1st	156,110	...	...	1·14	0·22	1·09	0·06	2·51	0·08	1·17	3·76
,, ,, ...	,,	1893	2nd	286,895	...	...	0·91	0·17	1·01	0·26	2·35	-0·03	0·64	2·96
,, ,, ...	,,	1894	3rd	583,701	...	...	0·83	0·23	0·39	0·36	1·81	0·01	0·48	2·30
,, ,, ...	,,	1895	4th	867,494	...	...	0·72	0·15	0·47	0·36	1·70	0·09	0·57	2·36
,, ,, ...	,,	1896	5th	1,388,821	6·7	19 1	0·69	0·10	0·35	0·30	1·44	0·21	0·39	2·04
Bristol Corporation	H	1894	1st	293,523	...	...	1·30	0·15	1·00	0·21	2·66	0·93	0·84	4·43
,, ,, ...	,,	1895	2nd	408,301	...	...	0·97	0·09	0 74	0·16	1·96	0·61	0·62	3·19
,, ,, ...	,	1896	3rd	650,758	16·2	8 9	0·76	0·08	0·53	0·26	1·63	0·37	0·50	2·50
Burnley Corporation	L.	1894	1st	74,383	..	...	0·62	0·17	0·83	0·21	1·83	0·53	0·77	3·1
,, ,, ...	,,	1895	2nd	124,938	...	...	0·38	0·11	0·60	0·24	1·33	0·33	0·56	2 22
,, ,, ...	,,	1896	3rd	173,152	...	..	0·39	0·12	0·52	0·31	1·34	0·25	0·48	2·0 7
Burton Corporation (*managed by Gas Committee*)	H.	1895	1st	40,094	...	..	1·33	0·54	4·72	0·29	6·88	0·42	0·42	7·72
,, ,,	,,	1896	2nd	58,192	...	...	0·58	0·23	3·39	0·19	4·39	0·28	0·39	5·06
Cambridge Company	H.	1894	2nd	112,084	...	...	2·37	0·27	1·26	0·58	4·48	0·43	0·36	5·27
,, ,, ...	,,	1895	3rd	150,510	...	...	1·98	0·19	1·13	0·61	3·91	0·84	0·28	4·53
,, ,, ...	,,	1896	4th	197,615	...	...	1·41	0·15	0 85	0·26	2·67	0·25	0·87	3·79
,, ,, ...	,,	1897	5th	221,507	...	...	1·19	0·15	0·75	0·45	2·54	0·37	0·84	3·75
Cardiff Corporation	H.	1895	1st	175,078	...	...	0·64	0·31	1·79	0·36	3·10	0·04	0·55	3·69
,, ,, ...	,,	1896	2nd	308,430	...	...	0·63	0·40	1·28	0·33	2·64	0·06	0·36	3·06
Cheltenham Corporation ...	H.	1896	1st	103,715	...	...	0·98	0·16	1 80	0·39	3·33	0·17	0·67	4·17
Coventry Corporation	H.	1896	1st	51,114	32·9	5 2	0·91	0·19	1·72	0·18	3·00	0·36	2·68	6·04

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Table I.—Costs per Unit Sold—*continued.*

Provincial Undertakings—continued.

—	Pressure.	Year.	Year of Operation.	Units Sold (Kilowatt-Hours).	Coal used per Unit.	Approx. Price of Coal per Ton.	Cost per Unit in Pence. Coal.	Oil, Waste, and Stores.	Wages.	Repairs and Maintenance.	Works Cost.	Rent, Rates, and Taxes.	Management Expenses.	Total Costs.
					Lbs.	s. d.	d.	d.	d.	d.	d.	d.	d.	d.
Dewsbury Corporation	L.	1895	1st	58,109	...	...	0·98	0·22	1·41	0·58	3·19	0·18	1·61	4·98
" "	"	1896	2nd	150,878	...	7 6	0·65	0·23	0·70	0·25	1·83	0·19	1·28	3·30
Dover Company	H.	1896	1st	154,200	...	19 3	1·62	0·18	0·82	0·27	2·89	0·18	1·86	4·93
Dublin Corporation	H.	1893	1st	346,998	...	...	1·44	0·31	1·30	0·26	3·31	0·18	0·60	4·09
" "	"	1894	2nd	...	...	...	...	...	...	...	...	...	...	...
" "	"	1895	3rd	453,294	...	...	1·79	0·12	1·06	0·39	3·36	0·15	0·42	3·93
" "	"	1896	4th	473,547	...	...	1·60	0·15	1·11	0·63	3·49	0·15	0·56	4·20
Dundee Corporation	L.	1893	1st	...	...	...	...	...	...	...	...	...	...	...
" "	"	1894	2nd	169,225	...	...	0·96	0·22	0·78	0·82	2·78	0·42	0·36	3·56
" "	"	1895	3rd	222,934	...	...	0·61	0·19	0·38	0·40	1·58	0·25	0·47	2·30
Ealing Corporation	H.	1896	2nd	246,902	19·0	18 0	1 84	0·30	0·72	0·28	3·14	0 23	0·54	3·91
Eastbourne Company	H.	1891	9th	126,038	...	...	2·18	0·22	1·19	0·70	4·29	0·97	1·21	6·47
" "	"	1892	10th	107,842	...	...	1·72	0·19	1·61	0·61	4·13	0·51	1·18	5·82
" "	"	1893	11th	124,663	...	...	1·42	0·15	1·55	0·75	3 87	0·48	0·85	5·15
" "	"	1894	12th	147,348	...	...	1·37	0·17	1·36	0·41	3·31	0 06	0·97	4·34
" "	"	1895	13th	175,007	...	...	1 87	0·25	1·43	0·50	3·55	0·18	0·83	4·51
" "	"	1896	14th	208,096	...	19 10	1·62	0·36	1·01	0·44	3·43	0·21	1·05	4·69

Mr. Hammond.

Edinburgh Corporation ...	H.& L.	1895	1st	888,385	...	...	0·38	0·09	0·30	0·15	0·92	0·23	0·52	1·67
,, ,, ...	,,	1896	2nd	1,721,557	8·3	7 2	0·31	0·06	0·20	0·06	0·63	0·17	0·33	1·13
Exeter Company ...	H.	1895	6th	110,000	...	...	1·18	0·57	1·48	0·55	3·78	0·12	0·64	4·54
Glasgow Corporation ...	L.	1892	1st	287,712	...	...	1·40	0·36	1·29	0·62	3·67	0·21	0·63	4·51
,, ...	,,	1893	2nd	702,248	...	...	0·59	0·12	0·66	1·20	2·57	0·16	0·55	3·28
,, ...	,,	1894	3rd	901,287	...	...	0·59	0·10	0·60	0·95	2·24	0·35	0·43	3·02
,, ...	,,	1895	4th	1,090,959	...	...	0·51	0·07	0·45	0·59	1·62	0·32	0·50	2·44
,, ...	,,	1896	5th	1 497,842	...	7 2½	0·45	0·07	0·32	0·48	1·32	0·25	0·35	1·92
Halifax Corporation ...	H.	1895	1st	119,028	...	...	0·97	0·52	1·58	0·11	3·18	0·47	1·06	4·71
,, ,, ...	,,	1896	2nd	177,531	24·7	6 9	0·90	0·40	1·02	0·10	2·42	0·37	0·78	3·57
Hanley	H.	1895	1st	180,469	...	...	0·74	0·25	0·62	0·48	2·09	0·16	0·55	2·80
,, ...	,,	1896	2nd	247,881	...	5 9	0·73	0·19	0·55	0·43	1·90	0·15	0·37	2·42
Hastings Company ...	H.	1894	12th	199,284	...	...	1·46	0·16	0·76	0·83	3·21	0·10	1·00	4·31
,, ,, ...	,,	1895	13th	265,846	...	...	1·76	0·18	0·68	0·42	3·04	0·10	0·74	3·88
,, ,, ...	,,	1896	14th	294,350	...	19 3	1·98	0·17	0·55	0·52	3·22	0·16	0·59	3·97
Hove Company	L.	1893	1st	49,170	...	...	1·58	0·25	1·74	0·61	4·13	0·61	2·11	6·85
,, ,, ...	,,	1894	2nd	110,818	...	...	1·31	0·25	1·14	0·50	3·20	0·35	1·87	5·42
,, ,, ...	,,	1895	3rd	162,428	...	...	1·05	0·28	1·00	0·20	2·53	0·28	1·35	4·16
,, ,, ...	,,	1896	4th	195,915	7·6	19 6	0·80	0·16	0·64	0·42	2·02	0·27	1·56	3·85
Huddersfield Corporation ...	H.	1894	1st	156,169	...	...	0·64	0·02	1·08	0·11	1·85	0·47	0·96	3·28
,, , ...	,,	1895	2nd	227,753	...	...	0·57	0·03	0·72	0·25	1·59	0·35	0·93	2·87
,, ,, ...	,,	1896	3rd	304,163	...	...	0·48	0·05	0·59	0·19	1·31	0·25	0·81	2·37

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Table I.—COSTS PER UNIT SOLD—*continued.*

Provincial Undertakings—continued.

—	Pressure.	Year.	Year of Operation.	Units Sold (Kilowatt-Hours).	Coal used per Unit.	Approx. Price of Coal per Ton.	Cost per Unit in Pence.							
							Coal.	Oil, Waste, and Stores.	Wages.	Repairs and Maintenance.	Works Cost.	Rent, Rates, and Taxes.	Management Expenses.	Total Costs.
					Lbs.	s. d.	d.	d.	d.	d.	d.	d.	d.	d.
Hull Corporation	L.	1893	1st	75,000	...	...	1·38	0·19	1·63	0·17	3·37	0·43	1·28	5·03
,, ,, ...	,,	1894	2nd	163,857	...	...	0·73	0·09	0·99	0·28	2·09	0·21	0·56	2·86
,, ,, ...	,,	1895	3rd	246,277	...	...	0·62	0·12	0·58	0·63	1·95	0·14	0·61	2·70
,, ,, ...	,,	1896	4th	340,439	9·5	9 $7\frac{3}{4}$	0·50	0·09	0·53	0·50	1·62	0·15	0·54	2·31
Kelvinside Company	L.	1894	1st	13,790	...	...	1·27	0·33	6·63	0·27	8·50	2·30	7·62	18·42
,, ,, ...	,,	1895	2nd	37,600	...	...	0·57	0·13	1·78	0·10	2·58	0·85	3·01	6·44
,, ,, ...	,,	1896	3rd	63,467	14·1	5 6	0·44	0·08	0·95	0·10	1·57	0·47	1·77	3·81
Kingston-on-Thames Corporation	H.	1894	1st	128,267	...	...	2·02	0·25	1·09	0·78	4·14	0·08	0·58	4·80
,, ,,	,,	1895	2nd	134,084	...	...	1·97	0·27	1·07	0·75	4·06	0·19	0·52	4·77
,, ,,	,,	1896	3rd	155,681	26·7	12 6	1·80	0·22	0·93	0·27	3·22	0·08	0·50	3·80
Lancaster Corporation	L.	1895	1st	78,264	...	...	1·06	0·34	1·31	0·26	2·97	0·10	0·73	3·80
,, ,, ...	,,	1896	2nd	106,125	14·3	9 9	0·75	0·23	0·79	0·15	1·92	0·19	0·48	2·59
Leeds Company	H.	1894	1st	291,113	...	...	0·58	0·18	1·19	0·22	2·17	0·20	0·74	3·11
,, ,, ...	,,	1895	2nd	524,629	...	...	0·30	0·16	0·81	0·19	1·46	0·03	0·56	2·05
,, ,, ...	,,	1896	3rd	701,409	...	...	0·29	0·07	0·38	0·22	0·96	0·08	0·69	1·73
,, ,, ...	,,	1897	4th	833,280	...	...	0·25	0·05	0·35	0·13	0·78	0·08	0·64	1·50

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Leicester Corporation (*managed by Gas Committee*)	H.	1895	1st	77,797	...	..	1·04	0·46	2·77	0·77	5·04	Nil	0·68	5·72
,, ,,	,,	1896	2nd	169,668	24·3	5 7	0·74	0·19	1·57	0·42	2·92	0·30	0·56	3·78
Liverpool Corporation (*Company up to* 1896)	L.	*1891	8th	580,131	...	...	0 96	0·21	0·48	0·60	2·25	0·10	0·67	3·02
,, ,, ,,	,,	*1892	9th	796,281	...	...	0·73	0·15	0·39	0·57	1·84	0·02	0·57	2·43
,, ,, ,,	,,	*1893	10th	873,325	...	...	0·93	0·16	0·38	0·64	2·11	0·06	0·69	2·86
,, ,, ,,	,,	*1894	11th	1,016,178	...	...	0·68	0·09	0 26	0·59	1·62	0·10	0·74	2·46
,, ,, ,,	,,	*1895	12th	1,185,964	...	...	0·55	0·10	0·22	0·56	1·43	0·26	0·75	2·44
,, ,, ,,	,,	1896	13th	844,617†	...	...	0·50	0·07	0 32	0·25	1·14	0·07	0·56	1·77
Manchester Corporation ...	L.	1894	1st	1,168,382	...	...	0·50	0·23	0·55	0·21	1·49	0·19	0·49	2·17
,, ,, ...	,,	1895	2nd	1,748,244	...	...	0·42	0·15	0·38	0·27	1·22	0·22	0·36	1·80
,, ,, ..	,,	1896	3rd	2,508,588	...	...	0·40	0·11	0·26	0·17	0·94	0·20	0·31	1·45
Nelson Corporation	L.	1896	4th	68,768	...	..	0·15	0·12	0·77	0·17	1·21	0·20	0·26	1 67
Newcastle District Company ...	H.	1891	1st	206,017	...	...	1·24	0·40	0·77	0 10	2·51	0·34	0·54	3·39
,, ,, ,, ...	,,	1892	2nd	290,469	...	...	1·13	0·37	0 87	0·27	2·64	0·26	0·21	3·11
,, ,, ,, ..	,,	1893	3rd	388,422	...	...	0·83	0·28	0·85	0·14	2·10	0·22	0·31	2·63
,, ,, ,, ...	,,	1894	4th	431,239	...	...	0·71	0·20	0·75	0·16	1·82	0·24	0·39	2·45
,, ,, ,, ..	,,	1895	5th	471,662		...	0·63	0·19	0·77	0·12	1·71	0·28	0·39	2·38
,, ,, ,, ...	,,	1896	6th	541,139	17·6	6 6	0 62	0·18	0·51	0·13	1·44	0·27	0·58	2·29
Newcastle-on-Tyne Company ...	H	1891	1st	244,470	...	...	0·66	0·22	1·10	0·24	2·22	0·30	0·38	2·90
,, ,, ...	,,	1892	2nd	335,141	...	...	0·72	0·25	0·80	0·58	2·35	0·17	0·48	3·00
,, ,, ...	,,	1893	3rd	360,467	...	...	0·63	0·18	0·77	0·53	2·11	0·17	0·61	2·89

† Half-year only.

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Table I.—Costs per Unit Sold—*continued.*
Provincial Undertakings—continued.

—	Pressure.	Year.	Year of Operation.	Units Sold (Kilowatt-Hours).	Coal used per Unit.	Approx. Price of Coal per Ton.	Cost per Unit in Pence. Coal.	Oil, Waste, and Stores.	Wages.	Repairs and Maintenance.	Works Cost.	Rent, Rates, and Taxes.	Management Expenses.	Total Costs.
					Lbs.	s. d.	d.	d.	d.	d.	d.	d.	d.	d.
Newcastle-on-Tyne Company—*continued*	H.	1894	4th	401,066	...	...	0·57	0·16	0·72	0·57	2·02	0·25	0·53	2·80
,, ,, ...	,,	1895	5th	448,832	...	...	0·49	0·13	0·71	0·47	1·80	0 21	0·44	2·45
,, ,, ...	,,	1896	6th	535,953	17·7	4 9	0·47	0·12	0·49	0·42	1·50	0·12	0·71	2·33
Newport (Mon.) Corporation ...	H.	1895	1st	91,557	...	..	0·66	0·45	0·93	0·07	2·11	0·22	0·88	3·21
,, ,,	,,	1896	2nd	195,932	...	6 2	0·55	0·27	0·98	0·06	1·86	0·20	0·70	2·76
Northampton Company ...	L.	1892	1st	38,293	...	...	1·64	0·32	1·77	0·33	4·06	0·34	1·36	5·76
,, ,, ...	,,	1893	2nd	44,485	...	...	1·50	0·37	1·91	0·10	3·88	0·17	1·01	5·06
,, ,, ...	,,	1894	3rd	59,474	...	...	1·51	0·20	1·55	0·31	3·57	0·02	0·83	4·42
,, ,, ...	,,	1895	4th	67,764	...	...	1·14	0·18	1·56	0·95	3·83	0·14	0·59	4·56
,, ,, ...	,,	1896	5th	89,445	...	...	1·05	0·21	0·56	0·45	2·27	0·13	1·31	3·71
,, ,, ...	,,	1897	6th	114,676	...	...	1·06	0·19	0·74	0·33	2·32	0·23	1·39	3·94
Norwich Company	L.	1894	1st	186,500	...	...	1·13	0·15	0·74	0·17	2·19	Nil	0·35	2·54
,, ,, ...	,,	1895	2nd	299,650	...	...	0·90	0·14	0 52	0·42	1·98	Nil	0·44	2·42
,, ,, ...	,,	1896	3rd	436,050	...	...	0·74	0·10	0·34	0·41	1·59	0·07	0·45	2·11
Nottingham Corporation ...	L	1895	1st	171,654	...	...	0·61	0·15	0·52	0·37	1·65	0·06	1·08	2·79
,, ,,	,,	1896	2nd	297,185	13·6	7 9	0·57	0·12	0·39	0·21	1·29	0·19	0·69	2·17

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Oldham Corporation	L.	1894	1st	68,998	...	...	0·59	0·04	1·15	0·40	2·18	0·62	1·13	3·93
,, ,, ...	,,	1895	2nd	147,432	...	...	0·48	0·05	0·62	0·25	1·40	0·27	0·75	2·42
,, ,, ...	,,	1896	3rd	227,982	...	...	0·47	0·05	0·45	0·91	1·88	0·25	0·53	2·66
Oswestry Company	L.	1896	1st	15,453	...	...	0·61	0·28	1·64	0·15	2·68	0·20	1·08	3·96
Oxford Company	H.c.	1893	1st	103,895	...	...	0·72	0·06	1·88	1·34	3·50	0·51	1·70	5·71
,, ,, ...	,,	1894	2nd	157,257	...	...	0·65	0·05	0·92	0·58	2·20	0·35	1·17	3·72
,, ,, ...	,,	1895	3rd	207,633	...	...	0·55	0·05	0·83	0·43	1·86	0·34	1·16	3·36
,, ,, ...	,,	1896	4th	291,640	5·8	18 4	0·58	0·06	0·60	0·78	2·02	0·25	1·17	3·44
Pontypool Company	L.	1895	2nd	32,271	...	...	0·41	0·34	2·10	0·54	3·39	0·65	0·44	4·48
,, ,, ...	,,	1896	3rd	35,011	9·9	6 0	0·33	0·22	1·27	0·27	2·09	0·54	1·65	4·28
Portsmouth Corporation ...	H.	1895	1st	406,118	...	...	0·67	0·16	0·40	0·29	1·52	0·11	0·43	2·06
,, ,, ...	,,	1896	2nd	839,392	...	10 10	0·56	0·15	0·22	0·39	1·32	0·09	0·30	1·71
Preston Company	L.	1893	1st	149,636	...	...	1·65	0·48	1·38	0·66	4·17	0·52	2·29	6·98
,, ,, ...	,,	1894	2nd	220,867	...	...	0·82	0·17	0·90	0·43	2·32	0·29	1·34	3·95
,, ,, ...	,,	1895	3rd	271,076	...	...	0·44	0·12	0·64	0·27	1·47	0·28	1·02	2·77
,, ,, ...	,,	1896	4th	320,500	...	...	0·38	0·10	0·33	0·19	1·00	0·22	0·72	1·94
,, ,, ...	,,	1897	5th	371,301	...	...	0·34	0·08	0·28	0·27	0·97	0·16	0·93	2·06
Reading Company	H.	1895	1st	42,596	...	...	3·04	0·38	1·80	0·50	5·72	0·50	1·50	7·72
,, ,, ...	,,	1896	2nd	82,165	26·0	14 0	1·79	0·21	1·33	0·26	3·59	0·55	1·07	5·21
Richmond Company	L.	1895	2nd	80,456	...	...	1·33	0·42	2·17	0·20	4·12	0·49	1·93	6·54
,, ,, ...	,,	1896	3rd	97,044	10·1	19 3	1·09	0·14	0·95	0·52	2·70	0·71	1·44	4·85

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Table I.—COSTS PER UNIT SOLD—*continued.*

Provincial Undertakings—continued.

—	Pressure.	Year.	Year of Operation.	Units Sold (Kilowatt-Hours).	Coal used per Unit.	Approx. Price of Coal per Ton.	Cost per Unit in Pence. Coal.	Oil, Waste, and Stores.	Wages.	Repairs and Maintenance.	Works Cost.	Rent, Rates, and Taxes.	Management Expenses.	Total Costs.
					Lbs.	s. d.	d.	d.	d.	d.	d.	d.	d.	d.
Salford Corporation (*managed by Gas Committee*)	H.	1896	1st	52,486	...	...	3·10	0·72	3·91	1·62	9·35	1·04	2·18	12·57
Scarborough Company	H.	1894	1st	86,594	...	..	1·87	0·13	1·57	0·70	4·27	0·16	0·40	4·83
,, ,, ...	,,	1895	2nd	135,177	...	...	1·66	0·12	1·06	0·63	3·47	0·13	0·76	4·36
,, ,, ...	,,	1896	3rd	174,515	...	...	1·24	0·10	0·73	0·48	2·55	0·12	0·96	3·63
Sheffield Company	H.	1894	7th	192,220	...	...	1·00	0·14	1·20	0·42	2·76	0·45	0·59	3·80
,, ,, ...	,,	1895	8th	288,406	...	...	0·88	0·17	1·05	0·62	2·72	0·28	0·65	3·65
,, ,, ...	,,	1896	9th	483,427	...	4 11	0·58	0·09	0·51	0·30	1·48	0·17	0·55	2·20
Shrewsbury Company	L.	1896	1st	28,820	...	...	1·10	0·83	1·19	0·09	2·71	0·48	1·88	5·07
Southampton Local Authority	L.	*1891	1st	5,469	...	...	3·70	2·08	8·85	0·19	14·82	1·64	15·78	32·24
,, ,, ,,	,,	*1892	2nd	35,537	...	...	1·88	0·85	2·75	0·25	5·23	0·41	2·63	8·27
,, ,, ,,	,,	*1893	3rd	58,848	...	..	1·21	0·24	1·67	0·23	3·35	0·86	0·61	4·32
,, ,, ,,	,,	*1894	4th	85,501	...	...	1·34	0·22	1·42	0·48	3·41	0·22	0·78	4·41
,, ,, ,,	,,	*1895	5th	100,478	...	...	1·30	0·19	1·29	0·45	3·23	0·23	1·06	4·52
,, ,, ,,	,,	1896	6th	181,843	12·7	16 9	1·16	0·14	0·66	0·81	2·27	0·24	1·16	3·67
Southport Corporation ...	H.	1895	1st	89,979	...	...	0·79	0·21	1·20	0·24	2·44	0·39	0·88	3·71
,, ,, ...	,,	1896	2nd	245,515	14·3	6 0½	0·47	0·10	0·91	0·17	1·65	0·14	0·52	2·31

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Stafford Corporation (*managed by the Gas Committee*)	L.	1896	1st	48,619	...	...	0·24	0·11	0·74	1·07	2·16	0·78	0·89	3·28
Sunderland Corporation ...	H.& L.	1895	1st	95,446	...	...	0·42	0·10	0·85	0·11	1·48	0·46	1·70	3·64
,, ,, ...	,,	1896	2nd	146,440	...	4 10½	0·50	0·21	0·98	0·31	2·00	0·47	0·79	3·26
Taunton Corporation	H.	*1894	4th	100,495	...	...	1·95	0·48	0·79	0·32	3·54	0·09	0·84	4·47
,, ,, ...	,,	*1895	5th	108,286	...	...	1·59	0·25	0·82	0·20	2·86	0·09	0·96	3·91
,, ,, ...	,,	*1896	6th	126,840	17·1	15 8	1·33	0·21	0·71	0·25	2·50	0·10	0·89	3·49
Tunbridge Wells Corporation ..	H.	1896	1st	174,053	...	...	1·40	0·08	0·58	0·16	2·22	0·11	0·49	2·82
Walsall Corporation	H.c.	1896	1st	67,170	20·7	5 6	0·64	0·20	0·91	0·47	2·22	0·60	1·06	3·88
Whitehaven Corporation ...	L.	1896	3rd	178,378	10·2	8 9	0·49	0·12	0·54	0·29	1·44	0·09	0·22	1·75
Wolverhampton Corporation ...	H.c.	1895	1st	191,701	...	...	0·62	0·21	0·69	0·31	1·83	0·15	0·84	2·82
,, ,, ...	,,	1896	2nd	224,709	...	...	0·54	0·21	0·69	0·44	1·88	0·13	0·70	2·71
Worcester Corporation.. ...	H.	1895	1st	246,912	...	...	1·06	0·16	0·99	0·10	2·31	0·04	0·39	2·74
,, ,, ...	,,	1896	2nd	333,644	...	...	0·64	0·09	0·72	0·09	1·54	0·06	0·51	2·11
Yarmouth Corporation	H.	1895	1st	114,645	...	...	1·33	0·32	0·62	0·20	2·47	0·12	0·58	3·17
,, ,, ...	,,	1896	2nd	157,254	17·4	14 3	1·43	0·35	0·60	0·13	2·51	0·10	0·66	3·27
,, ,, ...	,,	1897	3rd	190,000	...	...	1·36	0·25	0·50	0·13	2·24	0·08	0·66	2·98

* These undertakings were the properties of Companies during these years.

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Table I.—Costs per Unit Sold—*continued.*

Metropolitan Undertakings.

—	Pressure.	Year.	Year of Operation.	Units Sold (Kilowatt-Hours).	Coal used per Unit.	Approx. Price of Coal per Ton.	Price per Unit in Pence. Coal.	Oil, Waste, and Stores.	Wages.	Repairs and Maintenance.	Works Cost.	Rent, Rates, and Taxes.	Management Expenses.	Total Costs.
					Lbs.	s. d.	d.	d.	d.	d.	d.	d.	d.	d.
Charing Cross (London) ...	L.	1892	1st	652,703	..	...	1·59	0·21	0·81	0·39	3·01	0·04	0·56	3·60
" " ...	"	1893	2nd	934,276	...	...	1·13	0·19	0·62	0·47	2·41	0·09	0·43	2·93
" " ...	"	1894	3rd	1,115,609	...	...	1·06	0·13	0·54	0·37	2·10	0·09	0·39	2·58
" " ...	"	1895	4th	1,383,813	...	...	0·91	0·08	0·50	0·35	1·84	0·05	0·32	2·21
" " ...	"	1896	5th	1,944,402	9·0	17 6	0·87	0·08	0·32	0·25	1 52	0·27	0·51	2·30
Chelsea Company (London) ...	H.c.	1891	2nd	290,458	...	...	2·22	0·74	1·11	0·93	5·00	0·21	1·56	6·77
" " " ...	"	1892	3rd	357,162	...	...	1·63	0·59	0·95	0·71	3·88	0·21	1·26	5·35
" " " ...	"	1893	4th	402,848	...	...	1·11	0·35	0·80	0·53	2·79	0·36	1·17	4·32
" " " ...	"	1894	5th	469,416	...	...	1·06	0·19	0·68	0·38	2·31	0·37	1·01	3·69
" " " ...	"	1895	6th	577,770	...	...	0·92	0·20	0·63	0·30	2·05	0·22	0·96	3·23
" " " ...	"	1896	7th	813,764	8·4	19 5	0·88	0·20	0·49	0·27	1·84	0·36	0·88	3·08
City of London Company ...	H.	1892	1st	506,400	...	...	1·91	0·74	1·28	0·37	4·30	0·43	0·54	5·27
" " ...	"	1893	2nd	1,741,099	...	...	1·27	0·39	0·87	0·82	2·85	0·08	0·21	3·14
" " ...	"	1894	3rd	2,602,217	...	...	1·26	0·38	0·91	0·23	2·78	0·19	0·35	3·32
" " ...	"	1895	4th	3,845,096	...	...	0·98	0·24	0·80	0·44	2·46	0·50	0·54	3·50
" " ...	"	1893	5th	5,488,500	...	...	0·81	0·16	0·43	0·62	2·02	0·54	0·96	3 52
Crystal Palace Company ...	H.c.	1896	3rd	118,316	15·7	20 8	1·88	0·25	1·82	1·66	5·61	1·27	1·85	8·73

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Hampstead Corporation ...	H.	1895	1st	192,527	...	...	2·58	0·44	1·19	0·52	4·68	0·49	1·49	6·66
,, ,, ...	,,	1896	2nd	547,920	...	14 3	1·36	0·15	0·48	0·46	2·45	0·19	0·66	3·30
House-to-House Company ...	H.	1891	2nd	247,861	...	...	2·08	0·57	1·66	0·53	4·79	0·38	0·71	5·88
,, ,, ...	,,	1892	3rd	318,484	...	...	2·20	0·60	1·58	1·52	4·90	0·32	0·70	5·92
,, ,, ...	,,	1893	4th	358,506	...	...	1·93	0·45	1·30	0·38	4·06	0·31	0·78	5·15
,, ,, ...	,,	1894	5th	400,911	...	...	1·71	0·40	1·02	0·25	3·38	0·23	0·73	4·34
,, ,, ...	,,	1895	6th	476,714	...	...	1·66	0·32	0·94	0·28	3·20	0·26	0·78	4·24
,, ,, ...	,,	1896	7th	643,693	...	...	0·99	0·13	0·48	0·25	1·85	0·32	0·96	3·13
Islington Corporation	H.	1896	1st	297,834	...	...	1·50	0·46	1·20	0·20	3·36	0·30	0·92	4·58
,, ,,	,,	1897	2nd	503,572	...	...	1·14	0·37	0·82	0·26	2·59	0·23	0·75	3·57
Kensington and Knightsbridge Company	L.	1891	4th	385,050	...	...	...	2·58	...	1·00	3·53	0·36	1·02	4·91
,, ,,	,,	1892	5th	535,343	...	...	0·92	0·19	0·70	0·96	2·77	0·32	0·76	3·85
,, ,,	,,	1893	6th	724,308	...	...	0·74	0·19	0·63	0·92	2·48	0·21	0·76	3·45
,, ,,	,,	1894	7th	977,797	...	...	0·72	0·14	0·60	0·65	2·11	0·18	0·57	2·86
,, ,,	,,	1895	8th	1,228,734	...	...	0·68	0·13	0·54	0·47	1·82	0·21	0·54	2·57
,, ,,	,,	1896	9th	1,514,729	6·6	18 6	0·67	0·11	0·31	0·48	1·57	0·34	0·75	2·66
Metropolitan Electric Company	H. & L.	1891	3rd	1,527,741	...	...	2·15	0·40	1·12	0·54	4·21	0·41	0·72	5·34
,, ,, ,,	,,	1892	4th	2,035,000	...	...	1·80	0·25	0·78	0·45	3·28	0·31	0·69	4·28
,, ,, ,,	,,	1893	5th						No data for this year.					
,, ,, ,,	,,	1894	6th	2,941,550	...	...	1·90	0·31	0·70	0·59	3·50	0·30	0·72	4·52
,, ,, ,,	,,	1895	7th	3,661,895	...	...	1·90	0·27	0·64	0·67	3·48	0·21	0·72	4·41
,, ,, ,,	,,	1896	8th	4,075,000	...	...	1·82	0·22	0·38	0·57	2·99	0·26	0·88	4·13

Mr. Hammond.

Table I.—Costs per Unit Sold—*continued.*

Metropolitan Undertakings—continued.

—	Pressure.	Year.	Year of Operation.	Units Sold (Kilowatt-Hours).	Coal used per Unit.	Approx. Price of Coal per Ton.	Cost per Unit in Pence. Coal	Oil, Waste, and Stores.	Wages.	Repairs and Maintenance.	Works Cost.	Rent, Rates, and Taxes.	Management Expenses.	Total Costs.
					Lbs.	s. d.	d.	d.	d.	d.	d.	d.	d.	d.
Notting Hill Company	L.	1891	1st	29,985	...	...	1·28	0·43	2·11	2·41	6·23	-0·25	6·35	12·33
,, ,, ...	,,	1892	2nd	75,667	...	...	1·37	0·36	2·20	1·93	5·86	0·38	2·69	8·93
,, ,, ...	,,	1893	3rd	107,580	...	...	0·96	0·28	1·13	1·38	3·75	...	1·54	5·29
,, ,, ...	,,	1894	4th	130,266	...	...	0·85	0·21	0·65	0·94	2·65	0·10	2·00	4·55
,, ,, ...	,,	1895	5th	182,327		...	0·79	0·18	0·48	1·02	2·42	0·05	1·76	4·23
,, ,, ...	,,	1896	6th	230,787	7·0	18 9	0·71	0·18	0·43	0·46	1·78	0·27	1·69	3·74
,, ,, ...	,,	1897	7th	354,969	...	...	0·66	0·09	0·33	0·44	1·52	0·26	1·34	3·12
St James's Company	L.	1891	2nd	1,067,996	...	...	1·05	0·17	0·76	0·50	2·48	0·19	0·98	3·65
,, ,, ...	,,	1892	3rd	1,186,826	...	...	0·94	0·12	0·76	0·48	2·25	0·08	0·82	3·15
,, ,, ...	,,	1893	4th	1,211,451	...	...	0·87	0·14	0·78	0·63	2·42	0·12	1·14	3·68
,, ,, ...	,,	1894	5th	1,569,884	...	...	0·75	0·18	0·70	0·35	1·93	0·25	0·76	2·94
,, ,, ...	,,	1895	6th	1,846,064	...	...	0·59	0·09	0·59	0·40	1·67	0·26	0·81	2·74
,, ,, ...	,,	1896	7th	2,401,431	6·4	14 0	0·50	0·08	0·37	0·30	1·25	0·27	0·77	2·29
,, ,, ...	,,	1897	8th	3,028,242	...	...	0·51	0·07	0·29	0·40	1·27	0·21	0·71	2·19
St. Pancras Corporation ...	L.	1892	1st	433,519	...	...	1·66	0·34	1·36	0·19	3·35	Nil	1·11	4·66
,, ,, ...	,,	1893	2nd	549 898	...	...	1·28	0·29	1·00	0·59	3·11	Nil	0·44	3·55
,, ,, ...	,,	1894	3rd	719,484	...	...	0·99	0·15	0·69	0·63	2·46	0·10	0·47	3·03

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,, ,, .	,,	1895	4th	849,987	...	...	0·95	0·13	0·65	0·99	2·72	0·11	0·49	3·32
,, ,, ..	,,	1896	5th	1,201,229	...	...	0·99	0·17	0·72	0·92	2·80	0·10	0·70	3·60
Westminster Company	L.	1891	1st	627,500	...	...	1·37	0·38	1·45	0·13	3·33	0·60	1·39	5·32
,, ,, ...	,,	1892	2nd	1,217,871	...	..	1·04	0·25	1·38	0·31	2·93	0·40	0·91	4·24
,, ,, ...	,,	1893	3rd	1,704,615	...	...	0·78	0·19	0·83	0·47	2·27	0·31	0·85	3·43
,, ,, ...	,,	1894	4th	2,173,298	...	...	0·65	0·13	0·70	0·26	1·74	0·18	0·77	2·69
,, ,, ...	,,	1895	5th	2,830,396	...	...	0·58	0·11	0·57	0·25	1·51	0·13	0·67	2·31
,, ,, ...	,,	1896	6th	3,503,054	6·5	15 4½	0·53	0·09	0·39	0·23	1·24	0·16	0·69	2·09
,, ,, ...	,,	1897	7th	4,355,781	..	..	0·63	0·09	0·34	0·23	1·29	0·22	0·68	2·19
Woolwich Company	H.	1895	2nd	54,885	...	...	1·82	0·33	1·20	0·25	3·60	0·77	0·80	5·17
,, ,, ...	,,	1896	3rd	75,929	18·1	18 2	1·75	0·29	0·80	0·44	3·28	0·75	0·62	4·65

Mr. Hammond.

12. It may be objected that in going beyond the analysis of the *Works Costs* I am stepping out of the path of the engineer into that of the statistician; and I should admit the justice of the criticism had the Board of Trade form, in the case of local authorities, not located engineers' salaries, &c., under the head of "Management."

As, by this arrangement, *Works Costs* become bereft of the important item of superintendence, I feel compelled, in order to make my returns complete, to bring in the whole of the management items, though agreeing that the bulk are not chargeable to the engineering department.

On the other hand, general use of electrical energy will depend not entirely on the *Works Costs*, but upon the gross cost of the energy delivered to the consumer; and though the engineer may have no responsibility for the outlays on management—not even the fixing of his own salary—I feel that my analyses of *Costs* would be incomplete if I did not include all the items that contributed. In this connection I may repeat that in only the 1896 and 1897 analyses are the salaries of engineers, in the case of companies, included under the heading of "Management," as in the case of local authorities.

13. In analysing the returns there are a few pitfalls that one has to take care to avoid in order to secure uniformity, and these have been carefully considered in the compilation of the accompanying tables, with the result that I can say, with confidence, that the costs per unit sold of each undertaking are directly comparable.

14. Here, then, are the statistics relating to the costs of production and distribution of electrical energy in the United Kingdom.

The *Engineer*, in a recent comment on Mr. John S. Raworth's presidential address to the Northern Society of Electrical Engineers, said (January 14th, 1898):

"In dealing with the question of economy the electrician has "placed himself in a somewhat curious position. It is known "very well that as matters stand electricity is costly. It is only "under very peculiar circumstances, with which the general

"public have little to do, that it can be obtained for less than 5d. Mr. Hammond.
"per unit; a higher price is the rule.

"Mr. Raworth's anticipations are based on a rate very much "less than this. We have heard similar arguments before. They "all come to the same thing in the long run. Electricity can, "we are told, be generated much more cheaply than it is at "present produced. Now the electricians never cease to tell us "that their dynamos have an efficiency already of over 90 per "cent.; sometimes we hear of 98 per cent. How is further "economy to be effected when such machines are in use? As to "engines, we know exactly what can be saved by them, and it is "very small. As a matter of fact, electricity can be generated, "confining ourselves to the engine and dynamo alone, at about "1¼d. per unit. But there are a dozen charges to be added on, "which much enhance its cost. How are these to be got rid of? "Mr. Raworth placed before his hearers a very pretty picture "indeed of what cities are to be in future; but, even if all that "he spoke of were possible, the result would not be quite so "satisfactory, perhaps, as he wished his hearers to think. We "may, however, put his fancies and forecasts on one side, and "confine our attention to facts. They are very few and simple. "The use of electricity is spreading daily. There is more and "better lighting being done, and electricity will in time clear our "tunnels of foul air, and will do something to cleanse the "atmosphere of our manufacturing towns. In all this we believe. "Our faith gives way when we are told that all these and many "other blessings will be had for a small price. How this is to be "brought about neither Mr. Raworth nor anyone else has been "able to explain in a way that carries conviction.

"Electricity is an admirable servant, and is popular accord-"ingly; but, like most other good servants, it is very expensive. "For about 20 years we have been told that its cost is to be "reduced next year. The reduction has not come yet; and the "worst of the matter is that all the large producing companies, "who ought to know—and who are for the most part keenly "competitive—tell us that they see no prospect of being able to "reduce the price."

Mr. Hammond.

I am struck by the foregoing sentence:

> "*For about* 20 *years we have been told that its*
> "*cost is to be reduced next year. The reduction has not*
> "*come yet.*"

If the writer referred to the cost to the consumer, I would point out that the prices at present charged by the supply undertakings are on the average less than one-third of those fixed by the pioneer concerns that started into existence about 20 years ago.

At Leeds our average price this year will be 4d. per unit, compared with 8d. per unit five years ago; and at Brighton the charge is 1½d. per unit for all consumption beyond an average of one hour per day, the first hour's consumption being charged at 7d. per unit.

If, however, the writer refers to the cost of production and distribution, the figures set forth in Table I. will tend, I trust, completely to dispel this misconception.

Take, as an example, the costs achieved by Mr. Crompton's company at Kensington, the oldest established works in the metropolis:—

KENSINGTON WORKS.

Year.	No. of Units Sold.	Works Costs per Unit Sold.	Total Costs per Unit Sold.
		d.	d.
1891 ...	385,050 ...	3·53 ...	4·91
1892 ...	535,343 ...	2·77 ...	3·85
1893 ...	724,308 ...	2·48 ...	3·45
1894 ...	977,797 ...	2·11 ...	2·86
1895 ...	1,228,734 ...	1·82 ...	2·57
1896 ...	1,514,729 ...	1·57 ...	2·66

Or the works designed by Professor Kennedy for the Westminster Company:—

Mr. Hammond.

WESTMINSTER WORKS.

Year.	No. of Units Sold.	Works Costs per Unit Sold. d.	Total Costs per Unit Sold. d.
1891	627,500	3·33	5·32
1892	1,217,871	2·93	4·24
1893	1,704,615	2·27	3·44
1894	2,173,298	1·74	2·69
1895	2,830,396	1·51	2·31
1896	3,503,054	1·24	2·09
1897	4,355,781	1·29	2·19

Or the works designed by Dr. Hopkinson for the Manchester Corporation :—

MANCHESTER WORKS.

Year.	No. of Units Sold.	Works Costs per Unit Sold. d.	Total Costs per Unit Sold. d.
1894	1,168,382	1·49	2·17
1895	1,748,244	1·22	1·80
1896	2,508,588	0·94	1·45

Or those laid down by me at Leeds :—

LEEDS WORKS.

Year.	No. of Units Sold.	Works Costs per Unit Sold. d.	Total Costs per Unit Sold. d.
1894	291,113	2·17	3·11
1895	524,629	1·46	2·05
1896	701,409	0·96	1·73
1897	833,280	0·78	1·50

Or those laid down by Professor Kennedy at Edinburgh :—

EDINBURGH WORKS.

Year.	No. of Units Sold.	Works Costs per Unit Sold. d.	Total Costs per Unit Sold. d.
1895	888,335	0·92	1·67
1896	1,721,557	0·63	1·13

With only two exceptions, the costs show a decrease on those of the preceding year.

Table I. indeed provides conclusive evidence that costs of production and distribution tend to decrease year by year as the output increases.

The obvious inference is that on still increased outputs the costs per unit will continue to decrease.

Mr. Hammond.

Table II.—Costs per Unit Sold for 1894, 1895, and 1896, arranged in Order of Merit.

Coal.

1894. Works.	Year of Operations.	Units Sold.	Cost.	1895. Works.	Year of Operations.	Units Sold.	Cost.	1896. Works.	Year of Operations.	Units Sold.	Cost.
			d.				d.				d.
Manchester	1st	1,168,382	0·50	Leeds	2nd	524,629	0·30	Nelson	4th	68,768	0·15
Newcastle-on-Tyne	4th	401,066	0·57	Aberdeen	1st	138,372	0·33	Stafford	1st	48,619	0·24
Leeds	1st	291,113	0·58	Burnley	2nd	124,933	0·38	Aberdeen	2nd	210,185	0·25
Bradford	5th	554,633	0·58	Edinburgh	1st	888,335	0·38	Leeds	3rd	701,409	0·29
Oldham	1st	68,998	0·59	Pontypool	2nd	32,271	0·41	Edinburgh	2nd	1,721,557	0·31
Glasgow	3rd	901,287	0·59	Sunderland	1st	95,446	0·42	Pontypool	3rd	35,011	0·33
Burnley	1st	74,383	0·62	Manchester	2nd	1,748,244	0·42	Preston...	4th	320,500	0·38
Huddersfield ...	1st	156,169	0·64	Preston...	3rd	271,076	0·44	Blackburn	1st	157,000	0·39
Oxford	2nd	157,257	0·65	Oldham...	2nd	147,432	0·48	Burnley	3rd	173,152	0·39
Westminster ...	4th	2,173,298	0·65	Newcastle-on-Tyne	5th	448,832	0·49	Bolton	2nd	186,956	0·40
Liverpool	11th	1,016,178	0·68	Glasgow	4th	1,090,959	0·51	Bradford	7th	813,623	0·40
Newcastle District	4th	431,239	0·71	Oxford	3rd	207,633	0·55	Manchester	3rd	2,508,588	0·40
Kensington	7th	977,797	0·72	Liverpool	12th	1,185,964	0·55	Kelvinside	3rd	63,467	0·44
Hull	2nd	163,857	0·73	Bolton	1st	94,914	0·56	Glasgow	5th	1,497,842	0·45
St. James's	5th	1,569,884	0·75	Bradford	6th	673,699	0·56	Oldham	3rd	227,982	0·47
Preston...	2nd	220,867	0·82	Kelvinside	2nd	37,600	0·57	Southport	2nd	245,515	0·47
Brighton	3rd	583,701	0·83	Huddersfield ...	2nd	227,753	0·57	Newcastle-on-Tyne	6th	535,953	0·47

Mr. Hammond.

Notting Hill... ...	4th	130,266	0·85	Westminster ...	5th	2,830,896	0·58	Huddersfield ...	3rd	304,163	0·48
Dundee...	2nd	169,225	0·96	St. James's	6th	1,846,064	0·59	Whitehaven... ...	3rd	178,378	0·49
Birmingham ...	3rd	396,952	0·98	Nottingham	1st	171,654	0·61	Sunderland	2nd	146,440	0·50
St. Pancras	3rd	719,484	0·99	Dundee...	3rd	222,984	0·61	Hull	4th	340,489	0·50
Sheffield	7th	192,220	1·00	Wolverhampton ...	1st	191,701	0·62	Liverpool	18th	844,617	0·50
Chelsea...	5th	469,416	1·06	Hull	3rd	246,277	0·62	St. James's	7th	2,401,481	0·50
Charing Cross ...	3rd	1,115,609	1·06	Newcastle District	5th	471,662	0·63	Westminster ..	6th	3,503,054	0·53
Norwich	1st	186,500	1·18	Cardiff	1st	175,078	0·64	Wolverhampton ...	2nd	224,709	0·54
Blackpool	1st	217,085	1·19	Newport	1st	91,557	0·66	Newport	2nd	195,932	0·55
City of London ...	3rd	2,602,217	1·26	Portsmouth	1st	406,118	0·67	Portsmouth	2nd	839,392	0·56
Kelvinside	1st	13,790	1·27	Kensington	8th	1,228,784	0·68	Nottingham	2nd	297,185	0·57
Bristol	1st	293,523	1·30	Birmingham ...	4th	490,646	0·72	Burton	2nd	58,192	0·58
Hove	2nd	110,818	1·31	Brighton	4th	867,494	0·72	Oxford	4th	291,640	0·58
Southampton ...	4th	85,501	1·34	Hanley	1st	180,469	0·74	Sheffield	9th	483,427	0·58
Eastbourne	12th	147,348	1·37	Southport	1st	89,979	0·79	Oswestry	1st	15,453	0·61
Bath	4th	209,077	1·43	Notting Hill	5th	182,827	0·79	Newcastle District	6th	541,139	0·62
Hastings	12th	199,284	1·46	Sheffield	8th	288,406	0·88	Cardiff	2nd	308,430	0·63
Northampton ...	3rd	59,474	1·51	Norwich	2nd	299,650	0·90	Walsall... ...	1st	67,170	0·64
House-to-House ...	5th	400,911	1·71	Charing Cross ...	4th	1,383,813	0·91	Worcester	2nd	333,644	0·64
Scarborough ...	1st	86,594	1·87	Chelsea	6th	577,770	0·92	Dewsbury	2nd	150,878	0·65
Metropolitan ...	6th	2,941,550	1·90	St. Pancras	4th	849,987	0·95	Kensington	9th	1,514,729	0·67
Taunton	4th	100,495	1·95	Halifax	1st	119,028	0·97	Brighton	5th	1,388,821	0·69
Bournemouth ...	5th	214,874	2·00	Bristol	2nd	408,801	0·97	Notting Hill ..	6th	230,787	0·71
Kingston	1st	128,267	2·02	Dewsbury	1st	58,109	0·98	Birmingham ...	5th	756,428	0·71
Cambridge	2nd	112,084	2·37	City of London ...	4th	3,845,096	0·98	Hanley	2nd	247,881	0·73

Mr. Hammond.

Table II.—COSTS PER UNIT SOLD FOR 1894, 1895, AND 1896, ARRANGED IN ORDER OF MERIT.

Coal.

1894.				1895.				1896.			
Works.	Year of Operations.	Units Sold.	Cost.	Works.	Year of Operations.	Units Sold.	Cost.	Works.	Year of Operations.	Units Sold.	Cost.
			d.				d.				d.
Manchester	1st	1,168,382	0·50	Leeds	2nd	524,629	0·30	Nelson	4th	68,768	0·15
Newcastle-on-Tyne	4th	401,066	0·57	Aberdeen	1st	138,372	0·33	Stafford	1st	48,619	0·24
Leeds	1st	291,113	0·58	Burnley	2nd	124,933	0·38	Aberdeen	2nd	210,185	0·25
Bradford	5th	554,633	0·58	Edinburgh	1st	888,335	0·38	Leeds	3rd	701,409	0·29
Oldham	1st	68,998	0·59	Pontypool	2nd	32,271	0·41	Edinburgh	2nd	1,721,557	0·31
Glasgow	3rd	901,287	0·59	Sunderland	1st	95,446	0·42	Pontypool	3rd	35,011	0·33
Burnley	1st	74,383	0·62	Manchester	2nd	1,748,244	0 42	Preston	4th	320,500	0·38
Huddersfield ...	1st	156,169	0·64	Preston	3rd	271,076	0·44	Blackburn	1st	157,000	0·39
Oxford	2nd	157,257	0·65	Oldham...	2nd	147,432	0·48	Burnley	3rd	173,152	0·39
Westminster ...	4th	2,173,298	0·65	Newcastle-on-Tyne	5th	448,832	0·49	Bolton	2nd	186,956	0·40
Liverpool	11th	1,016,178	0·68	Glasgow	4th	1,090,959	0·51	Bradford	7th	813,623	0·40
Newcastle District	4th	431,239	0·71	Oxford	3rd	207,633	0·55	Manchester	3rd	2,508,588	0·40
Kensington	7th	977,797	0·72	Liverpool	12th	1,185,964	0·55	Kelvinside	3rd	63.467	0·44
Hull	2nd	163,857	0·73	Bolton	1st	94,914	0·56	Glasgow	5th	1,497,842	0·45
St. James's	5th	1,569,884	0·75	Bradford	6th	673,699	0·56	Oldham	3rd	227,982	0·47
Preston	2nd	220,867	0·82	Kelvinside	2nd	37,600	0·57	Southport	2nd	245,515	0·47
Brighton	3rd	588,701	0·83	Huddersfield ...	2nd	227,753	0·57	Newcastle-on-Tyne	6th	585,953	0·47

Mr. Hammond.

Notting Hill... ...	4th	130,266	0·85	Westminster ...	5th	2,830,396	0·58	Huddersfield ...	3rd	304,163	0·48
Dundee...	2nd	169,225	0·96	St. James's	6th	1,846,064	0·59	Whitehaven... ...	3rd	178,878	0·49
Birmingham ...	3rd	396,952	0·98	Nottingham... ...	1st	171,654	0·61	Sunderland	2nd	146,440	0·50
St. Pancras	3rd	719,484	0·99	Dundee...	3rd	222,984	0·61	Hull	4th	340,439	0·50
Sheffield	7th	192,220	1·00	Wolverhampton ...	1st	191,701	0·62	Liverpool	13th	844,617	0·50
Chelsea...	5th	469,416	1·06	Hull	3rd	246,277	0·62	St. James's	7th	2,401,431	0·50
Charing Cross ...	3rd	1,115,609	1·06	Newcastle District	5th	471,662	0·63	Westminster ..	6th	3,503,054	0·53
Norwich	1st	186,500	1·18	Cardiff	1st	175,078	0·64	Wolverhampton ...	2nd	224,709	0·54
Blackpool	1st	217,085	1·19	Newport	1st	91,557	0·66	Newport	2nd	195,932	0·55
City of London ...	3rd	2,602,217	1·26	Portsmouth	1st	406,118	0·67	Portsmouth	2nd	839,392	0·56
Kelvinside	1st	13,790	1·27	Kensington	8th	1,228,734	0·68	Nottingham	2nd	297,185	0·57
Bristol	1st	293,523	1·30	Birmingham ...	4th	490,646	0·72	Burton	2nd	58,192	0·58
Hove	2nd	110,818	1·31	Brighton	4th	867,494	0·72	Oxford	4th	291,640	0·58
Southampton ...	4th	85,501	1·34	Hanley	1st	180,469	0·74	Sheffield	9th	483,427	0·58
Eastbourne	12th	147,348	1·37	Southport	1st	89,979	0·79	Oswestry	1st	15,453	0·61
Bath	4th	209,077	1·43	Notting Hill	5th	182,327	0·79	Newcastle District	6th	541,139	0·62
Hastings	12th	199,284	1·46	Sheffield	8th	288,406	0·88	Cardiff	2nd	308,430	0·63
Northampton ...	3rd	59,474	1·51	Norwich	2nd	299,650	0·90	Walsall... ...	1st	67,170	0·64
House-to-House ...	5th	400,911	1·71	Charing Cross ...	4th	1,383,818	0·91	Worcester	2nd	333,644	0·64
Scarborough ...	1st	86,594	1·87	Chelsea	6th	577,770	0·92	Dewsbury	2nd	150,878	0·65
Metropolitan ...	6th	2,941,550	1·90	St. Pancras	4th	849,987	0·95	Kensington	9th	1,514,729	0·67
Taunton	4th	100,495	1·95	Halifax	1st	119,028	0·97	Brighton	5th	1,388,821	0·69
Bournemouth ...	5th	214,874	2·00	Bristol	2nd	408,801	0·97	Notting Hill ...	6th	230,787	0·71
Kingston	1st	128,267	2·02	Dewsbury	1st	58,109	0·98	Birmingham ...	5th	756,428	0·71
Cambridge	2nd	112,084	2·37	City of London ...	4th	3,845,096	0·98	Hanley	2nd	247,881	0·73

Mr. Hammond.

Table II.—COSTS PER UNIT SOLD FOR 1894, 1895, AND 1896, ARRANGED IN ORDER OF MERIT—*continued.*

Coal—continued.

1894.				1895.				1896.			
Works.	Year of Operations.	Units Sold.	Cost.	Works.	Year of Operations.	Units Sold.	Cost.	Works.	Year of Operations.	Units Sold.	Cost.
							d.				d.
				Leicester	1st	77,797	1·04	Leicester	2nd	169,668	0·74
				Hove	3rd	162,428	1·05	Norwich	3rd	436,050	0·74
				Lancaster	1st	78,264	1·06	Lancaster	2nd	106,125	0·75
				Worcester	1st	246,912	1·06	Bristol	3rd	650,758	0·76
				Northampton ..	4th	67,764	1·14	Hove	4th	195,915	0·80
				Blackpool	2nd	356,129	1·15	City of London ...	5th	5,488,500	0·81
				Exeter	6th	110,000	1·18	Charing Cross ...	5th	1,944,402	0·87
				Southampton ...	5th	100,473	1·30	Chelsea	7th	813,764	0·88
				Burton	1st	40,094	1·33	Halifax	2nd	177,531	0·90
				Richmond	2nd	80,456	1·33	Coventry	1st	51,114	0·91
				Yarmouth	1st	114,645	1·33	Ayr	1st	124,924	0·92
				Eastbourne	13th	175,007	1·37	Cheltenham ...	1st	103,715	0·98
				Belfast	1st	82,771	1·45	House-to-House ...	7th	643,693	0·99
				Taunton	5th	108,286	1·59	St. Pancras	5th	1,201,229	0·99
				Scarborough ..	2nd	135,177	1·66	Northampton ...	5th	89,445	1·05
				House-to-House ...	6th	476,71[illegible]	1·36	Richmond	3rd	97,044	1·09
				Hastings	13th	265,846	1·76	Shrewsbury	1st	23,820	1·10

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Dublin	3rd	458,294	1·79	Southampton ...	6th	131,843	1·16
Woolwich	2nd	54,885	1·82	Blackpool	3rd	429,669	1·21
Bournemouth ...	6th	248,816	1·85	Scarborough ...	3rd	174,515	1·24
Metropolitan ...	7th	3,661,895	1·90	Taunton	6th	126,840	1·33
Kingston	2nd	134,084	1·97	Belfast	2nd	149,721	1·33
Cambridge	3rd	150,510	1·98	Bedford...	2nd	158,236	1·35
Bedford	1st	45,500	2·10	Hampstead	2nd	547,920	1·36
Hampstead	1st	192,527	2·58	Tunbridge Wells	1st	174,053	1·40
Reading	1st	42,596	3·04	Cambridge	4th	197,615	1·41
				Yarmouth	2nd	157,254	1·43
				Bournemouth ...	7th	281,310	1·48
				Islingtón	1st	297,834	1·50
				Dublin	4th	473,547	1·60
				Dover	1st	154,200	1·62
				Eastbourne	14th	208,096	1·62
				Aberystwith... ...	2nd	36.681	1·71
				Woolwich ...	3rd	75,929	1·75
				Reading	2nd	82,165	1·79
				Kingston	3rd	155,681	1·80
				Metropolitan ...	8th	4,075,000	1·82
				Ealing	2nd	246,902	1·84
				Crystal Palace ...	3rd	118,316	1·88
				Hastings	14th	294,350	1·98
				Salford	1st	52,486	3·10

Mr. Hammond

Table III.—Costs per Unit Sold for 1894, 1895, and 1896, arranged in Order of Merit.

Oil, Waste, Water, and Stores.

1894.				1895.				1896.			
Works.	Year of Operations.	Units Sold.	Cost.	Works.	Year of Operations.	Units Sold.	Cost.	Works.	Year of Operations	Units Sold.	Cost.
			d.				d.				d.
Huddersfield ...	1st	156,169	0·02	Oldham...	2nd	147,432	0·05	Oldham...	3rd	227,982	0·05
Oldham	1st	68,998	0·04	Oxford	3rd	207,633	0·05	Huddersfield ...	3rd	304,163	0·05
Oxford	2nd	157,257	0·05	Huddersfield ...	2nd	227,753	0·05	Oxford	4th	291,640	0·06
Hull	2nd	163,857	0·09	Glasgow	4th	1,090,959	0·07	Edinburgh	2nd	1,721,557	0·06
Liverpool	11th	1,016,178	0·09	Charing Cross ...	4th	1,383,813	0·08	Leeds	3rd	701,409	0·07
Glasgow	3rd	901,287	0·10	Aberdeen	1st	138,372	0·09	Liverpool	13th	844,617	0·07
Scarborough... ...	1st	86,594	0·13	Bristol	2nd	408,301	0·09	Glasgow	5th	1,497,842	0·07
Charing Cross ...	3rd	1,115,609	0·13	Edinburgh	1st	888,335	0·09	Kelvinside	3rd	63,467	0·08
St. James's	5th	1,569,884	0·13	St. James's	6th	1,846,064	0·09	Tunbridge Wells	1st	174,053	0·08
Westminster ...	4th	2,173,298	0·13	Sunderland	1st	95,446	0·10	Aberdeen	2nd	210,185	0·08
Kensington	7th	977,797	0·14	Liverpool	12th	1,185,964	0·10	Bristol	3rd	650,758	0·08
Sheffield	7th	192,220	0·14	Burnley	2nd	124,933	0·11	Charing Cross ...	5th	1,944,402	0·08
Norwich	1st	186,500	0·15	Westminster ...	5th	2,830,896	0·11	St. James's	7th	2,401,431	0·08
Bristol	1st	293,523	0·15	Scarborough ...	2nd	135,177	0·12	Worcester	2nd	333,644	0·09
St. Pancras	3rd	719,484	0·15	Hull	3rd	246,277	0·12	Hull	4th	340,439	0·09
Hastings	12th	199,284	0·16	Preston	3rd	271,076	0·12	Sheffield	9th	483,427	0·09
Newcastle-on-Tyne	4th	401,066	0·16	Dublin	3rd	453,294	0·12	Westminster ...	6th	3,503,054	0·09

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Bradford	5th	554,033	0·16	Kelvinside	2nd	37,600	0·13	Scarborough ...	3rd	174,515	0·10
Burnley	1st	74,383	0·17	Notting Hill... ...	5th	182,327	0·13	Southport	2nd	245,515	0·10
Eastbourne	12th	147,348	0·17	Newcastle-on-Tyne	5th	448,832	0·13	Preston	4th	320,500	0·10
Preston...	2nd	220,867	0·17	St. Pancras	4th	849,987	0·13	Norwich	3rd	436,050	0·10
Birmingham ...	3rd	396,952	0·17	Kensington	8th	1,228,734	0·13	Brighton	5th	1,388,821	0·10
Leeds	1st	291,113	0·18	Norwich	2nd	299,650	0·14	Stafford...	1st	43,619	0·11
Chelsea...	5th	469,416	0·19	Nottingham	1st	171,654	0·15	Bradford	7th	813,623	0·11
Northampton ...	3rd	59,474	0·20	Brighton	4th	867,494	0·15	Kensington	9th	1,514,729	0·11
Newcastle District	4th	431,239	0·20	Manchester	2nd	1,748,244	0·15	Manchester	3rd	2,508,588	0·11
Notting Hill... ...	4th	130,266	0·21	Worcester	1st	246,912	0·16	Nelson	4th	68,768	0·12
Southampton ...	4th	85,501	0·22	Portsmouth	1st	406,118	0·16	Burnley	3rd	173,152	0·12
Dundee...	2nd	169,225	0·22	Leeds	2nd	524,629	0·16	Whitehaven... ...	3rd	178,378	0·12
Brighton	3rd	583,701	0·23	Bradford	6th	673,699	0·16	Nottingham	2nd	297,185	0·12
Manchester	1st	1,168,382	0·23	Sheffield	8th	288,406	0·17	Newcastle-on-Tyne	6th	535,953	0·12
Hove	2nd	110,818	0·25	Hastings	13th	265,846	0·18	House-to-House ...	7th	643,693	0·13
Kingston	1st	128,267	0·25	Northampton ...	4th	67,764	0·18	Richmond	3rd	97,044	0·14
Cambridge	2nd	112,084	0·27	Southampton ...	5th	100,473	0·19	Southampton ...	6th	131,843	0·14
Metropolitan ...	6th	2,941,550	0·31	Cambridge	3rd	150,510	0·19	Bedford	2nd	158,236	0·15
Bath	4th	209,077	0·32	Dundee...	3rd	222,934	0·19	Cambridge	4th	197,615	0·15
Kelvinside	1st	13,790	0·33	Newcastle District	5th	471,662	0·19	Hampstead	2nd	547,920	0·15
City of London ...	3rd	2,602,217	0·33	Chelsea	6th	577,770	0·20	Dublin	4th	573,547	0·15
House-to-House ...	5th	400,911	0·40	Southport	1st	89,979	0·21	Portsmouth	2nd	839,392	0·15
Bournemouth ...	5th	214,374	0·43	Wolverhampton ...	1st	191,701	0·21	Cheltenham	1st	103,715	0·16
Taunton	4th	100,495	0·48	Birmingham ...	4th	490,646	0·21	Hove	4th	195,915	0·16
Blackpool	1st	217,085	0·56	Dewsbury	1st	58,109	0·22	City of London ...	5th	5,488,500	0·16

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Table III.—COSTS PER UNIT SOLD FOR 1894, 1895, AND 1896, ARRANGED IN ORDER OF MERIT—*continued.*

Oil, Waste, Water, and Stores—continued.

1894.				1895.				1896.			
Works.	Year of Operations.	Units Sold.	Cost.	Works.	Year of Operations.	Units Sold.	Cost.	Works.	Year of Operations.	Units Sold.	Cost.
							d.				d.
				Belfast	1st	82,771	0·23	Hastings	14th	294,350	0·17
				City of London ..	4th	3,845,096	0·24	St. Pancras	5th	1,201,229	0·17
				Taunton	5th	108,286	0·25	Dover	1st	154,200	0·18
				Eastbourne	13th	175,007	0·25	Blackburn	1st	157,000	0·18
				Hanley	1st	180,469	0·25	Notting Hill... ...	6th	230,787	0·18
				Kingston	2nd	184,084	0·27	Newcastle District	6th	541,139	0·18
				Metropolitan ...	7th	3,661,895	0·27	Coventry	1st	51,114	0·19
				Hove	3rd	162,428	0·28	Belfast	2nd	149,721	0·19
				Cardiff	1st	175,078	0·31	Leicester	2nd	169,668	0·19
				Yarmouth	1st	114,615	0·32	Hanley	2nd	247,881	0·19
				House-to-House ...	6th	476,714	0·32	Birmingham... ...	5th	756,428	0·19
				Woolwich	2nd	54,885	0·33	Walsall	1st	67,170	0·20
				Pontypool	2nd	32,271	0·34	Ayr	1st	124,924	0·20
				Lancaster	1st	78,264	0·34	Chelsea	7th	813,764	0·20
				Bedford	1st	45,500	0·37	Reading...	2nd	82,165	0·21
				Reading	1st	42,596	0·38	Northampton ...	5th	89,445	0·21
				Richmond	2nd	80,456	0·42	Taunton	6th	126,840	0·21

Hampstead	1st	192,527	0·44	Sunderland	2nd	146,440	0·21
Newport	1st	91,557	0·45	Wolverhampton ..	2nd	224,709	0·21
Leicester	1st	77,797	0·46	Pontypool	3rd	35,011	0·22
Bournemouth ...	6th	243,816	0·47	Kingston	3rd	155,681	0·22
Blackpool	2nd	356,129	0·48	Metropolitan ...	8th	4,075,000	0·22
Halifax	1st	119,028	0·52	Burton	2nd	58,192	0·23
Burton	1st	40,094	0·54	Lancaster	2nd	106,125	0·23
Exeter	6th	110,000	0·57	Dewsbury	2nd	150,878	0·23
				Crystal Palace ...	3rd	118,816	0·25
				Bolton	2nd	186,956	0·26
				Newport	2nd	195,932	0·27
				Oswestry	1st	15,453	0·28
				Woolwich	3rd	75,929	0·29
				Ealing	2nd	246,902	0·30
				Shrewsbury	1st	23,820	0·33
				Aberystwith ...	2nd	36,681	0·35
				Yarmouth	2nd	157,254	0·35
				Eastbourne	14th	208,096	0·36
				Halifax	2nd	177,581	0·40
				Blackpool	3rd	429,669	0·40
				Cardiff	2nd	308,430	0·40
				Bournemouth ...	7th	281,310	0·41
				Islington	1st	297,834	0·46
				Salford	1st	52,48[illegible]	0·72

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Table IV.—COSTS PER UNIT SOLD FOR 1894, 1895, AND 1896, ARRANGED IN ORDER OF MERIT.

Wages (on Generation and Distribution).

1894.				1895.				1896.			
Works.	Year of Operations.	Units Sold.	Cost.	Works.	Year of Operations.	Units Sold.	Cost.	Works.	Year of Operations.	Units Sold.	Cost.
			d.				d.				d.
Liverpool	11th	1,016,178	0·26	Liverpool	12th	1,185,964	0·22	Edinburgh	2nd	1,721,557	0·20
Brighton	3rd	583,701	0·39	Edinburgh	1st	888,335	0·30	Portsmouth	2nd	839,392	0·22
Charing Cross ...	3rd	1,115,609	0·54	Dundee...	3rd	222,934	0·38	Manchester	3rd	2,508,588	0·26
Manchester	1st	1,168,382	0·55	Manchester	2nd	1,748,244	0·38	Kensington	9th	1,514,729	0·31
Birmingham ...	3rd	396,952	0·60	Portsmouth	1st	406,118	0·40	Liverpool	13th	844,617	0·32
Glasgow	3rd	901,287	0·60	Glasgow	4th	1,090,959	0·45	Glasgow	5th	1,497,842	0·32
Kensington	7th	977,797	0·60	Aberdeen	1st	138,372	0·46	Charing Cross ..	5th	1,944,402	0·32
Notting Hill... ...	4th	130,266	0·65	Brighton	4th	867,494	0·47	Preston...	4th	320,500	0·33
Chelsea...	5th	469,416	0·68	Notting Hill ...	5th	182,327	0·48	Norwich	3rd	436,050	0·34
St. Pancras	3rd	719,484	0·69	Charing Cross ...	4th	1,383,813	0·50	Brighton	5th	1,388,821	0·35
Bradford	5th	554,633	0·70	Nottingham	1st	171,654	0·52	St. James's	7th	2,401,431	0·37
St. James's	5th	1,569,884	0·70	Norwich	2nd	299,650	0·52	Leeds	3rd	701,409	0·38
Westminster ...	4th	2,173,298	0·70	Bradford	6th	673,699	0·52	Metropolitan ...	8th	4,075,000	0·38
Metropolitan ...	6th	2,941,550	0·70	Birmingham ...	4th	490,646	0·54	Nottingham	2nd	297,185	0·39
Newcastle-on-Tyne	4th	401,066	0·72	Kensington	8th	1,228,734	0·54	Westminster ...	6th	3,503,054	0·39
Norwich	1st	186,500	0·74	Westminster... ..	5th	2,830,396	0·57	Bradford	7th	813,623	0·42
Newcastle District	4th	431,239	0·75	Hull	3rd	246,277	0·58	Notting Hill ...	6th	230,787	0·43

Mr. Hammond.

Hastings	12th	199,284	0·76	St. James's	6th	1,846,064	0·59	City of London ...	5th	5,488,500	0·43
Dundee...	2nd	169,225	0·78	Burnley	2nd	124,933	0·60	Oldham...	3rd	227,982	0·45
Taunton	4th	100,495	0·79	Yarmouth	1st	114,645	0·62	Hampstead	2nd	547,920	0·48
Burnley	1st	74,383	0·83	Oldham	2nd	147,432	0·62	House-to-House ...	7th	643,693	0·48
Preston...	2nd	220,867	0·90	Hanley	1st	180,469	0·62	Newcastle-on-Tyne	6th	535,953	0·49
Oxford	2nd	157,257	0·92	Chelsea...	6th	577,770	0·63	Chelsea...	7th	813,764	0·49
City of London ...	3rd	2,602,217	0·92	Preston...	3rd	271,076	0·64	Sheffield	9th	483,427	0·51
Hull	2nd	163,857	0·99	Metropolitan ...	7th	3,661,895	0·64	Newcastle District	6th	541,139	0·51
Bristol	1st	293,523	1·00	St. Pancras	4th	849,987	0·65	Burnley	3rd	173,152	0·52
House-to-House ...	5th	400,911	1·02	Hastings	13th	265,846	0·68	Hull	4th	340,439	0·53
Bath	4th	209,077	1·07	Wolverhampton ...	1st	191,701	0·69	Bristol	3rd	650,758	0·53
Huddersfield ...	1st	156,169	1·08	Newcastle-on-Tyne	5th	448,832	0·71	Whitehaven... ...	3rd	178,378	0·54
Kingston	1st	128,267	1·09	Huddersfield ...	2nd	227,753	0·72	Hanley	2nd	247,881	0·55
Blackpool	1st	217,085	1·10	Bristol	2nd	408,301	0·74	Hastings	14th	294,350	0·55
Hove	2nd	110,818	1·14	Blackpool	2nd	356,129	0·75	Northampton ...	5th	89,445	0·56
Oldham...	1st	68,998	1·15	Newcastle District	5th	471,662	0·77	Birmingham ...	5th	756,428	0·56
Bournemouth ...	5th	214,874	1·15	City of London ..	4th	3,845,096	0·80	Blackburn	1st	157,000	0·58
Leeds	1st	291,113	1·19	Leeds	2nd	524,629	0·81	Tunbridge Wells	1st	174,053	0·58
Sheffield	7th	192,220	1·20	Taunton	5th	108,286	0·82	Aberdeen	2nd	210,185	0·59
Cambridge	2nd	112·084	1·26	Oxford	3rd	207,633	0·83	Huddersfield ...	3rd	304,163	0·59
Eastbourne	12th	147,348	1·36	Sunderland	1st	95,446	0·85	Oxford	4th	291,640	0·60
Southampton ...	4th	85,501	1·42	Newport	1st	91,557	0·93	Yarmouth	2nd	157,254	0·60
Northampton ...	3rd	59,474	1·55	House-to-House ...	6th	476,714	0·94	Hove	4th	195,915	0·64
Scarborough... ...	1st	86,594	1·57	Worcester	1st	246,912	0·99	Southampton ...	6th	131,843	0·66
Kelvinside	1st	13,790	6·63	Hove	3rd	162,428	1·00	Blackpool	3rd	429,669	0·68

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Table IV.—Costs per Unit Sold for 1894, 1895, and 1896, arranged in Order of Merit—*continued.*

Wages (on Generation and Distribution)—continued.

1894.				1895.				1896.			
Works.	Year of Operations.	Units Sold.	Cost.	Works.	Year of Operations.	Units Sold.	Cost.	Works.	Year of Operations.	Units Sold.	Cost.
							d.				d.
				Sheffield	8th	288,406	1·05	Wolverhampton ...	2nd	224,709	0·69
				Scarborough ...	2nd	135,177	1·06	Dewsbury	2nd	150,878	0·70
				Dublin	3rd	453,294	1·06	Taunton	6th	126,840	0·71
				Kingston	2nd	134,084	1·07	Ealing	2nd	246,902	0·72
				Cambridge	3rd	150,510	1·13	Worcester	2nd	333,644	0·72
				Hampstead	1st	192,527	1·19	St. Pancras	5th	1,201,229	0·72
				Woolwich	2nd	54,885	1·20	Scarborough ...	3rd	174,515	0·73
				Southport	1st	89,979	1·20	Stafford...	1st	43,619	0·74
				Bournemouth ...	6th	243,816	1·22	Bournemouth ..	7th	281,310	0·74
				Belfast	1st	82,771	1·29	Nelson	4th	68,768	0·77
				Southampton ...	5th	100,478	1·29	Lancaster	2nd	106,125	0·79
				Lancaster	1st	78,264	1·31	Woolwich	3rd	75,929	0·80
				Dewsbury	1st	58,109	1·41	Dover	1st	154,200	0·82
				Eastbourne	13th	175,007	1·43	Cambridge	4th	197,615	0·85
				Exeter	6th	110,000	1·48	Belfast	2nd	149,721	0·87
				Northampton ...	4th	67,764	1·56	Walsall	1st	67,170	0·91
				Halifax	1st	119,028	1·58	Southport	2nd	245,515	0·91

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Bedford...	1st	45,500	1·76	Bedford	2nd	158 236	0·92
Kelvinside	2nd	37,600	1·78	Kingston	3rd	155,681	0·93
Cardiff	1st	175,078	1·79	Kelvinside	3rd	63,467	0·95
Reading	1st	42,596	1·80	Richmond	3rd	97,044	0·95
Bolton	1st	94,914	1·80	Sunderland	2nd	146,440	0·98
Pontypool	2nd	32,271	2·10	Newport	2nd	195,932	0·98
Richmond	2nd	80,456	2·17	Eastbourne	14th	208,096	1·01
Leicester	1st	77,797	2·77	Halifax...	2nd	177,531	1·02
Burton	1st	40,094	4·72	Dublin	4th	473,547	1·11
				Ayr	1st	124,924	1·15
				Shrewsbury	1st	23,820	1·19
				Islington	1st	297,834	1·20
				Bolton	2nd	186,956	1·21
				Pontypool	3rd	35,011	1·27
				Cardiff	2nd	308,430	1·28
				Reading	2nd	82,165	1·33
				Aberystwith... ...	2nd	36,681	1·47
				Leicester	2nd	169,668	1·57
				Oswestry	1st	15,453	1·64
				Coventry	1st	51,114	1·72
				Cheltenham	1st	103,715	1·80
				Crystal Palace ...	3rd	118,316	1·82
				Burton	2nd	58,192	3·39
				Salford	1st	52,486	3·91

Mr. Hammond.

Table V.—COSTS PER UNIT SOLD FOR 1894, 1895, AND 1896, ARRANGED IN ORDER OF MERIT.

Repairs and Maintenance.

1894.				1895.				1896.			
Works.	Year of Operations.	Units Sold	Cost.	Works.	Year of Operations.	Units Sold.	Cost.	Works.	Year of Operations.	Units Sold.	Cost.
			d.				d.				d.
Huddersfield ...	1st	156,169	0·11	Bedford	1st	45,500	0·03	Newport	2nd	195,932	0·06
Newcastle District	4th	431,239	0·16	Newport	1st	91,557	0·07	Edinburgh	2nd	1,721,557	0·06
Norwich	1st	186,500	0·17	Worcester	1st	246,912	0·10	Shrewsbury	1st	23,820	0·09
Bath	4th	209,077	0·19	Kelvinside	2nd	37,600	0·10	Worcester	2nd	333,644	0·09
Burnley	1st	74,383	0·21	Sunderland	1st	95,446	0·11	Kelvinside	3rd	63,467	0·10
Bristol	1st	293,523	0·21	Halifax	1st	119,028	0·11	Halifax...	2nd	177,531	0·10
Manchester	1st	1,168,382	0·21	Newcastle District	5th	471,662	0·12	Bradford	7th	813,623	0·10
Leeds	1st	291,113	0·22	Edinburgh	1st	888,335	0·15	Blackburn	1st	157,000	0·11
House-to-House ...	5th	400,911	0·25	Bristol	2nd	408,301	0·16	Bolton	2nd	186,956	0·11
Westminster ...	4th	2,173,298	0·26	Leeds	2nd	524,629	0·19	Bedford	2nd	158,236	0·12
Kelvinside	1st	13,790	0·27	Richmond	2nd	80,456	0·20	Yarmouth	2nd	157,254	0·13
City of London ...	3rd	2,602,217	0·27	Taunton	5th	108,286	0·20	Newcastle District	6th	541,139	0·13
Hull	2nd	163,857	0·28	Yarmouth	1st	114,645	0·20	Oswestry	1st	15,453	0·15
Blackpool	1st	217,085	0·28	Hove	3rd	162,428	0·20	Lancaster	2nd	106,125	0·15
Northampton ...	3rd	59,474	0·31	Bolton	1st	94,914	0·21	Tunbridge Wells .	1st	174,053	0·16
Taunton	4th	100,495	0·32	Southport	1st	89,979	0·24	Nelson	4th	68,768	0·17
Birmingham ...	3rd	396,952	0·32	Burnley	2nd	124,933	0·24	Southport	2nd	245,515	0·17

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St. James's	5th	1,569,884	0·35	Bradford	6th	673,699	0·24	Manchester	3rd	2,508,588	0·17
Brighton	3rd	588,701	0·36	Woolwich	2nd	54,885	0·25	Coventry	1st	51,114	0·18
Charing Cross ...	3rd	1,115,609	0·37	Oldham	2nd	147,432	0·25	Burton	2nd	58,192	0·19
Chelsea	5th	469,416	0·38	Huddersfield ...	2nd	227,753	0·25	Huddersfield ...	3rd	304,163	0·19
Oldham...	1st	68,998	0·40	Westminster... ...	5th	2,830,396	0·25	Preston	4th	320,500	0·19
Eastbourne	12th	147,348	0·41	Lancaster	1st	78,264	0·26	Islington	1st	297,834	0·20
Sheffield	7th	192,220	0·42	Preston	3rd	271,076	0·27	Nottingham ...	2nd	297,185	0·21
Southampton ...	4th	85,501	0·43	Manchester	2nd	1,748,244	0·27	Leeds	3rd	701,409	0·22
Preston...	2nd	220,867	0·43	House-to-House ...	6th	476,714	0·28	Birmingham ...	5th	756,428	0·22
Hove	2nd	110,818	0·50	Birmingham ...	4th	490,646	0·28	Westminster ...	6th	3,503,054	0·23
Newcastle-on-Tyne	4th	401,066	0·57	Burton	1st	40,094	0·29	Taunton	6th	126,840	0·25
Cambridge	2nd	112,084	0·58	Portsmouth	1st	406,118	0·29	Dewsbury	2nd	150,878	0·25
Oxford	2nd	157,257	0·58	Chelsea	6th	577,770	0·30	House-to-House ...	7th	643,693	0·25
Bradford	5th	554,633	0·58	Wolverhampton ...	1st	191,701	0·31	Liverpool	13th	844,617	0·25
Liverpool	11th	1,016,178	0·59	Charing Cross ...	4th	1,383,813	0·35	Charing Cross ...	5th	1,944,402	0·25
Metropolitan ...	6th	2,941,550	0·59	Cardiff	2nd	175,078	0·36	Reading	2nd	82,165	0·26
St. Pancras	3rd	719,484	0·63	Brighton	4th	867,494	0·36	Cambridge	4th	197,615	0·26
Kensington	7th	977,797	0·65	Nottingham	1st	171,654	0·37	Bristol	3rd	650,758	0·26
Scarborough... ...	1st	86,594	0·70	Dublin	3rd	453,294	0·39	Pontypool	3rd	35,011	0·27
Kingston	1st	128,267	0·78	Dundee...	3rd	222,934	0·40	Dover	1st	154,200	0·27
Dundee	2nd	169,225	0·82	Blackpool	2nd	356,129	0·40	Kingston	3rd	155,681	0·27
Hastings	12th	199,284	0·83	St. James's	6th	1,846,064	0·40	Chelsea...	7th	813,764	0·27
Notting Hill ...	4th	130,266	0·94	Aberdeen	1st	138,372	0·42	Ealing	2nd	246,902	0·28
Glasgow	3rd	901,287	0·95	Hastings	13th	265,846	0·42	Whitehaven	3rd	178,378	0·29
Bournemouth ...	5th	214,374	1·09	Norwich	2nd	299,650	0·42	Sheffield	9th	483,427	0·30

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Table V.—COSTS PER UNIT SOLD FOR 1894, 1895, AND 1896, ARRANGED IN ORDER OF MERIT—*continued.*

Repairs and Maintenance—continued.

1894.				1895.				1896.			
Works.	Year of Operations.	Units Sold.	Cost.	Works.	Year of Operations.	Units Sold.	Cost.	Works.	Year of Operations.	Units Sold.	Cost.
							d.				d.
				Oxford	3rd	207,633	0·43	Brighton	5th	1,388,821	0·30
				Bournemouth ..	6th	243,816	0·43	St. James's	7th	2,401,431	0·30
				City of London ...	4th	3,845,096	0·44	Southampton ...	6th	131,843	0·31
				Southampton ...	5th	100,473	0·45	Sunderland	2nd	146,440	0·31
				Newcastle-on-Tyne	5th	448,832	0·47	Burnley	3rd	173,152	0·31
				Kensington	8th	1,228,734	0·47	Cardiff	2nd	308,430	0·33
				Hanley	1st	180,469	0·48	Aberystwith... ...	2nd	36,681	0·34
				Belfast	1st	82,771	0·49	Ayr	1st	124,924	0·37
				Reading	1st	42,596	0·50	Cheltenham... ...	1st	103,715	0·39
				Eastbourne	13th	175,007	0·50	Portsmouth	2nd	839,392	0·39
				Hampstead	1st	192,527	0·52	Norwich	3rd	436,050	0·41
				Pontypool	2nd	32,271	0·54	Leicester	2nd	169,668	0·42
				Exeter	6th	110,000	0·55	Hove	4th	195,915	0·42
				Liverpool	12th	1,185,964	0·56	Newcastle-on-Tyne	6th	535,953	0·42
				Dewsbury	1st	58,109	0·58	Hanley	2nd	247,881	0·43
				Glasgow	4th	1,090,959	0·59	Woolwich	3rd	75,929	0·44
				Cambridge	3rd	150,510	0·61	Eastbourne	14th	208.096	0·44

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Sheffield	8th	288,406	0·62	Wolverhampton ...	2nd	224,709	0·44
Scarborough ...	2nd	135,177	0·63	Northampton ..	5th	89,445	0·45
Hull	3rd	246,277	0·63	Notting Hill ...	6th	230.787	0·46
Metropolitan ...	7th	3,661,895	0·67	Hampstead	2nd	547,920	0·46
Kingston	2nd	134,084	0·75	Walsall...	1st	67,170	0·47
Leicester	1st	77,797	0·77	Scarborough ...	3rd	174,515	0·48
Northampton ...	4th	67,764	0·95	Blackpool	3rd	429,669	0·48
St. Pancras	4th	849,987	0·99	Glasgow	5th	1,497,842	0·48
Notting Hill ...	5th	182,327	1·02	Kensington	9th	1,514,729	0·48
				Hull	4th	340,439	0·50
				Richmond	3rd	97.044	0·52
				Hastings	14th	294,350	0·52
				Belfast	2nd	149,721	0·53
				Metropolitan ...	8th	4,075,000	0·57
				City of London ...	5th	5,488,500	0·62
				Dublin	4th	473,547	0·63
				Aberdeen	2nd	210,185	0·76
				Oxford	4th	291,640	0·78
				Oldham...	3rd	227,982	0·91
				St. Pancras	5th	1,201,229	0·92
				Stafford...	1st	43,619	1·07
				Bournemouth ...	7th	281,310	1·11
				Salford	1st	52,486	1·62
				Crystal Palace ...	3rd	118,316	1·66

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Table VI.—COSTS PER UNIT SOLD FOR 1894, 1895, AND 1896, ARRANGED IN ORDER OF MERIT.

Rent, Rates, and Taxes.

1894. Works.	Year of Operations.	Units Sold.	Cost. d.	1895. Works.	Year of Operations	Units Sold.	Cost. d.	1896. Works.	Year of Operations.	Units Sold.	Cost. d.
Norwich	1st	186,500	Nil	Leicester	1st	77,797	Nil	Aberystwith ...	2nd	36,681	Nil
Brighton	3rd	583,701	0·01	Norwich	2nd	299,650	Nil	Cardiff	2nd	308,430	0·06
Northampton ...	3rd	59,474	0·02	Leeds	2nd	524,629	0·03	Worcester	2nd	333,644	0·06
Eastbourne	12th	147,348	0·06	Cardiff	1st	175,078	0·04	Norwich	2nd	436,050	0·07
Kingston	1st	128,267	0·08	Worcester	1st	246,912	0·04	Liverpool	13th	844,617	0·07
Taunton	4th	100,495	0·09	Notting Hill ..	5th	182,327	0·05	Kingston	3rd	155,681	0·08
Charing Cross ..	3rd	1,115,609	0·09	Charing Cross ...	4th	1,383,813	0·05	Leeds	3rd	701,409	0·08
Notting Hill... ...	4th	130,266	0·10	Nottingham	1st	171,654	0·06	Whitehaven	3rd	178,378	0·09
Hastings	12th	199,284	0·10	Taunton	5th	108,286	0·09	Portsmouth	2nd	839,392	0·09
St. Pancras	3rd	719,484	0·10	Brighton	4th	867,494	0·09	Taunton	6th	126,840	0·10
Liverpool	11th	1,016,178	0·10	Lancaster	1st	78,264	0·10	Yarmouth	2nd	157,254	0·10
Scarborough ...	1st	86,594	0·16	Hastings	13th	265,846	0·10	Bournemouth ...	7th	281,310	0·10
Blackpool	1st	217,085	0·16	Blackpool	2nd	356,129	0·10	St. Pancras	5th	1,201,229	0·10
Kensington	7th	977,797	0·18	Portsmouth	1st	406,118	0·11	Tunbridge Wells...	1st	174,053	0·11
Westminster ...	4th	2,173,298	0·18	St. Pancras	4th	849,987	0·11	Blackpool	3rd	429,669	0·11
Manchester	1st	1,168,382	0·19	Exeter	6th	110,000	0·12	Scarborough ...	3rd	174,515	0·12
City of London ...	3rd	2,602,217	0·19	Yarmouth	1st	114,645	0·12	Newcastle-on-Tyne	6th	535,953	0·12

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Bath	4th	209,077	0·20	Scarborough ...	2nd	135,177	0·13	Northampton ...	5th	89,445	0·13
Leeds	1st	291,113	0·20	Eastbourne	13th	175,007	0·13	Ayr	1st	124,924	0·13
Bradford	5th	554,633	0·20	Westminster ...	5th	2,830,396	0·13	Wolverhampton ...	2nd	224,709	0·13
Hull	2nd	163,857	0·21	Northampton ...	4th	67,761	0·14	Southport	2nd	245,515	0·14
Southampton ..	4th	85,501	0·22	Hull	3rd	246,277	0·14	Hanley	2nd	247,881	0·15
House-to-House ...	5th	400,911	0·23	Wolverhampton ...	1st	191,701	0·15	Hull	4th	340,439	0·15
Newcastle District	4th	431,239	0·24	Dublin	3rd	453,294	0·15	Dublin	4th	473,547	0·15
Newcastle-on-Tyne	4th	401,066	0·25	Hanley	1st	180,469	0·16	Hastings	14th	294,350	0·16
St. James's	5th	1,569,884	0·25	Dewsbury	1st	58,109	0·18	Westminster ...	6th	3,503,054	0·16
Preston	2nd	220,867	0·29	Kingston	2nd	134,084	0·19	Cheltenham... ...	1st	103,715	0·17
Metropolitan ...	6th	2,941,550	0·30	Newcastle-on-Tyne	5th	448,832	0·21	Sheffield	9th	483,427	0·17
Hove	2nd	110,818	0·35	Kensington	8th	1,228,734	0·21	Edinburgh	2nd	1,721,557	0·17
Oxford	2nd	157,257	0·35	Metropolitan ..	7th	3,661,895	0·21	Dover	1st	154,200	0·18
Bournemouth ...	6th	214,374	0·35	Newport	1st	91,557	0·22	Lancaster	2nd	106,125	0·19
Glasgow	3rd	901,287	0·35	Chelsea...	6th	577,770	0·22	Dewsbury	2nd	150,878	0·19
Chelsea...	5th	469,416	0·37	Manchester	2nd	1,748,244	0·22	Bedford	2nd	158,236	0·19
Birmingham ...	3rd	396,952	0·41	Southampton ...	5th	100,473	0·23	Nottingham	2nd	297,185	0·19
Dundee	2nd	169,225	0·42	Edinburgh	1st	888,335	0·23	Hampstead	2nd	547,920	0·19
Cambridge	2nd	112,084	0·43	Bournemouth ...	6th	243,816	0·24	Oswestry	1st	15,453	0·20
Sheffield	7th	192,220	0·45	Dundee...	3rd	222,934	0·25	Nelson	4th	68,768	0·20
Huddersfield ...	1st	156,169	0·47	House-to-House ...	6th	476,714	0·26	Newport	2nd	195,932	0·20
Burnley	1st	74,383	0·53	Liverpool	12th	1,185,964	0·26	Manchester	3rd	2,508,588	0·20
Oldham	1st	68,998	0·62	St. James's	6th	1,846,046	0·26	Eastbourne	14th	208,036	0·21
Bristol	1st	293,523	0·93	Oldham...	2nd	147,432	0·27	Brighton	5th	1,388,821	0·21
Kelvinside	1st	13,790	2·30	Bradford	6th	673,699	0·27	Preston	4th	320,500	0·22

Mr. Hammond.

Table VI.—Costs per Unit Sold for 1894, 1895, and 1896, arranged in Order of Merit—*continued.*

Rent, Rates, and Taxes—continued.

1894.				1895.				1896.			
Works.	Year of Operations.	Units Sold.	Cost.	Works.	Year of Operations.	Units Sold.	Cost.	Works.	Year of Operations.	Units Sold.	Cost.
							d.				d.
				Hove	3rd	162,428	0·28	Ealing	2nd	246,902	0·23
				Preston	3rd	271,076	0·28	Southampton	6th	131,843	0·24
				Sheffield	8th	288,406	0·28	Burnley	3rd	173,152	0·25
				Newcastle District	5th	471,662	0·28	Cambridge	4th	197,615	0·25
				Bedford	1st	45,500	0·32	Oldham	3rd	227,982	0·25
				Glasgow	4th	1,090,959	0·32	Oxford	4th	291,640	0·25
				Burnley	2nd	124,933	0·33	Huddersfield	3rd	304,163	0·25
				Cambridge	3rd	150,510	0·34	Glasgow	5th	1,497,842	0·25
				Oxford	3rd	207,633	0·34	Metropolitan	8th	4,075,000	0·26
				Huddersfield	2nd	227,758	0·35	Blackburn	1st	157,000	0·27
				Aberdeen	1st	138,372	0·36	Hove	4th	195,915	0·27
				Southport	1st	89,979	0·39	Notting Hill	6th	230,787	0·27
				Burton	1st	40,094	0·42	Newcastle District	6th	541,139	0·27
				Birmingham	4th	490,646	0·42	Charing Cross	5th	1,944,402	0·27
				Bolton	1st	94,914	0·46	St. James's	7th	2,401,431	0·27
				Sunderland	1st	95,446	0·46	Burton	2nd	58,192	0·28
				Halifax	1st	119,028	0·47	Birmingham	5th	756,428	0·28

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Richmond	2nd	80,456	0·49	Bolton	2nd	186,956	0·29
Hampstead	1st	192,527	0·49	Leicester	2nd	169,668	0·30
Reading	1st	42,596	0·50	Islington	1st	297,834	0·30
City of London ...	4th	3,845,096	0·50	Bradford	7th	813,623	0·31
Bristol	2nd	408,301	0·61	House-to-House ...	7th	648,698	0·32
Pontypool	2nd	32,271	0·65	Kensington	9th	1,514,729	0·34
Belfast	1st	82,771	0·68	Coventry	1st	51,114	0·36
Woolwich	2nd	54,885	0·77	Chelsea...	7th	813,764	0·36
Kelvinside	2nd	37,600	0·85	Belfast	2nd	149,721	0·37
				Halifax...	2nd	177,531	0·37
				Bristol	3rd	650,758	0·37
				Aberdeen	2nd	210,185	0·40
				Kelvinside	3rd	63,467	0·47
				Sunderland	2nd	146,440	0·47
				Shrewsbury	1st	23,820	0·48
				Pontypool	3rd	35,011	0·54
				City of London ...	5th	5,488,500	0·54
				Reading	2nd	82,165	0·55
				Walsall...	1st	67,170	0·60
				Richmond	3rd	97,044	0·71
				Stafford...	1st	43,619	0·73
				Woolwich	3rd	75,929	0·75
				Salford	1st	52,486	1·04
				Crystal Palace ...	3rd	118,316	1·27

Mr. Hammond.

Table VII.—Costs per Unit Sold for 1894, 1895, and 1896, arranged in Order of Merit.

Management Expenses.

1894.				1895.				1896.			
Works.	Year of Operations.	Units Sold.	Cost.	Works.	Year of Operations.	Units Sold.	Cost.	Works.	Year of Operations.	Units Sold.	Cost.
			d.				d.				d.
City of London ...	3rd	2,602,217	0·30	Cambridge	3rd	150,510	0·28	Whitehaven... ...	1st	178,378	0·22
Bradford	5th	554,633	0·32	Charing Cross ...	4th	1,383,813	0·32	Nelson	4th	68,768	0·26
Norwich	1st	186,500	0·35	Manchester	2nd	1,748,244	0·36	Portsmouth	2nd	839,392	0·30
Cambridge	2nd	112,084	0·36	Worcester	1st	246,912	0·39	Manchester	3rd	2,508,588	0·31
Dundee...	2nd	169,225	0·36	Newcastle District	5th	471,662	0·39	Edinburgh	2nd	1,721,557	0·33
Newcastle District	4th	431,239	0·39	Burton	1st	40,094	0·42	Glasgow	5th	1,497,842	0·35
Charing Cross ...	3rd	1,115,609	0·39	Dublin	3rd	453,294	0·42	Cardiff	2nd	308,430	0·36
Scarborough ...	2nd	86,594	0·40	Blackpool	2nd	356,129	0·43	Hanley	2nd	247,881	0·37
Glasgow	3rd	901,287	0·43	Portsmouth	1st	406,118	0·43	Stafford	1st	43,619	0·39
St. Pancras	3rd	719,484	0·47	Pontypool	2nd	32,271	0·44	Burton	2nd	58,192	0·39
Brighton	3rd	583,701	0·48	Norwich	2nd	299,650	0·44	Brighton	5th	1,388,821	0·39
Bath	4th	209,077	0·49	Newcastle-on-Tyne	5th	448,832	0·44	Bedford	2nd	158,236	0·40
Manchester	1st	1,168,382	0·49	Dundee	3rd	222,934	0·47	Norwich	3rd	436,050	0·45
Newcastle-on-Tyne	4th	401,066	0·53	Bradford	6th	673,699	0·49	Bradford	7th	813,623	0·47
Blackpool	1st	217,085	0·54	St. Pancras	4th	849,987	0·49	Lancaster	2nd	106,125	0·48
Hull	2nd	163,857	0·56	Glasgow	4th	1,090,959	0·50	Burnley	3rd	173,152	0·48
Kensington	7th	977,797	0·57	Kingston	2nd	134,084	0·52	Tunbridge Wells	1st	174,053	0·49

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Kingston	1st	128,267	0·58	Edinburgh	1st	888,835	0·52	Bristol	3rd	650,758	0·50
Sheffield	7th	192,220	0·59	Kensington	8th	1,228,734	0·54	Kingston	3rd	155,681	0·50
Metropolitan ...	6th	2,941,550	0·72	City of London ...	4th	3,845,096	0·54	Worcester	2nd	333,644	0·51
House-to-House ...	5th	400,911	0·73	Cardiff	1st	175,078	·0·55	Charing Cross ...	5th	1,944,402	0·51
Liverpool	11th	1,016,178	0·74	Hanley	1st	180,469	0·55	Southport	2nd	245,515	0·52
Leeds	1st	291,113	0·74	Burnley	2nd	124,933	0·56	Oldham	3rd	227,982	0·53
St. James's	5th	1,569,884	0·76	Leeds	2nd	524,629	0·56	Blackpool	3rd	429,669	0·53
Barnley	1st	74,383	0·77	Brighton	4th	867,494	0·57	Bolton	2nd	186,956	0·54
Westminster ...	4th	2,173,298	0·77	Yarmouth	1st	114,645	0·58	Ealing	2nd	246,902	0·54
Southampton ...	4th	85,501	0·78	Northampton ...	4th	67,764	0·59	Hull	4th	340,439	0·54
Northampton ...	3rd	59,474	0·83	Hull	3rd	246,277	0·61	Sheffield	9th	483,427	0·55
Taunton	4th	100,495	0·84	Bristol	2nd	408,301	0·62	Leicester	2nd	169,668	0·56
Bristol	1st	293,523	0·84	Exeter	6th	110,000	0·64	Aberdeen	2nd	210,185	0·56
Bournemouth ...	5th	214,374	0·89	Sheffield	8th	288,406	0·65	Dublin	4th	473,547	0·56
Huddersfield ...	1st	156,169	0·96	Westminster ...	5th	2,830,396	0·67	Liverpool	13th	844,617	0·56
Eastbourne	12th	147,348	0·97	Leicester	1st	77,797	0·68	Newcastle District	6th	541,139	0·58
Hastings	12th	199,284	1·00	Aberdeen	1st	138,372	0·70	Hastings	14th	294,350	0·59
Chelsea	5th	469,416	1·01	Metropolitan ...	7th	3,661,895	0·72	Woolwich	3rd	75,929	0·62
Birmingham ...	3rd	396,952	1·06	Lancaster	1st	78,264	0·73	Yarmouth	2nd	157,254	0·66
Oldham	1st	68,998	1·13	Hastings	13th	265,846	0·74	Hampstead	2nd	547,920	0·66
Oxford	2nd	157,257	1·17	Oldham	2nd	147,432	0·75	Cheltenham... ...	1st	103,715	0·67
Preston	2nd	220,867	1·34	Liverpool	12th	1,185,964	0·75	Nottingham	2nd	297,185	0·69
Hove	2nd	110,818	1·87	Scarborough... ...	2nd	135,177	0·76	Leeds	3rd	701,409	0·69
Notting Hill ...	4th	130,266	2·00	House-to-House ...	6th	476,714	0·78	Westminster ..	6th	3,503,054	0·69
Kelvinside	1st	13,790	7·62	Woolwich	2nd	54,885	0·80	Newport	2nd	195,932	0·70

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Table VII.—Costs per Unit Sold for 1894, 1895, and 1896, arranged in Order of Merit—*continued.*

Management Expenses—continued.

1894.				1895.				1896.			
Works.	Year of Operations.	Units Sold.	Cost.	Works.	Year of Operations.	Units Sold.	Cost.	Works.	Year of Operations.	Units Sold.	Cost.
							d.				d.
				St. James's	6th	1,846,064	0·81	Wolverhampton	2nd	224,709	0·70
				Eastbourne	13th	175,007	0·83	St. Pancras	5th	1,201,229	0·70
				Wolverhampton	1st	191,701	0·84	Newcastle-on-Tyne	6th	535,953	0·71
				Bournemouth	6th	243,816	0·86	Belfast	2nd	149,721	0·72
				Southport	1st	89,979	0·88	Preston	4th	320,500	0·72
				Newport	1st	91,557	0·88	Kensington	9th	1,514,729	0·75
				Bolton	1st	94,914	0·92	St. James's	7th	2,401,431	0·77
				Huddersfield	2nd	227,753	0·93	Halifax	2nd	177,531	0·78
				Birmingham	4th	490,646	0·94	Sunderland	2nd	146,440	0·79
				Taunton	5th	108,286	0·96	Huddersfield	3rd	304,163	0·81
				Chelsea	6th	577,770	0·96	Birmingham	5th	756,428	0·82
				Bedford	1st	45,500	0·97	Ayr	1st	124,924	0·84
				Preston	3rd	271,076	1·02	Cambridge	4th	197,615	0·87
				Southampton	5th	100,473	1·06	Chelsea	7th	813,764	0·88
				Halifax	1st	119,028	1·06	Metropolitan	8th	4,075,000	0·88
				Nottingham	1st	171,654	1·08	Taunton	6th	126,840	0·89
				Oxford	3rd	207,638	1·16	Islington	1st	297,834	0·92

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Belfast	1st	82,771	1·28	Scarborough ...	3rd	174,515	0·96
Hove	3rd	162,428	1·35	House-to-House ...	7th	643,693	0·96
Hampstead	1st	192,527	1·49	City of London ...	5th	5,488,500	0·96
Reading	1st	42,596	1·50	Eastbourne	14th	208,096	1·05
Dewsbury	1st	58,109	1·61	Walsall...	1st	67,170	1·06
Sunderland	1st	95,446	1·70	Reading	2nd	82,165	1·07
Notting Hill	5th	182,337	1·76	Blackburn	1st	157,000	1·07
Richmond	2nd	80,456	1·93	Oswestry	1st	15,458	1·08
Kelvinside	2nd	37,600	3·01	Southampton ...	6th	131,843	1·16
				Oxford	4th	291,640	1·17
				Bournemouth ...	7th	281,310	1·23
				Dewsbury	2nd	150,878	1·28
				Northampton ...	5th	89,445	1·31
				Richmond	3rd	97,044	1·44
				Aberystwith... ...	2nd	36.681	1·56
				Hove	4th	195,915	1·56
				Pontypool	3rd	35,011	1·65
				Notting Hill ...	6th	230,787	1·69
				Kelvinside	3rd	63,467	1·77
				Crystal Palace ...	3rd	118,316	1·85
				Dover	1st	154,200	1·86
				Shrewsbury	1st	23,820	1·88
				Salford	1st	52,486	2·18
				Coventry	1st	51,114	2·68

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Table VIII.—Costs per Unit Sold for 1894, 1895, and 1896, arranged in Order of Merit.

Works Cost (being Totals of Figures in Tables II., III., IV., and V.).

1894.				1895.				1896.			
Works.	Year of Operations	Units Sold.	Cost.	Works.	Year of Operations.	Units Sold.	Cost.	Works.	Year of Operations	Units Sold.	Cost.
			d.				d.				d.
Manchester	1st	1,168,382	1·49	Edinburgh	1st	888,835	0·92	Edinburgh	2nd	1,721,557	0·63
Liverpool	11th	1,016,178	1·62	Manchester	2nd	1,748,244	1·22	Manchester	3rd	2,508,588	0·94
Westminster ...	4th	2,173,298	1·74	Aberdeen	1st	138,372	1·30	Leeds	3rd	701,409	0·96
Brighton	3rd	583,701	1·81	Burnley	2nd	124,933	1·33	Preston	4th	320,500	1·00
Newcastle District	4th	431,239	1·82	Oldham	2nd	147,432	1·40	Bradford	7th	813,623	1·03
Burnley	1st	74,383	1·83	Liverpool	12th	1,185,964	1·43	Liverpool	13th	844,617	1·14
Huddersfield ...	1st	156,169	1·85	Leeds	2nd	524,629	1·46	Nelson	4th	68,768	1·21
St. James's	5th	1,569,884	1·93	Preston	3rd	271,076	1·47	Westminster ...	6th	3,503,054	1·24
Newcastle-on-Tyne	4th	401,066	2·02	Sunderland	1st	95,446	1·48	St. James's	7th	2,401,431	1·25
Bradford	5th	554,633	2·02	Bradford	6th	673,699	1·48	Blackburn	1st	157,000	1·26
Birmingham ...	3rd	396,952	2·07	Westminster... ...	5th	2,830,896	1·51	Nottingham	2nd	297,185	1·29
Hull	1st	163,857	2·09	Portsmouth	1st	406,118	1·52	Huddersfield ...	3rd	304,163	1·31
Charing Cross ...	3rd	1,115,609	2·10	Dundee...	3rd	222,934	1·58	Portsmouth	2nd	839,392	1·32
Kensington	7th	977,797	2·11	Huddersfield ...	2nd	227,753	1·59	Glasgow	5th	1,497,842	1·32
Leeds	1st	291,118	2·17	Glasgow	4th	1,090,959	1·62	Burnley	3rd	178,152	1·34
Oldham...	1st	68,998	2·18	Nottingham	1st	171,654	1·65	Whitehaven ..	3rd	173,378	1·44
Norwich	1st	186,500	2·19	St. James's	6th	1,846,064	1·67	Newcastle District	6th	541,139	1·44

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Oxford	2nd	157,257	2·20	Brighton	4th	867,494	1·70	Brighton	5th	1,388,821	1·44
Glasgow	3rd	901,287	2·24	Newcastle District	5th	471,662	1·71	Sheffield	9th	483,427	1·48
Chelsea	5th	469,416	2·31	Birmingham ...	4th	490,646	1·75	Newcastle-on-Tyne	6th	535,953	1·50
Preston	2nd	220,867	2·32	Newcastle-on-Tyne	5th	448,832	1·80	Charing Cross ...	5th	1,944,402	1·52
St. Pancras	3rd	719,484	2·46	Kensington	8th	1,228,734	1·82	Worcester	2nd	333,644	1·54
Notting Hill ...	4th	180,266	2·65	Wolverhampton ...	1st	191,701	1·83	Kelvinside	3rd	63,467	1·57
Bristol	1st	293,523	2·66	Charing Cross ...	4th	1,383,813	1·84	Kensington	9th	1,514,729	1·57
Sheffield	7th	192,220	2·76	Oxford	3rd	207,633	1·86	Norwich	3rd	436,050	1·59
Dundee	2nd	169,225	2·78	Hull	3rd	246,277	1·95	Hull	4th	340,439	1·62
City of London ..	3rd	2,602,217	2·78	Bristol	2nd	408,301	1·96	Bristol	3rd	650,758	1·63
Bath	4th	209,077	3·01	Norwich	2nd	299,650	1·98	Southport	2nd	245,515	1·65
Blackpool	1st	217,085	3·13	Chelsea	6th	577,770	2·05	Aberdeen	2nd	210,185	1·68
Hove	2nd	110,818	3·20	Hanley	1st	180,469	2·09	Birmingham ...	5th	756,428	1·68
Hastings	12th	199,284	3·21	Newport	1st	91,557	2·11	Notting Hill ...	6th	230,787	1·78
Eastbourne	12th	147,348	3·31	Worcester	1st	246,912	2·31	Dewsbury	2nd	150,878	1·83
House-to-House ...	5th	400,911	3·38	Notting Hill... ...	5th	182,327	2·42	Chelsea	7th	813,764	1·84
Southampton ...	4th	85,501	3·41	Southport	1st	89,979	2·44	House-to-House ...	7th	643,693	1·85
Metropolitan ...	6th	2,941,550	3·50	City of London ...	4th	3,845,096	2·46	Newport	2nd	195,932	1·86
Taunton	4th	100,495	3·54	Yarmouth	1st	114,645	2·47	Wolverhampton ...	2nd	224,709	1·88
Northampton ...	3rd	59,474	3·57	Hove	3rd	162,428	2·53	Oldham	3rd	227,982	1·88
Kingston	1st	128,267	4·14	Kelvinside	2nd	37,600	2·58	Hanley	2nd	247,881	1·90
Scarborough ...	1st	86,594	4·27	Sheffield	8th	288,406	2·72	Lancaster	2nd	106,125	1·92
Cambridge	2nd	112,084	4·48	St. Pancras	4th	849,987	2·72	Bolton	2nd	186,956	1·98
Bournemouth ...	5th	214,374	4·67	Blackpool	2nd	356,129	2·78	Sunderland	2nd	146,440	2·00
Kelvinside	1st	13,790	8·50	Bolton	1st	94,914	2·80	Hove	4th	195,915	2·02

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Table VIII.—Costs per Unit Sold for 1894, 1895, and 1896, arranged in Order of Merit.

Works Cost (being Totals of Figures in Tables II., III., IV., and V.).

1894.				1895.				1896.			
Works.	Year of Operations	Units Sold.	Cost.	Works.	Year of Operations	Units Sold.	Cost.	Works.	Year of Operations	Units Sold.	Cost.
			d.				d.				d.
Manchester	1st	1,168,382	1·49	Edinburgh	1st	888,835	0·92	Edinburgh	2nd	1,721,557	0·63
Liverpool	11th	1,016,178	1·62	Manchester	2nd	1,748,244	1·22	Manchester	3rd	2,508,588	0·94
Westminster ...	4th	2,173,298	1·74	Aberdeen	1st	138,372	1·30	Leeds	3rd	701,409	0·96
Brighton	3rd	583,701	1·81	Burnley	2nd	124,933	1·33	Preston	4th	320,500	1·00
Newcastle District	4th	431,239	1·82	Oldham	2nd	147,432	1·40	Bradford	7th	813,623	1·03
Burnley	1st	74,383	1·83	Liverpool	12th	1,185,964	1·43	Liverpool	13th	844,617	1·14
Huddersfield ...	1st	156,169	1·85	Leeds	2nd	524,629	1·46	Nelson	4th	68,768	1·21
St. James's	5th	1,569,884	1·93	Preston	3rd	271,076	1·47	Westminster ...	6th	3,503,054	1·24
Newcastle-on-Tyne	4th	401,066	2·02	Sunderland	1st	95,446	1·48	St. James's	7th	2,401,431	1·25
Bradford	5th	534,633	2·02	Bradford	6th	673,699	1·48	Blackburn	1st	157,000	1·26
Birmingham ...	3rd	396,952	2·07	Westminster... ...	5th	2,830,396	1·51	Nottingham	2nd	297,185	1·29
Hull	1st	163,857	2·09	Portsmouth	1st	406,118	1·52	Huddersfield ...	3rd	304,163	1·31
Charing Cross ...	3rd	1,115,609	2·10	Dundee	3rd	222,934	1·58	Portsmouth	2nd	839,392	1·32
Kensington	7th	977,797	2·11	Huddersfield ...	2nd	227,753	1·59	Glasgow	5th	1,497,842	1·32
Leeds	1st	291,113	2·17	Glasgow	4th	1,090,959	1·62	Burnley	3rd	178,152	1·34
Oldham...	1st	68,998	2·18	Nottingham	1st	171,654	1·65	Whitehaven ..	3rd	173,378	1·44
Norwich	1st	186,500	2·19	St. James's	6th	1,846,064	1·67	Newcastle District	6th	541,139	1·44

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Oxford	2nd	157,257	2·20	Brighton	4th	867,494	1·70	Brighton	5th	1,388,821	1·44
Glasgow	3rd	901,287	2·24	Newcastle District	5th	471,662	1·71	Sheffield	9th	483,427	1·48
Chelsea	5th	469,416	2·31	Birmingham ...	4th	490,646	1·75	Newcastle-on-Tyne	6th	535,958	1·50
Preston	2nd	220,867	2·32	Newcastle-on-Tyne	5th	448,832	1·80	Charing Cross ...	5th	1,944,402	1·52
St. Pancras	3rd	719,484	2·46	Kensington ...	8th	1,228,734	1·82	Worcester	2nd	333,644	1·54
Notting Hill ...	4th	180,266	2·65	Wolverhampton ...	1st	191,701	1·83	Kelvinside	3rd	63,467	1·57
Bristol	1st	293,523	2·66	Charing Cross ...	4th	1,383,813	1·84	Kensington	9th	1,514,729	1·57
Sheffield	7th	192,220	2·76	Oxford	3rd	207,633	1·86	Norwich	3rd	436,050	1·59
Dundee	2nd	169,225	2·78	Hull	3rd	246,277	1·95	Hull	4th	340,439	1·62
City of London ..	3rd	2,602,217	2·78	Bristol	2nd	408,301	1·96	Bristol	3rd	650,758	1·63
Bath	4th	209,077	3·01	Norwich	2nd	299,650	1·98	Southport	2nd	245,515	1·65
Blackpool	1st	217,085	3·13	Chelsea	6th	577,770	2·05	Aberdeen	2nd	210,185	1·68
Hove	2nd	110,818	3·20	Hanley	1st	180,469	2·09	Birmingham ...	5th	756,428	1·68
Hastings	12th	199,284	3·21	Newport	1st	91,557	2·11	Notting Hill ...	6th	230,787	1·78
Eastbourne	12th	147,348	3·31	Worcester	1st	246,912	2·31	Dewsbury	2nd	150,878	1·83
House-to-House ...	5th	400,911	3·38	Notting Hill... ...	5th	182,327	2·42	Chelsea	7th	813,764	1·84
Southampton ...	4th	85,501	3·41	Southport	1st	89,979	2·44	House-to-House ...	7th	643,693	1·85
Metropolitan ...	6th	2,941,550	3·50	City of London ...	4th	3,845,096	2·46	Newport	2nd	195,932	1·86
Taunton	4th	100,495	3·54	Yarmouth	1st	114,645	2·47	Wolverhampton ...	2nd	224,709	1·88
Northampton ...	3rd	59,474	3·57	Hove	3rd	162,428	2·53	Oldham	3rd	227,982	1·88
Kingston	1st	128,267	4·14	Kelvinside	2nd	37,600	2·58	Hanley	2nd	247,881	1·90
Scarborough ...	1st	86,594	4·27	Sheffield	8th	288,406	2·72	Lancaster	2nd	106,125	1·92
Cambridge	2nd	112,084	4·48	St. Pancras	4th	849,987	2·72	Bolton	2nd	186,956	1·98
Bournemouth ...	5th	214,374	4·67	Blackpool	2nd	356,129	2·78	Sunderland	2nd	146,440	2·00
Kelvinside	1st	13,790	8·50	Bolton	1st	94,914	2·80	Hove	4th	195,915	2·02

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Table VIII.—COSTS PER UNIT SOLD FOR 1894, 1895, AND 1896, ARRANGED IN ORDER OF MERIT—*continued.*

Works Cost (being Totals of Figures in Tables II., III., IV., and V.)—continued.

1894.				1895.				1896.			
Works.	Year of Operations.	Units Sold.	Cost.	Works.	Year of Operations.	Units Sold.	Cost.	Works.	Year of Operations.	Units Sold.	Cost.
							d.				d.
				Taunton	5th	108,286	2·86	Oxford	4th	291,640	2·02
				Lancaster	1st	78,264	2·97	City of London ...	5th	5,488,500	2·02
				Hastings	13th	265,846	3·04	Pontypool	3rd	35,011	2·09
				Cardiff	1st	175,078	3·10	Stafford	1st	43,619	2·16
				Halifax...	1st	119,028	3·18	Walsall	1st	67,170	2·22
				Dewsbury	1st	58,109	3·19	Tunbridge Wells ..	1st	174,053	2·22
				House-to-House ...	6th	476,714	3·20	Northampton ...	5th	89,445	2·27
				Southampton ...	5th	100,473	3·23	Southampton ...	6th	131,843	2·27
				Dublin	3rd	453,294	3·36	Halifax	2nd	177,531	2·42
				Pontypool	2nd	32,271	3·39	Hampstead	2nd	547,920	2·45
				Belfast	1st	82,771	3·46	Taunton	6th	126,840	2·50
				Scarborough ...	2nd	135,177	3·47	Yarmouth	2nd	157,254	2·51
				Metropolitan ..	7th	3,661,895	3·48	Bedford	2nd	158,236	2·54
				Eastbourne	13th	175,007	3·55	Scarborough ...	3rd	174,515	2·55
				Woolwich	2nd	54,885	3·60	Ayr	1st	124,924	2·64
				Exeter	6th	110,000	3·78	Cardiff	2nd	308,430	2·64
				Northampton ...	4th	67,764	3·83	Cambridge	4th	197,615	2·67

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Cambridge ...	3rd	150,510	3·91	Oswestry	1st	15,453	2·68
Bournemouth ...	6th	243,816	3·97	Richmond	3rd	97,044	2·70
Kingston	2nd	134,084	4·06	Shrewsbury	1st	23,820	2·71
Richmond	2nd	80,456	4·12	Blackpool	3rd	429,669	2·77
Bedford...	1st	45,500	4·26	St. Pancras	5th	1,201,229	2·80
Hampstead	1st	192,527	4·68	Dover	1st	154,200	2·89
Leicester	1st	77,797	5·04	Belfast	2nd	149,721	2·92
Reading	1st	42,596	5·72	Leicester	2nd	169,668	2·92
Burton	1st	40,094	6·88	Metropolitan ...	8th	4,075,000	2·99
				Coventry	1st	51,114	3·00
				Ealing	2nd	246,902	3·14
				Kingston	3rd	155,681	3·22
				Hastings	14th	294,350	3·22
				Woolwich	3rd	75,929	3·28
				Cheltenham	1st	103,715	3·33
				Islington	1st	297,834	3·36
				Eastbourne	14th	208,096	3·43
				Dublin	4th	473,547	3·49
				Reading	2nd	82,165	3·59
				Bournemouth ...	7th	281,310	3·69
				Aberystwith... ...	2nd	36,681	3·87
				Burton	2nd	58,192	4·39
				Crystal Palace ...	3rd	118,316	5·61
				Salford	1st	52,486	9·35

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Table IX.—COSTS PER UNIT SOLD FOR 1894, 1895, AND 1896, ARRANGED IN ORDER OF MERIT.

Total Costs (being Totals of Figures in Tables II., III., IV., V., VI., and VII.).

1894.				1895.				1896.			
Works.	Year of Operations.	Units Sold.	Cost.	Works.	Year of Operations.	Units Sold.	Cost.	Works.	Year of Operations.	Units Sold.	Cost.
			d.				d.				d.
Manchester	1st	1,168,382	2·17	Edinburgh	1st	888,335	1·67	Edinburgh	2nd	1,721,557	1·13
Brighton	3rd	583,701	2·30	Manchester	2nd	1,748,244	1·80	Manchester	3rd	2,508,588	1·45
Newcastle District	4th	431,239	2·45	Leeds	2nd	524,629	2·05	Nelson	4th	68,768	1·67
Liverpool	11th	1,016,178	2·46	Portsmouth	1st	406,118	2·06	Portsmouth	2nd	839,392	1·71
Norwich	1st	186,500	2·54	Charing Cross ..	4th	1,383,813	2·21	Leeds	3rd	701,409	1·73
Bradford	5th	554,638	2·54	Burnley	2nd	124,933	2·22	Whitehaven... ...	3rd	178,378	1·75
Charing Cross ..	3rd	1,115,609	2·58	Bradford	6th	673,699	2·24	Liverpool	13th	844,617	1·77
Westminster... ...	4th	2,173,298	2·69	Dundee...	3rd	222,934	2·30	Bradford	7th	813,623	1·81
Newcastle-on-Tyne	4th	401,066	2·80	Westminster ...	5th	2,830,396	2·31	Glasgow	5th	1,497,842	1·92
Hull	2nd	163,857	2·86	Aberdeen	1st	138,372	2·36	Preston...	4th	320,500	1·94
Kensington	7th	977,797	2·86	Brighton	4th	867,494	2·36	Brighton	5th	1,388,821	2·04
St. James's	5th	1,569,884	2·94	Newcastle District	5th	471,662	2·38	Burnley	3rd	178,152	2·07
Glasgow	3rd	901,287	3·02	Oldham...	2nd	147,432	2·42	Westminster ...	6th	3,503,054	2·09
St. Pancras	3rd	719,484	3·03	Norwich	2nd	299,650	2·42	Worcester	2nd	333,644	2·11
Leeds	1st	291,113	3·11	Glasgow	4th	1,090,959	2·44	Norwich	3rd	436,050	2·11
Burnley	1st	74,388	3·18	Liverpool	12th	1,185,964	2·44	Nottingham	2nd	297,185	2·17
City of London ...	3rd	2,602,217	3·27	Newcastle-on-Tyne	5th	448,832	2·45	Sheffield	9th	488,427	2·20

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Huddersfield	1st	156,169	3·28	Kensington	8th	1,228,734	2·57	Newcastle District	6th	541,139	2·29
Birmingham ...	3rd	396,952	3·54	Hull	3rd	246,277	2·70	St. James's	7th	2,401,431	2·29
Dundee...	2nd	169,225	3·56	Worcester	1st	246,912	2·74	Charing Cross ...	5th	1,944,402	2·30
Chelsea...	5th	469,416	3·69	St. James's	6th	1,846,064	2·74	Southport	2nd	245,515	2·31
Bath	4th	209,077	3·70	Preston...	3rd	271,076	2·77	Hull	4th	340,439	2·31
Oxford	2nd	157,257	3·72	Nottingham	1st	171,654	2·79	Newcastle-on-Tyne	6th	535,953	2·33
Sheffield	7th	192,220	3·80	Hanley	1st	180,469	2·80	Huddersfield ...	3rd	304,163	2·37
Blackpool	1st	217,085	3·83	Wolverhampton ...	1st	191,701	2·82	Hanley	2nd	247,881	2·42
Oldham...	1st	68,998	3·93	Huddersfield ...	2nd	227,753	2·87	Bristol	3rd	650,758	2·50
Preston...	2nd	220,867	3·95	Birmingham ...	4th	490,646	3·11	Lancaster	2nd	106,125	2·59
Hastings	12th	199,284	4·31	Yarmouth	1st	114,645	3·17	Blackburn	1st	157,000	2·60
Eastbourne	12th	147,348	4·34	Bristol	2nd	408,301	3·19	Aberdeen	2nd	210,185	2·64
House-to-House ...	5th	400,911	4·34	Newport	1st	91,557	3·21	Oldham	3rd	227,982	2·66
Southampton ...	4th	85,501	4·41	Chelsea...	6th	577,770	3·23	Kensington	9th	1,514,729	2·66
Northampton ...	3rd	59,474	4·42	Blackpool	2nd	356,129	3·31	Wolverhampton ...	2nd	224,709	2·71
Bristol	1st	293,523	4·43	St. Pancras	4th	849,987	3·32	Newport	2nd	195,932	2·76
Taunton	4th	100,495	4·47	Oxford	3rd	207,633	3·36	Birmingham ...	5th	756,428	2·78
Metropolitan ...	5th	2,941,550	4·52	City of London ...	4th	3,845,096	3·50	Bolton	2nd	186,956	2·81
Notting Hill ...	4th	130,266	4·55	Sunderland	1st	95,446	3·64	Tunbridge Wells...	1st	174,053	2·82
Kingston	1st	128,267	4·80	Sheffield	8th	288,406	3·65	Cardiff	2nd	308,430	3·06
Scarborough ...	1st	86,594	4·83	Cardiff	1st	175,078	3·69	Chelsea...	7th	813,764	3·08
Cambridge	2nd	112,084	5·27	Southport	1st	89,979	3·71	Bedford	2nd	158,236	3·13
Hove	2nd	110,818	5·42	Lancaster	1st	78,264	3·80	House-to-House ...	7th	643,693	3·13
Bournemouth ...	5th	214,374	5·91	Hastings	13th	265,846	3·88	Sunderland	2nd	146,440	3·26
Kelvinside	1st	13,790	18·42	Taunton ...	5th	108,286	3·91	Yarmouth	2nd	157,254	3·27

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Table IX.—COSTS PER UNIT SOLD FOR 1894, 1895, AND 1896, ARRANGED IN ORDER OF MERIT—*continued.*

Total Costs (being Totals of Figures in Tables II., III., IV., VI., V., and VII.)—continued.

1894.				1895.				1896.			
Works.	Year of Operations.	Units Sold.	Cost.	Works.	Year of Operations.	Units Sold.	Cost.	Works.	Year of Operations.	Units Sold.	Cost.
							d.				d.
				Dublin	3rd	453,294	3·93	Stafford	1st	43,619	3·28
				Hove	3rd	162,428	4·16	Dewsbury	2nd	150,878	3·30
				Bolton	1st	94,914	4·18	Hampstead	2nd	547,920	3·30
				Notting Hill ...	5th	182,327	4·23	Blackpool	3rd	429,669	3·41
				House-to-House ...	6th	476,714	4·24	Oxford	4th	291,640	3·44
				Scarborough ...	2nd	135,177	4·36	Taunton	6th	126,840	3·49
				Metropolitan ...	7th	3,661,895	4·41	City of London ...	5th	5,488,500	3·52
				Pontypool	2nd	32,271	4·48	Halifax...	2nd	177,531	3·57
				Eastbourne	13th	175,007	4·51	St. Pancras	5th	1,201,229	3·60
				Southampton ...	5th	100,473	4·52	Ayr	1st	124,924	3·61
				Cambridge	3rd	150,510	4·53	Scarborough ..	3rd	174,515	3·63
				Exeter	6th	110,000	4·54	Southampton ..	6th	131,843	3·67
				Northampton ...	4th	67,764	4·56	Northampton ...	5th	89,445	3·71
				Halifax	1st	119,028	4·71	Notting Hill... ..	6th	230,787	3·74
				Kingston	2nd	134,084	4·77	Leicester	2nd	169,668	3·78
				Dewsbury	1st	58,109	4·98	Cambridge ...	4th	197,615	3·79
				Bournemouth ...	6th	243,816	5·07	Kingston	3rd	155,681	3·80

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Woolwich	2nd	54,885	5·17	Kelvinside	3rd	68,467	3·81
Belfast	1st	82,771	5·42	Hove	4th	195,915	3·85
Bedford	1st	45,500	5·55	Walsall	1st	67,170	3·88
Leicester	1st	77,797	5·72	Ealing	2nd	246,902	3·91
Kelvinside	2nd	37,600	6·44	Oswestry	1st	15,453	3·96
Richmond	2nd	80,456	6·54	Hastings	14th	294,350	3·97
Hampstead	1st	192,527	6·66	Belfast	2nd	149,721	4·01
Burton	1st	40,094	7·72	Metropolitan ...	8th	4,075,000	4·13
Reading	1st	42,596	7·72	Cheltenham	1st	103,715	4·17
				Dublin	4th	473,547	4·20
				Pontypool	3rd	35,011	4·28
				Islington	1st	297,834	4·58
				Woolwich	3rd	75,929	4·65
				Eastbourne	14th	208,096	4·69
				Richmond	3rd	97,044	4·85
				Dover	1st	154,200	4·93
				Bournemouth ...	7th	281,310	5·02
				Burton	2nd	58,192	5·06
				Shrewsbury	1st	23,820	5·07
				Reading	2nd	82,165	5·21
				Aberystwith... ...	2nd	36,681	5·43
				Coventry	1st	51,114	6·04
				Crystal Palace ...	3rd	118,316	8·73
				Salford	1st	52,486	12·57

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15. Before more closely examining this deduction, it may be useful to analyse the costs into various headings. In the foregoing tables I set out the costs for 1894, 1895, and 1896 in order, from the lowest to the highest :—

Costs per Unit Sold.

Table II.—Coals or other fuel.
,, III.—Oil, waste, water, and stores.
,, IV.—Wages (on generation and distribution).
,, V.—Repairs and maintenance.
,, VI.—Rents, rates, and taxes.
,, VII.—Management—*i.e.*, salaries, stationery and printing, general establishment charges, law expenses, insurance, &c.
,, VIII.—Works costs, being the sum in the case of each works of II., III., IV., and V.
,, IX.—Total (works and management) costs, being the sum in the case of each works of II., III., IV., V., VI., and VII.

16. In drawing deductions from these tables it is, I think, wise to exclude the results shown by works in their first year of operation.

As the prevailing custom is to place upon the contractors the responsibility of maintaining the plant for nine months to a year, it frequently happens that a portion of the first year's costs for repairs and maintenance, and even for wages, is borne by them.

This rarely obtains in the second year, and therefore, if the first year be disregarded, a fair approximation may be made of *ideal costs* by extracting from the above tables the lowest costs under each heading; thus :—

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AGGREGATION OF THE LOWEST COSTS IN THE UNITED KINGDOM.

Second Year of Operation and onwards.

Name of Place.	Year of Operation.	Units Sold.	Cost per Unit Sold.	d.
Aberdeen ...	2nd	210,185	Fuel ...	0·25
Leeds	4th	833,280		
Oldham	3rd	227,982	Oil, waste, water, &c.	0·05
Leeds	4th	833,280		
Edinburgh ...	2nd	1,721,557	Wages ...	0·20
Newport, Mon. ...	2nd	195,932	Repairs ...	0·06
			Works Costs	0·56
Cardiff	2nd	308,430	Rent, rates, and taxes	0·06
Worcester ...	2nd	333,644		
Whitehaven ...	3rd	178,378	Management	0·22
			Total Costs	0·84

Nelson's 0·15d. for fuel is excluded, as the bulk of the steam at those works was raised from the heat generated in the gas retorts.

17. Apparently, then, if the most favourable features of each works were reproduced in one single works, electrical energy for lighting would be produced and distributed for 0·84d. per unit; but this happy combination necessitates the command of fuel as cheap, and I suppose I ought to add as efficiently used, as at Aberdeen and Leeds; it necessitates the careful control and use of oil which characterises Oldham and Leeds; it requires the minimising of the wages list to the standard of Edinburgh, and the absence from breakdowns to machinery which signalises the working at Newport; it demands the happy position of freedom of rent and the friendly consideration of the rating authority, which obtains at Cardiff and Worcester, combined with the economical apportionment of management charges that prevails at Whitehaven.

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18. The assemblage of all the virtues in one works may come in time, but meanwhile we have to be content with an approximation thereto, and it is satisfactory to find that two of the above-named works—Edinburgh and Leeds—show satisfactory signs of nearing the goal with increased outputs, both having respectively succeeded in lowering the record of *costs per output*

19. In view, therefore, of the exceptional conditions under which the above "ideal" costs are obtained, I do not think that we should be right in taking 0·84d. as the goal to which the bulk of the works in this country can expect to arrive, and I prefer to direct your attention to the carefully reasoned out "ideal "standard costs" which Mr. R. E. Crompton laid before us in his 1894 paper.

20. It will be remembered by those who had the pleasure of hearing Mr. Crompton that he prophesied, to a somewhat incredulous audience, that the day would come when, with an output of 5,000,000 units, costs of production and distribution, in places where best coal cost 20s. per ton, would work out as follows:—

	Per Unit Sold.	
	d.	d.
Works Costs:		
Fuel—2·5 lbs. Welsh coal, at 20s. ...	0·27	
Water	0·01	
Petty stores	0·02	
Wages	0·10	
Superintendence	0·10	
		0·50
Maintenance:		
Material on repairs	0·20	
Wages on repairs...	0·15	
Superintendence	0·05	
		0·40
		0·90
Management		0·42
		1·32

or, to arrange the figures in the abbreviated Board of Trade form now become common :— Mr. Hammond

		d.
Fuel	...	0·27
Oil, waste, water, and stores	...	0·03
Wages	...	0·10
Repairs and maintenance	...	0·35
Works Costs	...	0·75
Rent, rates, and taxes ...		0·57
Management (including engineers' superintendence) ...		
		1·32

21. After the lapse of four years we are able to chronicle the fact that these "ideal standard costs" have in one case been improved upon, and in two others closely approached, with much lower outputs than the 5,000,000 units fixed by Mr. Crompton :—

Works Costs:

Place.	Year.	Output.	Works Costs per Unit Sold.
Edinburgh ...	1896–7	1,721,557	0·63d.
Leeds ...	1897	833,280	0·78d.
Manchester ...	1896–7	2,508,588	0·94d.

Total Costs:

Place.	Year.	Output.	Total Costs per Unit Sold.
Edinburgh ...	1896–7	1,721,557	1·13d.
Manchester ...	1896–7	2,508,588	1·45d.
Leeds ..	1897	833,280	1·50d.

In all these cases, however, the coal was obtainable much below the 20s. per ton which Mr. Crompton took as a basis.

22. It may be objected that Mr. Crompton had specially in view London works, and it is interesting to compare, with his "ideal" figures, the lowest results yet attained in the metropolis :—

Mr. Hammond.

Metropolitan Works.

	Costs per Unit Sold.		
	Mr. Crompton, 1894.	1897 Costs.	
	"Ideal Costs." Units. 5,000,000	St. James's. Units. 3,028,242	Westminster. Units. 4,355,781
Fuel	0·27d.	0·51d.	0·63d.
Oil, waste, water, and stores...	0·03d.	0·07d.	0·09d.
Wages	0·10d.	0·29d.	0·34d.
Repairs and maintenance ...	0·35d.	0·40d.	0·23d.
Total Works Costs	0·75d.	1·27d.	1·29d.
Rent, rates, and taxes ...	0·57d.	0·21d.	0·22d.
Management expenses (including engineers' superintendence)		0·71d.	0·68d.
Total Costs	1·32d.	2·19d.	2·19d.

23. It is a singular coincidence that both these works, though with widely different outputs, should achieve exactly the same total costs per unit, *i.e.*, 2·19d.

Bradford, Brighton, Edinburgh, Glasgow, Leeds, Liverpool, Manchester, Portsmouth, and Whitehaven have improved upon these results, even after allowance be made for their cheaper coal (see Table X.).

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Table X.—PROVINCIAL WORKS.—SOME COMPARATIVE LOW COSTS PER UNIT SOLD.

—	Bradford, 1896. Units Sold, 818,623.	Brighton, 1896. Units Sold, 1,888,821.	Edinburgh, 1896–7. Units Sold, 1,721,557.	Glasgow, 1896–7. Units Sold, 1,497,942.	Leeds, 1897. Units Sold, 883,280.	Liverpool, 1896. Units Sold, 844,617.†	Manchester, 1896–7. Units Sold, 2,508,588.	Portsmouth, 1896–7. Units Sold, 839,302.	Whitehaven, 1896. Units Sold, 178,378.
	d.	d.	d.	d	d.	d.	d.	d.	d.
Fuel	0·40	0·69	0·31	0·45	0·25	0·50	0·40	0·56	0·49
Oil, waste, water, and stores ...	0·11	0·10	0·06	0·07	0·05	0·07	0·11	0·15	0·12
Wages	0·42	0·35	0·20	0·32	0·35	0·32	0·26	0·22	0·54
Repairs and maintenance ...	0·10	0·30	0·06	0·48	0·13	0·25	0·17	0·39	0·29
Works Costs ...	1·03	1·44	0·63	1·32	0·78	1·14	0·94	1·32	1·44
Rent, rates, and taxes	0·31	0·21	0·17	0·25	0·08	0·07	0·20	0·09	0·09
Management	0·47	0·39	0·33	0·35	0·64	0·56	0·31	0·30	0·22
TOTAL COSTS ...	1·81	2·04	1·13	1·92	1·50	1·77	1·45	1·71	1·75

† *Half-year only.*

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24. The question arises, What are the conditions tending towards such low costs as those recorded above?

Manufacturers of electrical apparatus, of engines, and of boilers, or advocates of certain systems, are only human, and we may pardon them the weakness of claiming the low costs as due entirely to the use of their plant, or the adoption of their pet system; but an examination of the records achieved dispels the illusion that lowest costs can only be obtained by the use of particular engines, boilers, dynamos, &c., or special systems of distribution.

A closer examination of the data reveals the fact that low costs are the result of the combination of favourable conditions.

A.—*Output.*

25. In the first place, the data under review show conclusively, what perhaps would be otherwise sufficiently obvious—that *output* is the greatest factor in the reduction of costs per unit.

For the purpose of illustrating this graphically, I set out curves showing the relation of works costs per unit to units sold, in the following places:—

Diagram I. (a).—Works Costs.

Edinburgh.	Leeds.	St. James'.
Kensington.	Manchester.	Westminster.

Diagram I. (b).—Works Costs.

Bradford.	Bristol.	Portsmouth.
Brighton.	Glasgow.	

In these diagrams the abscissæ represent the units sold, and the ordinates the cost per unit.

Diagram I. (*b*) is, as far as the abscissæ are concerned, of double the scale of No. I. (*a*). When originally drawn to a like scale, it was found difficult to show the lines clearly. I have, however, adhered to the same scale for the ordinates, so that with the two diagrams side by side the *costs* per unit are directly comparable.

26. In each place the works costs per unit sold, fall rapidly as the output increases from year to year.

Comparing the curves of the three London companies, Kensington, St James', and Westminster, it will be seen that the Mr. Hammond.

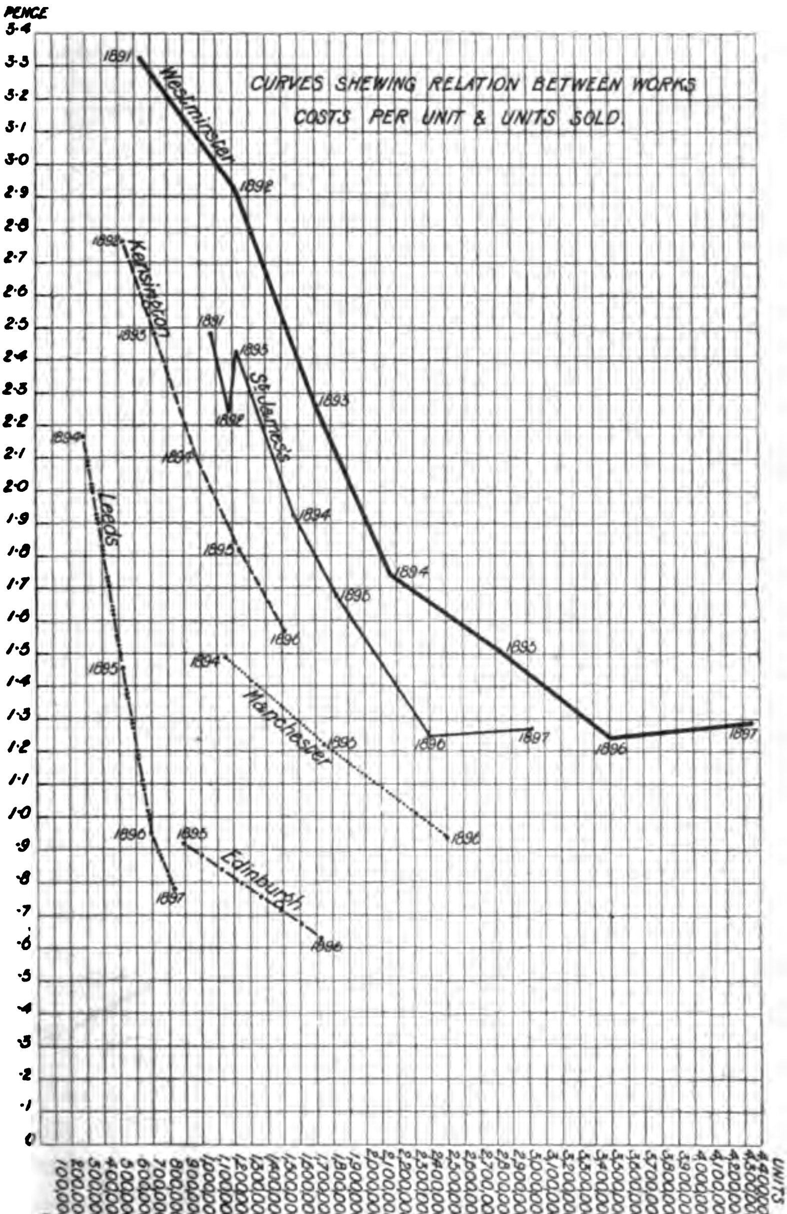

DIAGRAM I. (a).—WORKS COSTS.

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works costs of Kensington and Westminster fell in parallel lines from 2·77d. per unit to 1·74d. per unit, with an increased

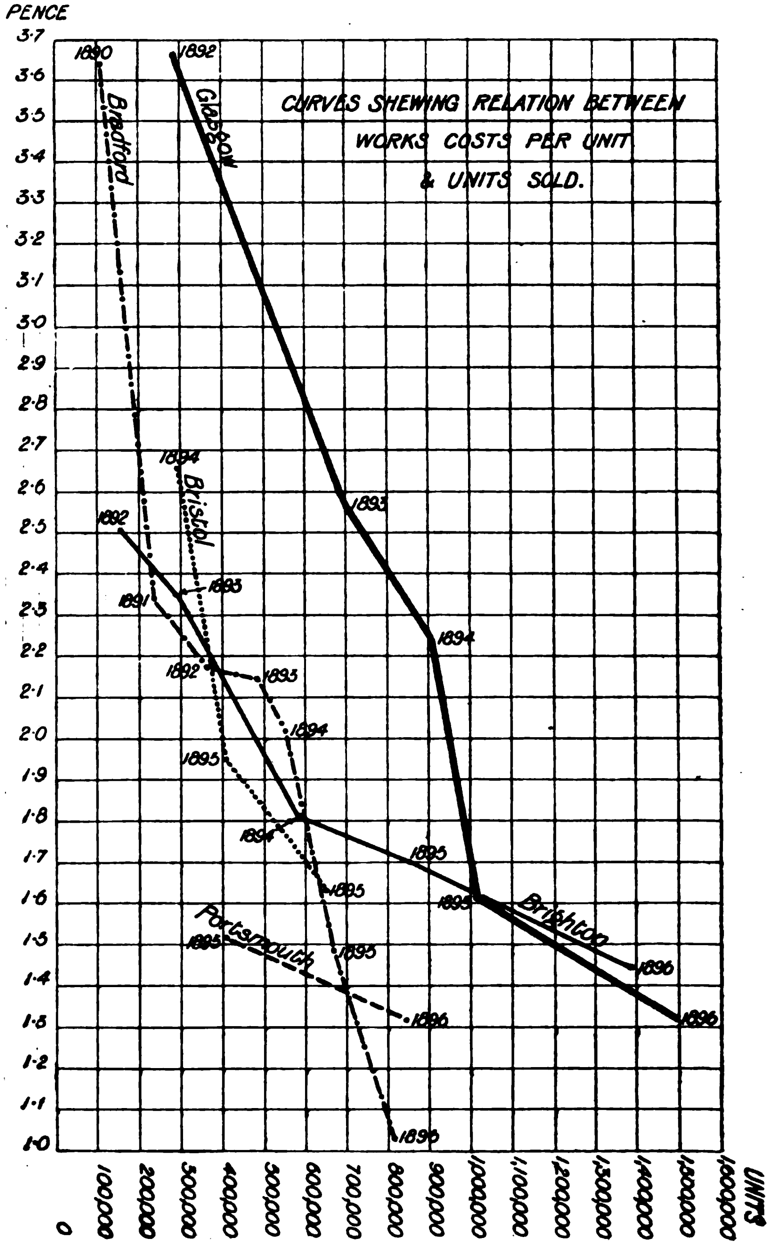

Diagram I. (b).—Works Costs.

output in each case of 800,000 units, while St. James' exhibits a parallel fall from 2·42d. per unit. Mr. Hammond.

Edinburgh, Manchester, and Westminster also run on parallel lines of falling prices, but with widely different outputs.

Leeds exhibits the steepest line, and secures the record for lowest price on output.

In Diagram I. (*b*) the best curve is that of Bradford, which achieved works costs of 1·03d. on an output of 813,623 units.

In spite of the higher price of coal at Brighton, it kept well ahead of Glasgow till the output reached a million units, when Glasgow scored a slight advantage.

Portsmouth ran almost exactly parallel with Brighton, but achieved the same works costs on an output lower by 800,000 units, and finally recorded 1·32d. per unit on an output of 839,392 units, against Brighton's 1·44d. on 1,388,821 units.

27. Turning to the "*Total Costs*" diagrams, it will be noted that, on the whole, the curves follow similar lines.

The curious rise in the St. James' 1893 costs is explained by the exceptionally heavy outlay in repairs and maintenance, viz., £3,228, being £1,113 beyond the amount spent in 1892, and including £1,290 for renewals. Then the general charges were burdened with the heavy law charges—£1,528. Altogether, this rise may be attributed to abnormal circumstances.

It will be seen that Portsmouth displaces Bradford from its position of eminence, having scored total costs of 1·71d., against Bradford's 1·81d. on an almost identical output.

The downward tendency of works costs of both St. James' and Westminster is checked at 1·25d. and 1·24d. respectively, leading to the supposition that the minimum of works costs for a lighting load in the metropolis should be placed at this figure. On total costs, however, St. James' still shows a falling curve.

28. Later in my paper I will discuss the question as to the probable limit of fall in costs, but, after drawing attention to the main features of the curves, I will pass to my next heading, limiting myself for the moment to the very evident deduction that increasing output has a paramount influence on costs per unit.

Obviously, however, *output* is not the only influence on costs,

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for if it were we should find that throughout the kingdom costs were in inverse ratio to output; whereas, if the record costs are arranged under outputs, as in Table XI., it will be seen that the highest costs are by no means characteristic of the lowest outputs, though the converse does apply—*i.e.*, that the lowest costs accompany the higher outputs.

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Table XI.—Record Costs.

(1.) *Coal and other Fuel.*

Units Sold (Kilowatt-Hours).	1894.			1895.			1896.		
Between	Place.	Year of Operation.	Cost per Unit Sold.	Place.	Year of Operation.	Cost per Unit Sold.	Place.	Year of Operation.	Cost per Unit Sold.
			d.			d.			d.
100,000 and 200,000	Huddersfield	1st	0·64	Aberdeen	1st	0·23	Blackburn	1st	0·39
200,000 and 350,000	Leeds...	1st	0·58	Preston	3rd	0·44	Aberdeen	2nd	0·25
350,000 and 600,000	Newcastle-on-Tyne ..	4th	0·57	Leeds	2nd	0·30	Newcastle-on-Tyne ..	6th	0·47
600,000 and 1,000,000 ...	Glasgow	3rd	0·59	Edinburgh	1st	0·38	Leeds	3rd	0·29
1,000,000 and 1,500,000 ...	Manchester	1st	0·50	Glasgow	4th	0·51	Glasgow	5th	0·45
1,500,000 and 2,000,000 ...	St. James's	5th	0·75	Manchester	2nd	0·42	Edinburgh	2nd	0·31
2,000,000 and 2,500,000 ...	Westminster	4th	0·65	......	...	...	St. James's	7th	0·50
2,500,000 and 3,000,000 ...	Metropolitan	6th	1·90	Westminster	5th	0·58	Manchester	3rd	0·40
3,000,000 and upwards ...	......	...	...	City of London ...	4th	0·98	Westminster	6th	0·53

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Table XI.—Record Costs—*continued.*

(2.) *Oil, Waste, Water, and Stores.*

Units Sold (Kilowatt-Hours).	1894.			1895.			1896.		
Between	Place.	Year of Operation.	Cost per Unit Sold.	Place.	Year of Operation.	Cost per Unit Sold.	Place.	Year of Operation.	Cost per Unit Sold.
			d.			d.			d.
100,000 and 200,000	Huddersfield	1st	0·02	Oldham	2nd	0·05	Tunbridge Wells ...	1st	0·08
200,000 and 350,000	Bristol	1st	0·15	Oxford	3rd	0·05	Oldham	3rd	0·05
350,000 and 600,000	Newcastle-on-Tyne .	4th	0·16	Bristol	2nd	0·09	Sheffield	9th	0·09
600,000 and 1,000,000 ...	Glasgow	3rd	0·10	Edinburgh	1st	0·09	Leeds	3rd	0·07
1,000,000 and 1,500,000 ...	Liverpool	11th	0·09	Glasgow	4th	0·07	Glasgow	5th	0·07
1,500,000 and 2,000,000 ...	St. James's	5th	0·18	St. James's	6th	0·09	Edinburgh	2nd	0·06
2,000,000 and 2,500,000 ...	Westminster	4th	0·13	......	...	...	St. James's	7th	0·08
2,500,000 and 3,000,000 ...	Metropolitan	6th	0·31	Westminster	5th	0·11	Manchester	3rd	0·11
3,000,000 and upwards ...	......	...	...	City of London ...	4th	0·24	Westminster	6th	0·09

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Table XI.—Record Costs—*continued.*

(3.) *Wages.*

Units Sold (Kilowatt-Hours).	1894.			1895.			1896.		
Between	Place.	Year of Operation.	Cost per Unit Sold.	Place.	Year of Operation.	Cost per Unit Sold.	Place.	Year of Operation.	Cost per Unit Sold.
			d.			d.			d.
100,000 and 200,000	Norwich	1st	0·74	Aberdeen	1st	0·46	Burnley	3rd	0·52
200,000 and 350,000	Preston	3rd	0·90	Dundee	3rd	0·38	Preston	4th	0·33
350,000 and 600,000	Brighton	3rd	0·39	Portsmouth	1st	0·40	Norwich	3rd	0·34
600,000 and 1,000,000 ...	Glasgow	3rd	0·60	Edinburgh	1st	0·30	Portsmouth	2nd	0·22
1,000,000 and 1,500,000 ...	Liverpool...	11th	0·26	Liverpool...	12th	0·22	Glasgow	5th	0·32
1,500,000 and 2,000,000 ...	St. James's	5th	0·70	Manchester	2nd	0·38	Edinburgh	2nd	0·20
2,000,000 and 2,500,000 ...	Westminster	4th	0·70	......	...	...	St. James's	7th	0·37
2,500,000 and 3,000,000 ...	Metropolitan	6th	0·70	Westminster	5th	0·57	Manchester	3rd	0·26
3,000,000 and upwards ...	.. ..	...	...	Metropolitan	7th	0·64	Metropolitan	8th	0·38

Mr. Hammond

Table XI.—Record Costs—*continued.*

(4.) *Repairs and Maintenance.*

Units Sold (Kilowatt-Hours).	1894.			1895.			1896.		
Between	Place.	Year of Operation.	Cost per Unit Sold.	Place.	Year of Operation.	Cost per Unit Sold.	Place.	Year of Operation.	Cost per Unit Sold.
			d.			d.			d.
100,000 and 200,000	Huddersfield	1st	0·11	Halifax	1st	0·11	Newport	2nd	0·06
200,000 and 350,000	Bristol	1st	0·21	Worcester...	1st	0·10	Worcester ...	2nd	0·09
350,000 and 600,000	Newcastle District ...	4th	0·16	Newcastle District ...	5th	0·12	Newcastle District ..	6th	0·18
600,000 and 1,000,000 ..	St. Pancras	3rd	0·63	Edinburgh	1st	0·15	Leeds	3rd	0·22
1,000,000 and 1,500,000 ...	Manchester	1st	0·21	Charing Cross	4th	0·35	Brighton	5th	0·30
1,500,000 and 2,000,000 ..	City of London ..	3rd	0·20	Manchester	2nd	0·27	Edinburgh	2nd	0·06
2,000,000 and 2,500,000 ...	Westminster	4th	0·26	......	...	...	St. James's	7th	0·30
2,500,000 and 3,000,000 ...	Metropolitan	6th	0·59	Westminster	5th	0·25	Manchester	3rd	0·17
3,000,000 and upwards ...	......	...	...	City of London ...	4th	0·44	Westminster	6th	0·28

Mr. Hammond.

Table XI.—Record Costs—*continued.*

(5.) *Works Costs.*

Units Sold (Kilowatt-Hours).	1894.			1895.			1896.		
Between	Place.	Year of Operation.	Cost per Unit Sold.	Place.	Year of Operation.	Cost per Unit Sold.	Place.	Year of Operation.	Cost per Unit Sold.
			d.			d.			d.
100,000 and 200,000	Huddersfield	1st	1·85	Aberdeen	1st	1·30	Blackburn	1st	1·26
200,000 and 350,000	Leeds	1st	2·17	Preston	3rd	1·47	Preston	4th	1·00
350,000 and 600,000	Brighton	3rd	1·81	Leeds	2nd	1·46	Newcastle District ...	6th	1·44
600,000 and 1,000,000 ...	Kensington	4th	2·11	Edinburgh	1st	0·92	Leeds	3rd	0·96
1,000,000 and 1,500,000 ...	Manchester	1st	1·49	Liverpool	12th	1·43	Glasgow	5th	1·32
1,500,000 and 2,000,000 ...	St. James's	5th	1·93	Manchester	2nd	1·22	Edinburgh	2nd	0·68
2,000,000 and 2,500,000 ...	Westminster	4th	1·74	.....	...	...	St. James's	7th	1·25
2,500,000 and 3,000,000 ...	Metropolitan	6th	3·50	Westminster	5th	1·51	Manchester	3rd	0·94
3,000,000 and upwards ...	......	...	...	City of London ...	4th	2·46	Westminster	6th	1·24

Mr. Hammond.

Table XI.—Record Costs—*continued.*

(6.) *Rent, Rates, and Taxes.*

Units Sold (Kilowatt-Hours).	1894.			1895.			1896.		
Between	Place.	Year of Operation.	Cost per Unit Sold.	Place.	Year of Operation.	Cost per Unit Sold.	Place.	Year of Operation.	Cost per Unit Sold.
			d.			d.			d.
100,000 and 200,000 … …	Norwich … … …	1st	Nil	Notting Hill … …	5th	0·05	Kingston … … …	3rd	0·08
200,000 and 350,000 … …	Blackpool… … …	1st	0·16	Norwich … … …	2nd	Nil	Cardiff … … …	2nd	0·06
350,000 and 600,000 … …	Brighton … … …	3rd	0·01	Leeds … … …	2nd	0·03	Norwich … … …	3rd	0·07
600,000 and 1,000,000 …	St. Pancras … …	3rd	0·10	Brighton … … …	4th	0·09	Leeds … … …	3rd	0·08
1,000,000 and 1,500,000 …	Charing Cross… …	3rd	0·09	Charing Cross… …	4th	0·05	St. Pancras … …	5th	0·10
1,500,000 and 2,000,000 …	St. James's … …	5th	0·25	Manchester … …	2nd	0·22	Edinburgh … …	2nd	0·17
2,000,000 and 2,500,000 …	Westminster … …	4th	0·18	……	…	…	St. James's … …	7th	0·27
2,500,000 and 3,000,000 …	Metropolitan … …	6th	0·30	Westminster … …	5th	0·18	Manchester … …	3rd	0·20
3,000,000 and upwards …	……	…	…	Metropolitan … …	7th	0·21	Westminster … …	6th	0·16

Mr. Hammond.

Table XI.—RECORD COSTS—*continued.*

(7.) *Management Expenses.*

UNITS SOLD (Kilowatt-Hours).	1894.			1895.			1896.		
Between	Place.	Year of Operation.	Cost per Unit Sold.	Place.	Year of Operation.	Cost per Unit Sold.	Place.	Year of Operation.	Cost per Unit Sold.
			d.			d.			d.
100,000 and 200,000 … …	Norwich … … …	1st	0·35	Cambridge … …	3rd	0·28	Whitehaven … …	3rd	0·22
200,000 and 350,000 … …	Bath … … … …	4th	0·49	Worcester … …	1st	0·39	Cardiff … … …	2nd	0·36
350,000 and 600,000 … …	Bradford … … …	5th	0·32	Newcastle District ..	5th	0·39	Norwich … … …	3rd	0·45
600,000 and 1,000,000 …	Glasgow … … …	3rd	0·43	Bradford … … …	5th	0·49	Portsmouth … …	2nd	0·30
1,000,000 and 1,500,000 …	Charing Cross .. …	3rd	0·39	Charing Cross … …	4th	0·32	Glasgow … … …	5th	0·35
1,500,000 and 2,000,000 …	St. James's … …	5th	0·76	Manchester … …	2nd	0·36	Edinburgh … …	2nd	0·33
2,000,000 and 2,500,000 …	Westminster … …	4th	0·77	……	…	…	St. James's … …	7th	0·77
2,500,000 and 3,000,000 …	Metropolitan … …	6th	0·72	Westminster … …	5th	0·67	Manchester … …	3rd	0·31
3,000,000 and upwards …	……	…	…	City of London …	4th	0·54	Westminster … …	6th	0·69

Mr Hammond.

Table XI.—Record Costs—*continued.*

(8.) *Total Costs.*

Units Sold (Kilowatt-Hours). Between	1894. Place.	Year of Operation.	Cost per Unit Sold.	1895. Place.	Year of Operation.	Cost per Unit Sold.	1896. Place.	Year of Operation.	Cost per Unit Sold.
			d.			d.			d.
100,000 and 200,000	Norwich	1st	2·54	Burnley	2nd	2·22	Whitehaven	3rd	1·75
200,000 and 350,000	Leeds	1st	3·11	Dundee	3rd	2·30	Preston	4th	1·94
350,000 and 600,000	Brighton	3rd	2·30	Leeds	2nd	2·05	Norwich	3rd	2·11
600,000 and 1,000,000 ...	Kensington	4th	2·86	Edinburgh	1st	1·67	Portsmouth	2nd	1·71
1,000,000 and 1,500,000 ..	Manchester	1st	2·17	Charing Cross	4th	2·21	Glasgow	5th	1·92
1,500,000 and 2,000,000 ...	St. James's	5th	2·94	Manchester	2nd	1·80	Edinburgh	2nd	1·18
2,000,000 and 2,500,000 ...	Westminster	4th	2·69	......	...	...	St. James's	7th	2·29
2,500,000 and 3,000,000 ...	Metropolitan	6th	4·52	Westminster	5th	2·81	Manchester	3rd	1·45
3,000,000 and upwards ...	......	...	..	City of London ...	4th	3·50	Westminster	6th	2·09

B.—*Load-Factor.*

Mr. Hammond.

29. Former writers have not failed to dwell upon the marked effect which the *load-factor* has upon the costs of production, and in my estimation of the influences which bear upon that problem I place load-factor directly after *output*. Without the latter the most favourable load-factor is useless, but, combined with a healthy output, a favourable load-factor greatly reduces costs.

So convinced is Mr. Arthur Wright of the importance of load-factor that he offers to supply all his customers at 1½d. per unit for any consumption beyond an average of one hour per day of their maximum demand throughout the year.

The advantages arising from a high load-factor are, however, universally acknowledged, and I, therefore, pass on to lay before you the data in connection with it, that I have been able to collect.

These data are not demanded by the Board of Trade, and I am much indebted to those works engineers who have responded to my request for information on this head.

30. I use "load-factor" in the sense of the ratio of the actual units generated to the product of the maximum of the year and the total hours of the year; the "load-factor," therefore, of 1896 being obtained by multiplying the maximum load of 1896 by 8,784 (*i.e.*, 366 days × 24 hours), and of 1897 by 8,760.

Though it appears to be the custom at every works to record the maximum load, the "Units generated" are in the majority not arrived at by meter, but by calculation, and in some by a process which one of my correspondents describes as "cooking."

Under these circumstances I have hesitated about dealing with the data; but finally, being convinced of the absolute accuracy of a portion, and of the approximate accuracy of the bulk, I have decided to set it out (Table XII.), leaving the members of the Institution to convince themselves in particular cases as to whether the "Units generated" have been arrived at in an accurate manner.

I also plot a curve (Diagram II.) showing the relation between load-factor and works costs per unit sold.

Mr. Hammond.

Table XII.—Showing (*a*) Load-Factor, and (*b*) Units used in Distribution.

Provincial Undertakings.

Works.	Year.	Units Sold to Consumers, including Public Lighting.	Units used on Works, &c.	Units used in Distribution and unaccounted for, &c.	Total Units Generated.	Maximum Load of Year—K.W.	Load-Factor = Per cent. of Units Generated to Output of Maximum Load in use constantly throughout the Year.	Ratio of Units used in Distribution and unaccounted for, to Units Generated. Per cent.
Aberdeen	1896	210,185	3,869	22,595	236,649	343	7·85	9·46
,, ...	1897	287,072	6,524	23,956	317,552	369	9·82	7·54
Ayr	1896	124,924	9,200	57,762	191,886	129	16·98	30·10
Bedford	1896	158,238	11,040	38,828	208,106	200	11·84	18·65
,, ...	1897	255,990	20,400	65,964	342,354	270	14·47	19·26
Blackpool	1896	403,667	12,524	179,849	596,340	590	11·50	31·66
,, ...	1897	631,942	15,076	84,221	731,239	800	10·43	11·51
Bournemouth	1896	281,810	10,269	103,145	394,724	401	11·20	26·13
,, ...	1897	334,406	11,689	167,153	513,248	486	12·65	32·56
Bradford	1896	813,623	14,085	46,817	874,525	1,040	9·59	5·35
,, ...	1897	993,588	21,658	89,250	1,104,496	1,128	11·22	8·08
Brighton	1896	1,388,821	32,891	231,864	1,653,576	1,295	14·53	14·02
,, ...	1897	1,992,527	111,693	335,951	2,440,171	1,763	15·80	13·76
Bristol	1896	650,758	18,766	191,641	861,156	766	12·99	22·25
Bury	1897	64,162	3,804	3,824	71,790	91	9·00	5·32

Mr. Hammond.

Cheltenham	1896	105,500	16,100	57,400	179,000	157	12·98	32·07
Coventry	1896	51,114	8,000	20,000	79,114	100	9·00	25·20
,, ...	1897	79,588	9,878	39,384	128,845	126	12·82	30·56
Dover	1896	154,200	5,578	47,249	207,027	154	15·34	22·82
,, ...	1897	234,074	9,037	74,079	317,190			
Dublin	1896	473,547	15,000	229,527	718,074	511	15·99	32·10
,, ...	1897	518,812	14,600	227,155	760,067	541	16·04	29·86
Dundee	1896	254,278	26,271	38,200	319,149	346	10·50	11·97
Ealing...	1896	246,902	11,338	121,617	379,857	350	12·38	32·16
Eastbourne	1896	208,094	9,555	141,639	359,288	221	18·56	39·42
,, ...	1897	240,806	10,876	150,386	402,068	257	17·09	37·14
Edinburgh	1896	1,721,573	61,653	134,278	1,917,504	1,621	13·48	7·00
Halifax	1896	177,500	16,240	66,000	259,740	242	12·21	25·41
,, ...	1897	240,000	25,000	68,000	333,000	296	12·12	20·43
Hanley	1896	247,881	4,443	68,581	320,905	325	11·24	21·37
,, ...	1897	351,762	5,032	43,796	400,590	376	12·16	10·93
Hastings	1896	294,000	5,060	101,300	400,360	266	17·13	25·30
,, ...	1897	338,705	5,000	122,657	466,362	310	17·17	26·32
Hove	1896	195,915	14,304	29,560	239,779	253	10·78	12·32
,, ...	1897	268,243	10,501	32,385	311,129	310	11·45	10·40
Kingston-on-Thames	1896	155,681	6,000	17,000	178,681	152	13·46	9·51
,, ...	1897	194,268	7,000	20,000	221,268	203	12·46	9·01

Mr. Hammond.

Table XII.—~~Showing (a) Load-Factor, and~~ (b) Units used in Distribution—*continued.*

Provincial Undertakings—continued.

Works	Year.	Units Sold to Consumers, including Public Lighting.	Units used on Works, &c.	Units used in Distribution and unaccounted for, &c.	Total Units Generated.	Maximum Load of Year—K.W.	Load-Factor = Per cent. of Units Generated to Output of Maximum Load in use constantly throughout the Year.	Ratio of Units used in Distribution and unaccounted for, to Units Generated. Per cent.
Lancaster	1896	98,977	3,385	8,570	110,932	119	10·61	7·72
"	1897	134,717	3,856	8,558	146,631	190	8·80	5·83
Leeds	1897	833,280	29,597	327,123	1,190,000*	1,010	13·44	27·48
Manchester	1896	2,508,588	266,519	253,250	3,028,357	2,738	12·62	8·36
Newcastle District	1896	503,894	37,245	359,058	900,197	559	18·33	39·88
" "	1897	600,971	41,998	398,760	1,041,729	464	26·96	38·27
Newcastle-on-Tyne	1896	535,953	28,644	299,416	864,013	580	16·95	34·65
"	1897	660,906	18,356	375,398	1,054,660	700	17·15	35·59
Newport	1896	195,932	14,277	82,930	293,089	192	17·45	28·29
Nottingham	1896	171,654	2,855	9,185	183,694	255	8·20	5·00
"	1897	297,185	3,621	15,832	316,638	400	9·01	5·00
Oxford	1896	260,595	21,902	143,569	426,066	321	15·14	33·69
"	1897	313,037	26,376	172,432	511,845	392	15·02	33·68
Pontypool	1896	35,011	994	2,226	38,231	68	6·39	5·82
"	1897	40,488	1,427	2,866	44,776	75	6·81	6·39

* Calculated by me.

Mr. Hammond.

Portsmouth	1896	839,392	...	...	900,490	660	15·57	...
Preston	1896	320,500	12,500	16,920	849,920	354	11·28	4·83
,, ...	1897	371,301	12,500	15,700	408,111	428	10·80	5·95
Reading	1896	82,165	5,148	9,038	96,351	178	6·16	9·38
,, ...	1897	123,702	4,568	10,677	138,947	216	7·37	7·68
Richmond	1896	97,044	1,891	6,836	105,771	135	8·92	6·46
,, ...	1897	138,916	2,071	11,065	152,052	185	9·38	7·27
Salford	1897	111,142	16,800	117,375	245,397	231	12·12	47·83
Sheffield	1896	483,427	...	152,644	636,071	642	11·27	23·99
,, ...	1897	747,063	...	296,365	1,043,428	886	13·44	28·37
Southampton...	1896	131,843	3,495	22,212	157,550	187	9·59	14·09
Southport	1896	245,515	7,797	82,552	335,864	257	14·91	24·28
South Shields	1897	109,543	7,145	21,691	138,379	126	12·53	15·67
Sunderland	1897	146,440	33,462	36,380	216,282	224	11·00	16·82
Taunton	1896	126,840	6,300	31,254	164,394	146	13·01	19·01
,, ...	1897	140,019	6,550	32,023	178,562	165	12·35	17·93
Walsall	1896	67,170	7,174	36,147	110,491	94	13·38	32·71
,, ...	1897	96,376	9,136	40,094	145,606	155	10·72	27·53
Whitehaven	1896	178,378	3,000	17,673	199,051	126	17·98	8·87
,, ...	1897	178,072	6,000	15,401	199,473	170	13·39	7·72
Worcester	1896	333,644	5,635	84,942	424,221	310	15·57	20·02
,, ...	1897	408,830	5,155	145,290	559,275	336	19·00	25·97
Yarmouth	1897	190,000	5,120	68,640	271,420	213	14·54	25·28

Mr. Hammond.

Table XII.—SHOWING (*a*) LOAD-FACTOR, AND (*b*) UNITS USED IN DISTRIBUTION—*continued.*

Metropolitan Undertakings.

WORKS.	Year.	Units Sold to Consumers, including Public Lighting.	Units used on Works, &c.	Units used in Distribution and unaccounted for, &c.	Total Units Generated.	Maximum Load of Year—K.W.	Load-Factor = Per cent. of Units Generated to Output of Maximum Load in use constantly throughout the Year.	Ratio of Units used in Distribution and unaccounted for, to Units Generated. Per cent.
Charing Cross	1896	1,944,402	81,887	279,432	2,305,721	1,066	24·62	12·98
,, ...	1897	2,615,508	93,879	457,809	3,167,214	1,378	26·25	14·45
Hampstead	1896	392,238	12,657	110,688	515,578	440	13·34	21·46
Islington	1897	503,572	45,480	99,877	648,929	585	12·66	15·39
St. James's	1896	2,401,431	28,682	257,855	2,687,968	2,052	14·91	9·59
Westminster	1896	3,503,054	51,249	514,881	4,069,184	3,000	15·44	12·65
,, ...	1897	4,355,781	61,096	629,623	5,046,500	3,650	15·78	12·47

31. After what I have said under the heading of Output, it will not be expected that works with high load-factors and low outputs will show better results than those with high outputs and Mr. Hammond

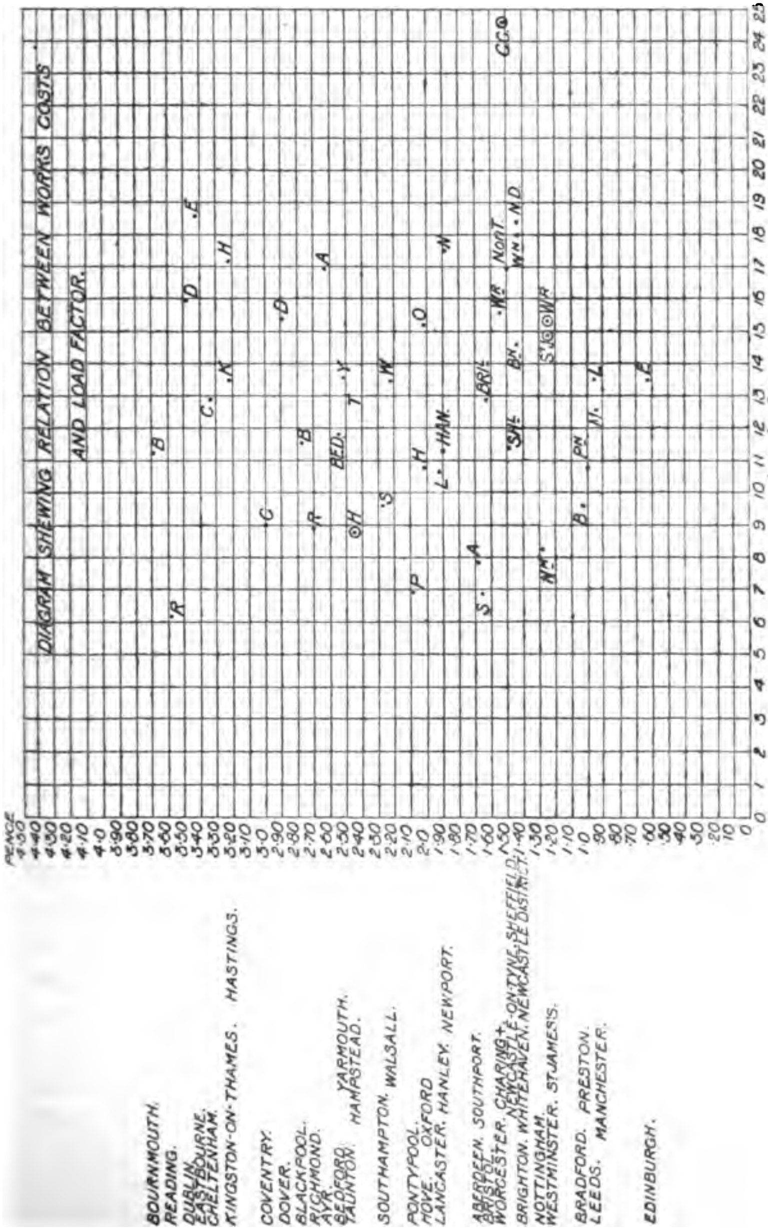

Diagram II.—Load-Factor.

low load-factors, but the curve supports the contention that the load factor has a marked influence on cost.

Mr. Hammond.

Note the positions on the diagram of some of the works that have achieved the lowest works costs per output, and it will be seen that the load-factors almost all rule high.

Names of Place.	1896 Load-Factor.
Aberdeen	7·85 %
Brighton	14·53 %
Charing Cross	24·62 %
Dundee	10·50 %
Edinburgh	13·48 %
Leeds	*13·44 %
Manchester	12·62 %
Newcastle District	18·33 %
Preston	11·28 %
St. James'...	14·91 %
Westminster	15·44 %
Whitehaven	17·98 %

Observe also that the load-factors vary from 6·16 per cent. to 24·62 per cent.; that the highest load-factor in the kingdom is found, as might naturally be expected, in the Charing Cross and Strand area.

Observe also that curves drawn through many of the points show the tendency of works costs per unit to fall in ratio to the increase of the load-factor.

32. In the discussion of Mr. Crompton's 1894 paper I drew attention to the fact that, whereas the cheapness of gas was due to the advantages arising from "by-products," so the cost of production of electrical energy would be greatly reduced if "by-uses" of the plant were general in those hours when the bulk of the lighting was not required.

Let us take some town as an illustration. Bristol may perhaps be chosen as a place where a day load is likely to develop. Consider what would be the effect on the costs per unit if the load-factor of 12·99 per cent.—equivalent to about 3 hours per day—were increased to 50 per cent., or 12 hours per day, with an increase in output from 650,758 units to, say, 2,500,000 units.

Taking output into consideration, the 1896 costs at Bristol are high. The coal works out 0·76d. per unit, as against 0·30d. achieved on an even smaller output in other alternating works.

* Calculated by the author.

Mr. Hammond.

The three-fold addition to the load, and its prolongation over 12 hours in place of the present 3 hours, would certainly reduce the coal bill to 0·30d. Oil, waste, water, would probably go down from 0·08d. to 0·04d. The amount paid in wages of workmen would only slightly be increased by the more favourable load-factor, as three shifts of men are doubtless engaged all the year round. In 1896 the wages item amounted to £1,443. It would be fair to assume that the increase of the load-factor by 300 per cent. would not affect the wages bill by more than 25 per cent., in which case the 0·53d. per unit for wages would be reduced to 0·17d. The repairs and maintenance expenditure item, standing at £713, would possibly be increased in proportion to the amount of work done by the plant. In any case it would not be greatly diminished from the 0·26d. at which it stood in the 1896 accounts. It might be fair to take it for the larger output at 0·20d.

Rents, rates, and taxes would suffer no addition, and would therefore drop down to 0·10d.; and management would hardly be increased by more than 50 per cent., thereby reducing this item to 0·19d.

To summarise:—

Table XIII.—Bristol Works.
(*With greatly improved Load-Factor.*)

—	1896 Cost of Production and Distribution of 650,758 Units.	Cost per Unit Sold of the 650,758 Units in 1896.	Estimated Costs of Production and Distribution of 2,500,000 Units, the increase being obtained by increase of Load-Factor.
	£	d.	d.
Coals	2,068	0·76	0·30
Oil	214	0·08	0·04
Wages	1,443	0·53	0·17
Repairs	713	0·26	0·20
Works Costs ...	4,438	1·63	0·71
Rent, &c.	1,019	0·37	0·10
Management ...	1,341	0·50	0·19
Total Costs ...	£6,798	2·50	1·00

Mr. Hammond.

33. Here, then, we have in a hypothetical case a reduction of costs per unit from 2½d. per unit to 1d. per unit, by means of a considerable increase in the load-factor.

Possibly I have not stated the case as strongly as I might, as the hypothetical works costs of 0·71d. are only on a par with those that have actually been attained by alternating works at Leeds.

The illustration will, however, serve to show how important an element in cost is load-factor, and how earnest the zealous works manager should be in his endeavour to secure a day load—one of the prime essentials towards a substantial reduction of cost per unit.

34. Before leaving this branch of the subject I desire to say a few words with regard to the current method of arriving at the load-factor.

It will be seen that I have followed the lines laid down by former writers; but I beg to point out that, though the method applied to each works in succession produces results which are reliable for the purpose of comparison, the load-factor arrived at by no means represents the percentage of use of the plant, and for the following reasons:—

In the present stage of every works in the country, almost without exception, there is a gradual increase throughout the whole of the year in the number of lights attached to the circuit, culminating usually in a maximum demand at the end of the year appreciably higher than that which obtained at the beginning. The result is that the ratio of the units actually generated to the product of the maximum demand (which only occurred at the end of the year) and the total hours of the year, is much greater than it would be if an average of the maximum demand at the beginning and at the end of the year were used as the basis of calculation; though it is obvious that such average maximum demand is the more correct basis to take in order to arrive at the ratio of actual demand for current to a demand extending over the whole 24 hours daily.

In order to illustrate my meaning, I have in the following cases taken the maximum demand of the close of 1895, averaged it with the maximum which occurred at the end of 1896, and set out the resultant load-factors (see Table XIV.).

Mr. Hammond.

Table XIV.—LOAD-FACTORS,

Worked out on the basis of averaging the maxima of the beginning and end of the year.

Name of Place.	Units Generated during 1896.	Mean of 1895 and 1896 Maximum Demand.	Load-Factor on Basis of Average Maximum Demand.	Load-Factor on Basis of 1896 Maximum Demand.
		K.W.		
Bradford	874,525	1,120	10·82	9·59
Brighton	1,653,576	1,107	17·00	14·53
Bristol	861,156	666	14·74	12·90
Edinburgh	1,917,504	1,318	16·56	13·48
Glasgow	1,729,483	1,593	12·36	9·53
Leeds	1,190,000	829	16·34	13·44
Manchester	3,028,357	2,410	14·30	12·62
Portsmouth	900,490	577	17·76	15·57
St. James's	2,687,968	1,866	16·39	14·91
Westminster	3,591,788	2,565	15·94	13·63

The new load-factor, of course, in each case, is higher than the one in current use, and it forms a much more reliable guide to the percentage of hours of demand upon the plant than the older method; though, in order to follow in the footsteps of my predecessors, I have adopted the older method in my tables and diagrams.

35. There is a third method which is sometimes suggested as more reliable than either of the two named, *i.e.*, the comparison of the ratio of the number of units sold to the product of the total of the maximum demands of the consumers into the hours of the year; but these data it is impossible to obtain, as in only a limited number of cases is a record made of the maximum demands of consumers.

The nearest approach to this, is a comparison of the units used per annum per light fixed; but too much reliance cannot be placed upon these data, as the works engineer can never be certain as to the number of the lights fixed. Doubtless the returns made to him by the consumer or by the wiring firm when the lights are first connected are correct, but he cannot rely upon alterations in the number of lights being

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reported to him. Indeed, it is obvious that the number of lights fixed on a consumer's premises is only of comparative interest to him, the real thing of moment being the resultant maximum demand.

As, however, some may deem it interesting to know what is the relation throughout the kingdom between the units sold per annum per lamp fixed, averaged over the year, and the price per unit, I have plotted the data on Diagram No. III.

By a comparison of Diagrams II. and III. it is possible to arrive at the ratio of lamps fixed to the average number in use throughout the year; but, as the information obtained, though striking, is of little practical value, I do not purpose setting it out on this occasion.

C.—*Reliability of Plant.*

36. Here I feel that I am on dangerous ground, for I lay myself open to the challenge to name any electrical plant manufactured in this country which is not perfectly reliable. My wish to-night is to avoid as far as possible provoking a discussion on the merits of particular engines, boilers, or dynamos. I would rather see the subject discussed on a broader basis.

On the other hand, though I desire to acknowledge to the full the indebtedness of the profession to those firms of manufacturers who in this country have placed excellence of workmanship above every other consideration, I trust I may be pardoned the expression of belief that in certain cases the pursuit after *efficiency* has sometimes resulted in the sacrifice of *reliability*.

Perfectly reliable generating and distributing plant has a very important bearing on costs.

37. It is my custom, and doubtless that of other consulting engineers, to insist upon very rigid and prolonged tests of the generating plant after the completion of erection on the site. These tests invariably include—

(*a*) Ten or 12 hours' consecutive running at full load.

(*b*) Two hours' running at emergency load.

(*c*) Governing.

(*d*) Whole load thrown suddenly on and off.

(*e*) Limit of temperature rise at end of the 10 or 12 hours' run.

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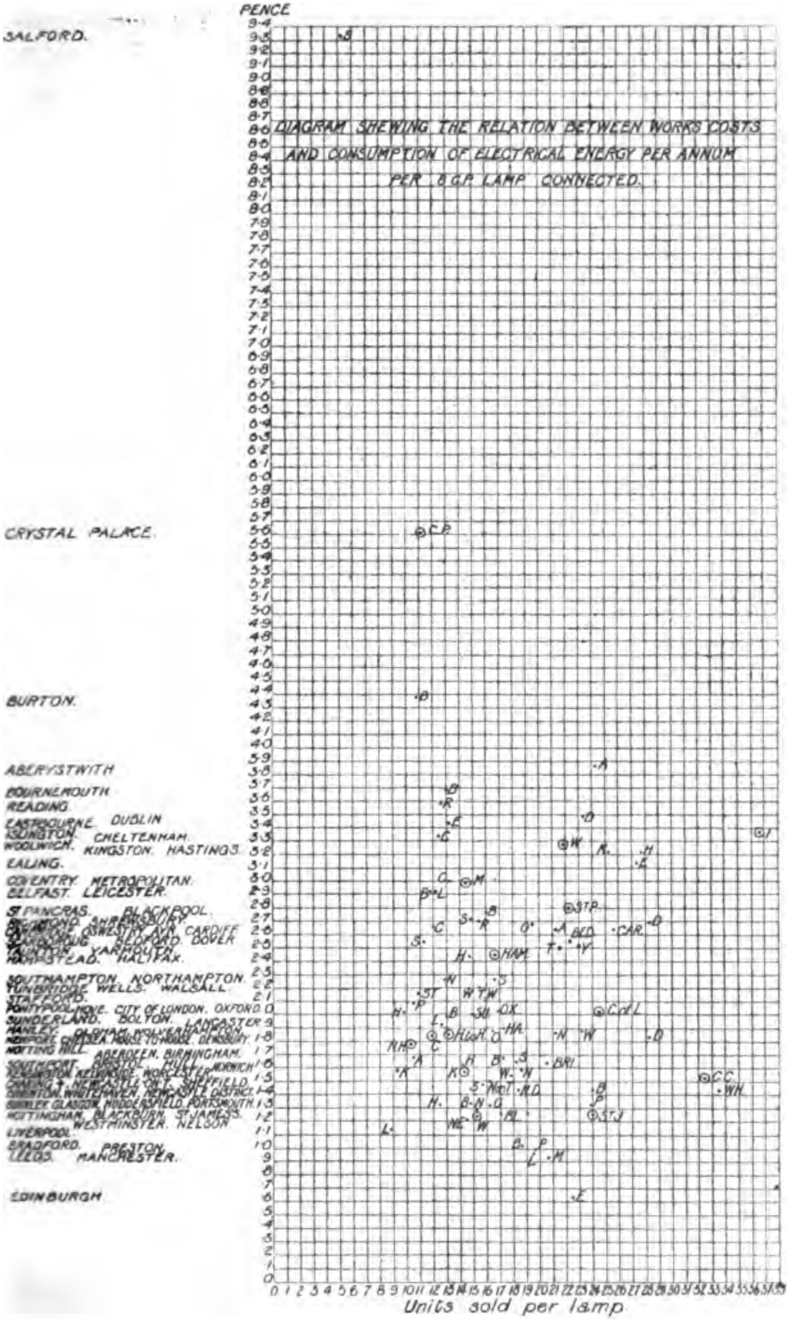


Diagram III.—Units per 8-C.P. Lamp per Annum.

Mr. Hammond.

Until all these tests (*inter alia*) have been fully complied with, the plant is not taken over. The almost invariable result is that when the plant has passed this ordeal it can be depended upon to cope with almost every emergency arising in the regular course of its work.

Plant that will go on year in and year out doing its duty, without the slightest hitch, is the first essential toward the running of works with the minimum expenditure on oil, wages, repairs, superintendence, &c.

If you desire to run your works economically, secure first of all, and at almost any cost, perfectly reliable plant.

D.—*The Engineer-Factor.*

38. This is an item that does not figure in the Board of Trade returns, but I venture to say that in every one of the works in this country where low costs prevail the engineer in charge is distinguished for ability, assiduity, and earnestness, combined with rigid weekly analysis of his works costs. There are, fortunately, a host of such in this country; but as I write the words I cannot help calling to mind an example of the man I mean, in the person of one of our members, Mr. Harold Dickinson, the engineer of the Leeds works. I mention him because I think his career forms an excellent pattern to our younger members. From the position of clerk of works, under me, of the first works at Leeds, he was promoted to the engineering management, and doggedly set before himself the task of running the works so economically that they should be second to none in the kingdom. The result is that, in spite of the low efficiency of rope-driven alternators and house-to-house converters, he has scored the record costs for output of any undertaking in the kingdom.

Certainly one of the greatest factors operating towards low costs is the engineer-factor.

E.—*Efficiency of Generating Plant.*

39. As the number of electricity works has increased, more and more attention has been paid to the important question of

efficiency. Valuable papers have from time to time been read before this and kindred Societies, or written for the technical Press, on the most efficient types of boilers, feed pumps, mechanical stokers, forced draught, steam ranges, engines, dynamos, condensers, &c. The persistency with which the electrical engineers' attention is directed to "efficiency" has had important results. Undoubtedly the plant available to-day for use in our electricity works has an all-round efficiency far higher than that which was current 10 years ago, and close consideration is given in most works to the best means of minimising "obscure "losses," in order to arrive at the *summum bonum* of the minimum of cost for the maximum of output. Mr. Hammond.

40. In carrying out my own work, I have made it a rule for some years to stipulate for a minimum consumption of water per kilowatt of electrical energy given off at the dynamo terminals, and to enforce this under a money penalty of about £1 per kilowatt per lb. of steam—that is to say, £100 per lb. in the case of 100-kilowatt plants, £200 in the case of 200-kilowatt plants, £300 in the case of 300-kilowatt plants, &c.

I specify that the measurement shall be made during the 10 or 12 hours' test after erection on site, and under the following conditions:—

(*a*) Engine working condensing with not less than 26 inches of vacuum.

(*b*) One or two boilers, at contractor's option, solely devoted to running the set of plant under test, and entirely under the control of the contractor; *the basis of the steam consumed to be the weight of water pumped into the boilers*, with the following deductions:—

(1) The steam for driving the feed pumps, the air and circulating pumps, the steam jet for the forced draught, &c., supplied by another boiler.

(2) All water drawn off during the test from the separator and steam range in use for the test to be deducted from the amount of that measured into the boilers.

Mr. Hammond.

Table XV.

Rated Full Load in Kilowatts.	Description of Engine.	Type of Valves.	Revolutions per Minute.	Method of Driving.	Type of Alternator.	Type of Boiler.	Steam Pressure at Engine.	Vacuum—Inches.	Steam per Kilowatt-Hour.	Remarks.
100	Slow-speed horizontal compound.	Piston valves ...	88	Ropes	Iron cores. Revolving multiple-coil magnets.	Lancashire	125	24	33·93	Separate condenser. Self-exciting.
100	Slow-speed horizontal compound.	Equilibrium valves. Trip gear.	99½	Direct	Iron cores. Revolving single-coil magnets.	Lancashire	125	26	25·94	Separate condenser. Self-exciting.
120	Slow-speed horizontal compound.	Equilibrium valves. Trip gear.	96	Direct	Iron cores. Revolving single-coil magnets.	Lancashire	125	25	26·37	Separate condenser and independent excitation.
200	Slow-speed horizontal compound.	Piston valves ..	97·5	Ropes	Iron cores. Revolving multiple-coil magnets.	Lancashire	125	25	30·7	Separate condenser. Self-Exciting.
200	Slow-speed horizontal compound.	Positive Corliss	91	Ropes	Iron cores. Revolving multiple-coil magnets.	Lancashire	125	...	30·61	Working own jet condenser and pump. Self-exciting.

Mr. Hammond.

(*c*) All portions of the range conveying the steam to the plant under test, which are not necessary to the test, to be blanked off.

(*d*) All pipes through which water might accidentally find its way into the boilers to be blanked off.

(*e*) The steam pressure in the H.P. steam chest to be kept as closely as possible at the steam pressure specified in the contract.

41. Under these conditions I have obtained with slow-speed alternating plants the results set forth in Table XV.

The recently erected 300-kilowatt high-speed alternating plants at Leeds were guaranteed to have a limit of steam consumption of 30 lbs. per kilowatt-hour, self-excited and with separate condensers, but in the preliminary tests this limit has been exceeded. The final "money guarantee" tests have not yet taken place, and the contractors fully expect, with the valves set to the most economical load, to fulfil the guarantee.

Under the above-named conditions I have recently, in the case of the plant for the Corporation of Gloucester, obtained a guarantee of 26 lbs. of steam per kilowatt-hour with a three-crank nine-cylinder triple-expansion engine, running at a speed of 350 revolutions, driving direct two 150-kilowatt continuous-current dynamos, and working with a steam pressure of 160 lbs.; also 30 lbs. of steam per kilowatt-hour with a two-crank four-cylinder compound engine, combined with two 75-kilowatt continuous-current dynamos.

42. Data as to the "efficiency" of the plant in use throughout the kingdom would prove most valuable, but, except in isolated cases, it is, I am sorry to say, unobtainable; but, in spite of the absence of official data, it is certain that in designing all new works, and in extending old ones, the closest attention is being paid to "efficiency" of generating plant.

F.—*Efficiency of Distribution.*

Form No. VIII. of the Board of Trade accounts is as follows:—

Mr. Hammond.

No. VIII. *Table XVI.*—Statement of Electricity Generated, Sold, &c.

Quantity generated in B.T. Units.	Quantity Sold.				Quantity used on Works.	Total Quantity accounted for.	Quantity not accounted for.	Number of Public Lamps.	Total Maximum Supply demanded.
	Public Lamps.	By Contract.	Private Consumers by Meter.	Total Sold.					

43. If a correct determination be made of the units:— Mr. Hammond

(*a*) Generated,

(*b*) Used at the works,

(*c*) Delivered to consumers,

it might be presumed that the balance "not accounted for" would represent

(*d*) The units used in distribution;

and the percentage of (*d*) to (*a*) give the distribution "losses."

This simple view of the matter is, however, not general. Some engineers consider that "magnetising watts" should be treated as "units used on works," and others place the "loss in "batteries" in the same category. Some determine by hypothesis or by calculation the "units used in distribution," and then, finding their totals fall short of their metered "generated units," fill in a balance figure under the head of "Units not accounted "for."

Some regard the whole difference between the "generated "units" and units delivered to consumers as "units unaccounted "for," while others urge that no electrical engineer is worthy of the name who is unable to account for all the units he generates, whether they be used in his batteries, feeders, transformers, or delivered through the consumers' meters.

In the case of the Westminster and St. James' undertakings the bold course is adopted of remodelling the Board of Trade form, per Table XVII.

44. This is certainly an improvement upon the standard form. The amendments which I would venture to suggest are the substitution of the heading "Used at Generating Works" for "*Used on Works*," the placing of "Quantity unaccounted for" under "Quantity expended in Distribution," the substitution of the word *units* throughout for that of *quantity*, and the inclusion of a heading under Distribution for transformers.

45. As already stated under the heading *Load-Factor*, the custom of metering the units generated is not general, and therefore in many cases the figure upon which to base the distribution losses is not available. Even, however, where the generated units are registered by meter, it is often impossible to

Mr. Hammond.

Table XVII.—STATEMENT OF ELECTRICITY GENERATED, SOLD, &c., *Year ending* 31*st December*, 1896.

Board of Trade Units Generated.	Quantity Utilised.			Quantity Expended in Distribution.			Quantity Accounted for.	Quantity Unaccounted for.	No. of Lamps on Circuit 31st Dec., 1896.
	Sold to Consumers.	Used on Works.	Total.	In Batteries.	In Feeders.	Total.			

Mr. Hammond.

accurately determine the distribution "losses," in view of the divergent methods already alluded to. I must confess to some disappointment at finding that the majority of works' engineers appear to be indifferent to the recording of their distribution losses, as it is obvious that a badly planned or obsolete method of distribution must seriously affect the costs.

Some seem to be under the impression that 5 per cent. more or less "efficiency" only affects the coal bill, and this is doubtless the case when the output of the works is small; but as the output grows, the "efficiency" factor tends more and more to affect the whole of the "works costs."

46. I have in Table XII. set out the percentage of units used in distribution, as far as I have been able to obtain them. I have in each case included under this designation the "units "unaccounted for." It will be seen that here the direct systems have naturally a great advantage over the transformer systems. In the latter systems, the bulk of the transformers, whether the current be continuous or alternating, are in circuit the whole of the 24 hours, and while in circuit are absorbing energy.

47. Irrespective, however, of the advantages that a system serving a compact area has over one serving a scattered district, there is a great field open to the engineer, working either system, to keep his distribution losses to a minimum. In the case of alternating systems house-to-house transformers must be replaced with large-sized transformers in sub-stations; and in the case of direct systems skill must be exercised in the planning of feeders and in the use of accumulators. The fruit of the various improvements made in different works throughout the country is marked by the lower distribution losses of 1897 over 1896.

G.—*All-round Efficiency.*

48. Here we have a combination of efficiency of generating plant and distribution system. The former is directly under the control of the engineer, and the latter largely so, though modified by the extent of the area to be served.

It is often said that a rough test of all-round efficiency of any works can be obtained by a study of the coal bill.

Mr. Hammond. Under this method the palm of efficiency would be awarded to the works with the lowest cost of coal per unit; but a general comparison, on this basis, of works situated in different parts of the country would be misleading, since the price of coal varies in various districts. It is no doubt possible to compare with safety the coal results of the undertakings at Kensington, Westminster, and St. James', or of those drawing from the same coal-fields, such as Cheltenham and Worcester, Blackpool and Southport, Huddersfield and Halifax, Bradford and Leeds; but how is it possible to compare Bradford with Cheltenham, or Leeds with St. James'?

Some reply, "Disregard cost per unit, and rely on *lbs. per* "*unit.*" Unfortunately, however, the statistics of coal consumption are only of partial use, as an indication of "efficiency," due to the fact that in different districts the coal differs in calorific value.

In some works it is possible to obtain low-class fuel at a very cheap rate, and in those the *lbs. per unit sold* are much greater than in works where only high-class fuel is obtainable.

49. If it were possible to obtain statistics of the consumption of British thermal units, we should have a basis of comparison for the whole of the works in the kingdom. I have indeed at various times endeavoured to obtain these data, but I have found it impossible to do so. It appears that very few engineers analyse their coal. They are content to go on putting forth their endeavours to reduce the *cost* of fuel per unit sold, and they disregard the anxiety of those individuals, like myself, who desire to see all the coal bills of the country put upon a scientific basis. When they are dissatisfied with one class of coal, they make a careful trial of another, and judge its merits on the amount of duty they extract from it.

50. In the course of my analytical work, however, it has frequently been suggested to me that, in spite of these drawbacks, I should make an attempt to classify coal results; and, as the first step in that direction, I have asked each works engineer to favour me with a note of the *quantity used* and the *average price paid* over the year.

I desire to express my great indebtedness to those who have

complied with my request. The data obtained is included in Table I. Mr. Hammond.

Though I am not able to set out calorific values, I am inclined to think that those who are acquainted with the price of the highest class coal in each district will be able to form a fair idea of the relative calorific values of the coal used in various works.

51. In any case, however, the statistics which I have been able to collate will prove of service in comparing the all-round efficiency of plant and distribution, in places, like the metropolis, where similar coal is used by almost all the works (see Table XVIII).

Table XVIII.—COAL STATISTICS OF METROPOLITAN UNDERTAKINGS.

METROPOLITAN WORKS.		COAL—1896.		
Name of Works.	Units Sold.	Per Unit Sold.	Lbs. per Unit Sold.	Average Price per Ton.
		d.		s. d.
Charing Cross	1,944,402	0·87	9·0	17 6
Chelsea	813,764	0·88	8·4	19 5
City of London	5,488,500	0·81	Information	unobtainable.
Crystal Palace	118,316	1·88	15·7	20 8
Hampstead	547,920	1·36	...	14 3
House-to-House	643,693	0·99	Information	unobtainable.
Islington	297,834	1·50	Information	unobtainable.
Kensington	1,514,729	0·67	6·6	18 6
Metropolitan	4,075,000	1·82	Information	unobtainable.
Notting Hill	230,787	0·71	7·0	18 9
St James's	2,401,431	0·50	6·4	14 0
St. Pancras	1,201,229	0·99	Information	unobtainable.
Westminster	3,503,054	0·53	6·5	15 4
Woolwich	75,929	1·75	18·1	18 0

Again the "alternating works" stand at the head of the list, while among the direct systems the St. James' Company comes out lowest, combined, curiously, with the lowest price paid for coal, viz., 14s. per ton.

I have shown in Diagram IV. the relation of coal consumption per unit to units sold in all the works in respect of which I have received the data, and I leave the figures to speak for themselves.

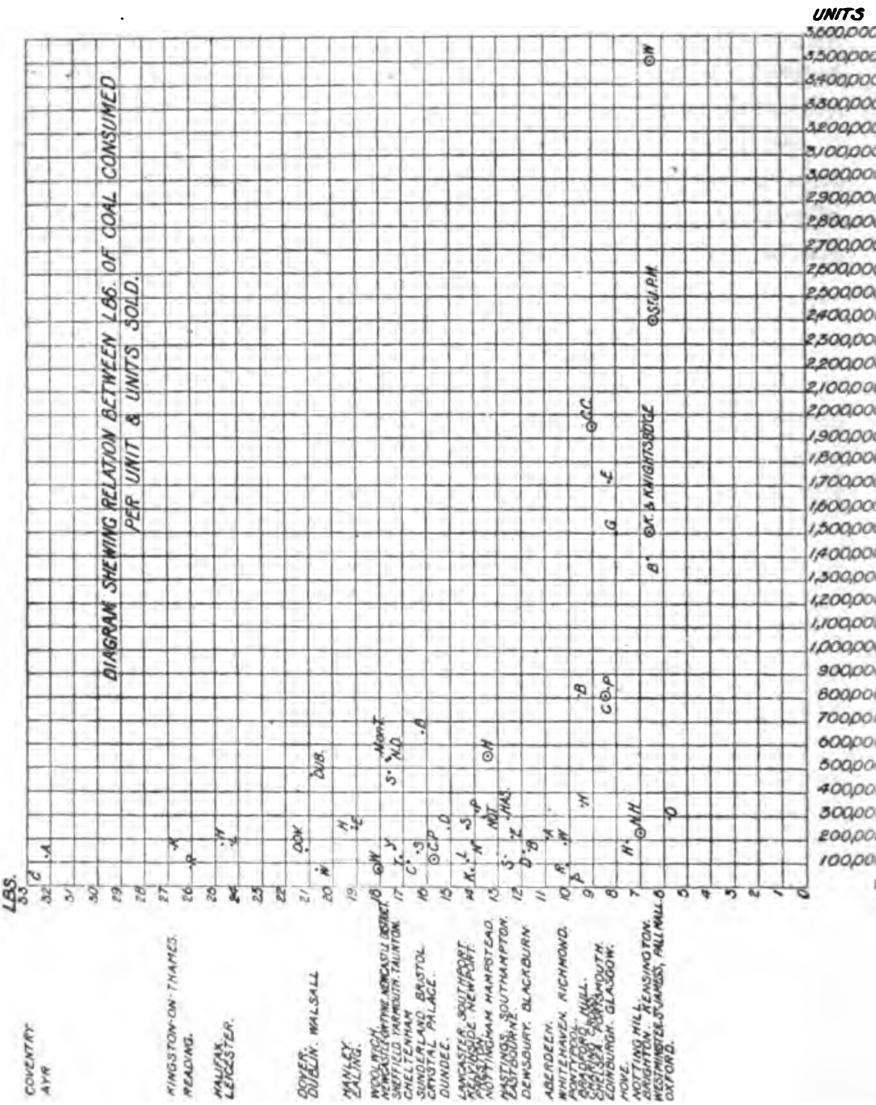


DIAGRAM IV.—LBS. OF COAL PER UNIT

SUMMARY.

Mr. Hammond.

52. So far I have, with one exception, considered the question of costs from a past, rather than a future, point of view; but I feel that I should not be doing justice to my subject if I did not devote some space to the consideration of the range of ultimate costs of production and distribution.

53. I have already, under a number of headings, set out what are the most important factors affecting the costs of production and distribution, and I now purpose considering the probable influence of these factors on the further reduction of same.

(a) *Output.*

54. In Diagrams I. (*a*) and I. (*b*) I have plotted out the relation of works cost per unit and units sold in the principal works in the kingdom, and I have already drawn attention to the falling costs that accompany increasing outputs.

Table XIX. will serve to further illustrate this fall.

Mr. Hammond.

Table XIX.—SHOWING THE FALL IN COSTS WHICH ACCOMPANIES INCREASE IN UNITS SOLD.

Per Diagram I. (*a*).

Name of Works.	Year.	Increase over previous Year.	PER UNIT SOLD. Decrease in Coal Cost.	Decrease in Oil, Waste, and Stores.	Decrease in Wages.	Decrease in Repairs and Maintenance.	Decrease in Works Cost.	Decrease in Rent, Rates, and Taxes.	Decrease in Management Expenses.	Decrease in Total Costs.
			d.	d.	d.	d.	d.	d.	d.	d.
Edinburgh	1896	833,222	0·07	0·03	0·10	0·09	0·29	0·06	0·19	0·54
Kensington	1893	188,965	0·18	Nil	0·07	0·04	0·29	0·11	Nil	0·40
,, ...	1894	253,489	0·02	0·05	0·03	0·27	0·37	0·03	0·19	0·59
,, ...	1895	250,937	0·04	0·01	0·06	0·18	0·29	+0·03	0·08	0·29
,, ...	1896	285,995	0·01	0·02	0·23	+0·01	0·25	+0·13	+0·21	+0·09
Leeds	1895	222,516	0·28	0·02	0·38	0·03	0·71	0·17	0·18	1·06
,, ...	1896	176,780	0·01	0·09	0·43	+0·03	0·50	+0·05	+0·13	0·32
,, ...	1897	131,871	0·04	0·02	0·03	0·09	0·18	Nil	0 05	0·23
Manchester	1895	597,862	0·08	0 08	0·17	+0·06	0·27	+0·03	0·13	0·37
,, ...	1896	760,344	0·02	0·04	0·12	0·10	0·28	0·02	0·05	0·35
Westminster	1892	590,371	0·33	0·13	0·12	+0·18	0·40	0·20	0·48	1·08
,, ...	1893	486,744	0·26	0·06	0·50	+0·16	0·66	0·09	0·06	0·81
,, ...	1894	468,683	0·13	0·06	0·13	0·21	0·53	0·13	0·08	0·74
,, ...	1895	657,098	0·07	0·02	0·18	0·01	0·23	0·05	0·10	0·38
,, ...	1896	672,658	0·05	0·02	0·28	0·02	0·27	+0·03	+0·02	0·22

+ = increase; − is omitted.

Mr. Hammond.

Table XIX.—Showing the Fall in Costs which accompanies Increase in Units Sold—*continued.*

Per Diagram I. (*b*).

Name of Works.	Year.	Increase over Previous Year.	PER UNIT SOLD. Decrease in Coal.	Decrease in Oil, Waste, and Stores.	Decrease in Wages.	Decrease in Repairs and Maintenance.	Decrease in Works Cost.	Decrease in Rent, Rates, and Taxes.	Decrease in Management Expenses.	Decrease in Total Costs.
			d.	d.	d.	d.	d.	d.	d.	d.
Bradford	1891	131,454	0·46	0·08	0·80	+0·03	1·31	0·40	+0·07	1·64
,, ...	1892	126,050	0·17	0·01	+0·18	0·15	0·15	0·05	0·17	0·37
,, ...	1893	115,203	+0·20	Nil	0·06	0·17	0·03	0·04	+0·07	Nil
,, ...	1894	74,019	0·08	+0·10	0·35	+0·20	0·13	0·02	+0·12	0·03
,, ...	1895	119,066	0·02	Nil	0·18	0·34	0·54	+0·07	+0·17	0·30
,, ...	1896	139,924	0·16	0·05	0·10	0·14	0·45	+0·04	0·02	0·43
Brighton	1893	130,785	0·23	0·05	0·08	+0·20	0·16	0·11	0·53	0·80
,, ...	1894	296,806	0·08	+0·06	0·62	+0·10	0·54	+0·04	0·16	0·66
,, ...	1895	283,793	0·11	0·08	+0·08	Nil	0·11	+0·08	+0·09	+0·06
,, ...	1896	521,327	0·03	0·05	0·12	0·06	0·26	+0·12	0·18	0·32
Bristol	1895	114,778	0·33	0·06	0·26	0·05	0·70	0·32	0·22	1·24
,, ...	1896	242,457	0·21	0·01	0·21	+0·10	0·33	0·24	0·12	0·69
Glasgow	1893	414,536	0·81	0·24	0·63	+0·58	1·10	0·05	0·08	1·23
,, ...	1894	199,029	Nil	0·02	0·06	0·25	0·33	+0·19	0·12	0·26
,, ...	1895	189,672	0·08	0·03	0·15	0·36	0·62	0·03	+0·07	0·58
,, ...	1896	406,883	0·06	Nil	0·13	0·11	0·30	0·07	0·15	0·52
Portsmouth	1896	433,274	0·11	0·01	0·18	+0·10	0·20	0·02	0·13	0·35

+ = increase; — is omitted.

Mr. Hammond.

It will be seen that I have in these tables analysed the fall in all the items that make up the cost of the unit sold. In addition, in order to shed the fullest light on the relation between costs per unit and increasing outputs, I have plotted a series of curves of the items—*i.e.*, coals, oil, waste, water and stores, wages, repairs and maintenance—which make up the cost of production and generation in the places referred to on Diagrams I. (*a*) and I. (*b*). See Diagrams V. to VIII.

I also show the curves of the total costs (Diagram XI.), and to complete the series I give the rent, rates, taxes, and management (Diagrams IX. and X.). I have adhered to the same scale as that of the *Works Costs*, except in case of the oil, waste, water, and stores, where I have plotted the ordinates to a much larger scale, for the sake of clearly displaying this comparatively small item.

55. These subsidiary curves set forth some most interesting features, which I will not, however, dwell upon at the moment, as the real question is, to what extent increasing outputs will be accompanied by reduction in costs per unit sold.

It will be seen on reference to Diagram I. (*a*) (works costs) that in the cases of both the St. James' and Westminster works the downward tendency of costs per unit has been checked on outputs respectively of 3,028,242 units and 4,355,781. I may add that the recently published accounts of the Charing Cross and Strand Company show the same check in reduction of costs. In each case I have made careful inquiries, and I am convinced that the non-reduction of costs is due to exceptional circumstances.

In the case of the Westminster Company an important rearrangement of the steam-raising plant has been made during the past year, and it is patent that while carrying on works in the midst of alteration economical production must in many directions be sacrificed. The net result of my investigation is that I feel safe in assuming that the St. James', the Westminster, and the Charing Cross Companies will in the current year all show lower costs than those for 1897, and that in the case of the other London companies whose outputs have not yet reached the amounts of these concerns the downward tendency of costs of production will continue.

Mr. Hammond.

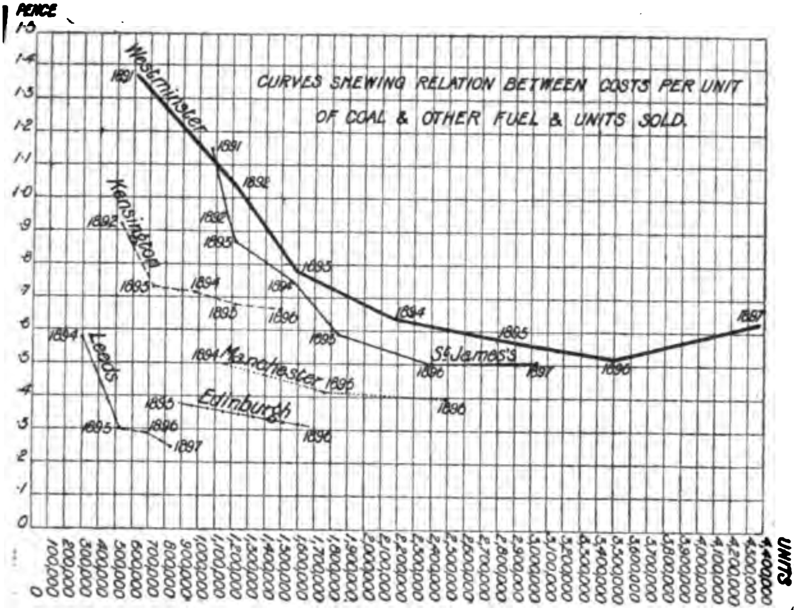
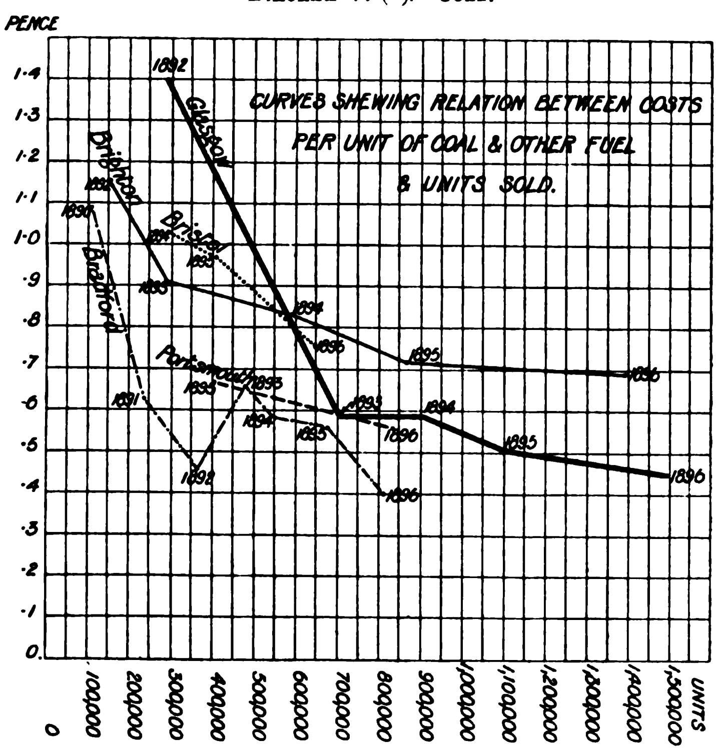


DIAGRAM V. (a).—COAL.



Mr. Hammond.

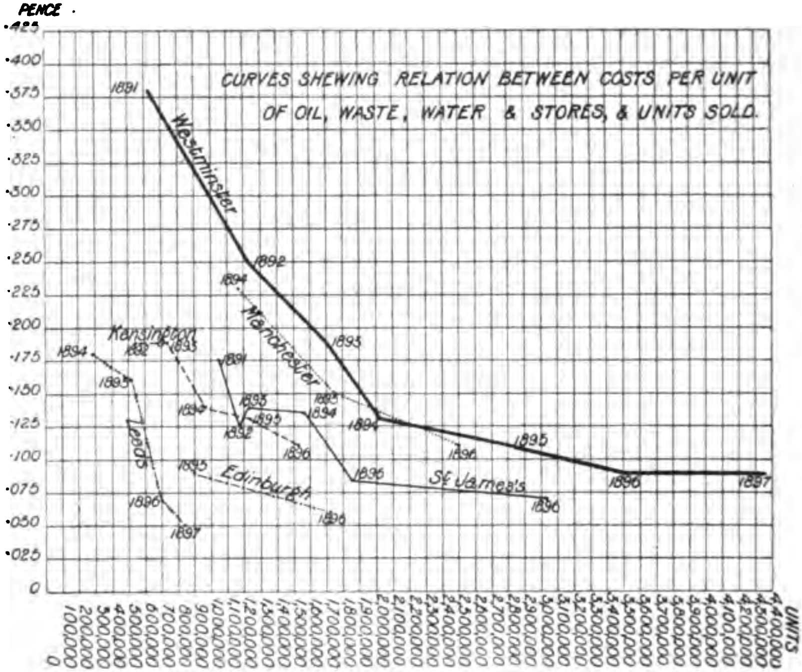


Diagram VI. (a).—Oil, Waste, Water, and Stores.

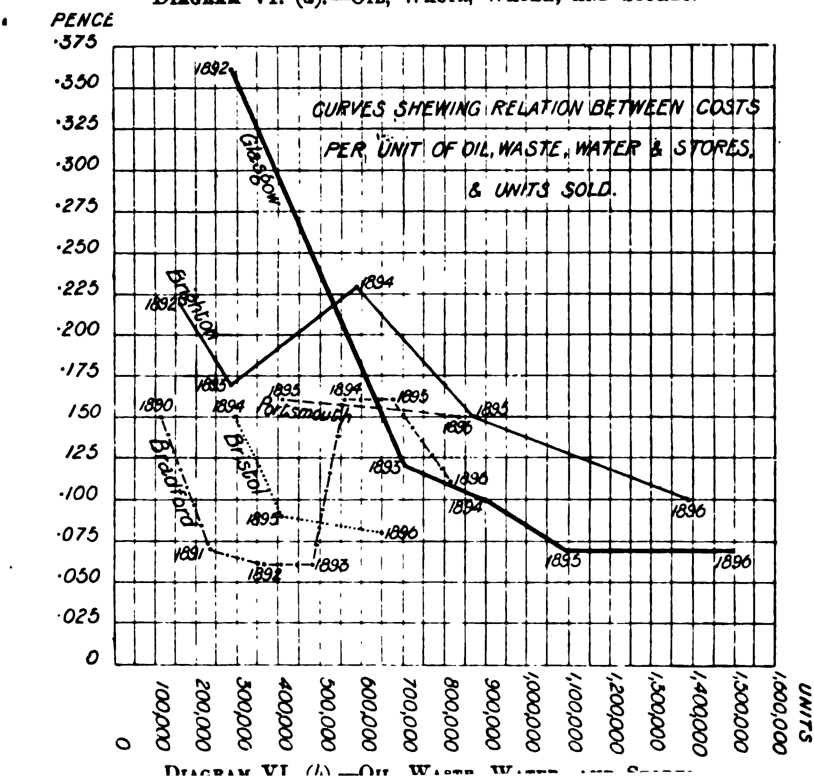


Diagram VI. (b).—Oil, Waste, Water, and Stores.

Mr.
Hammond.

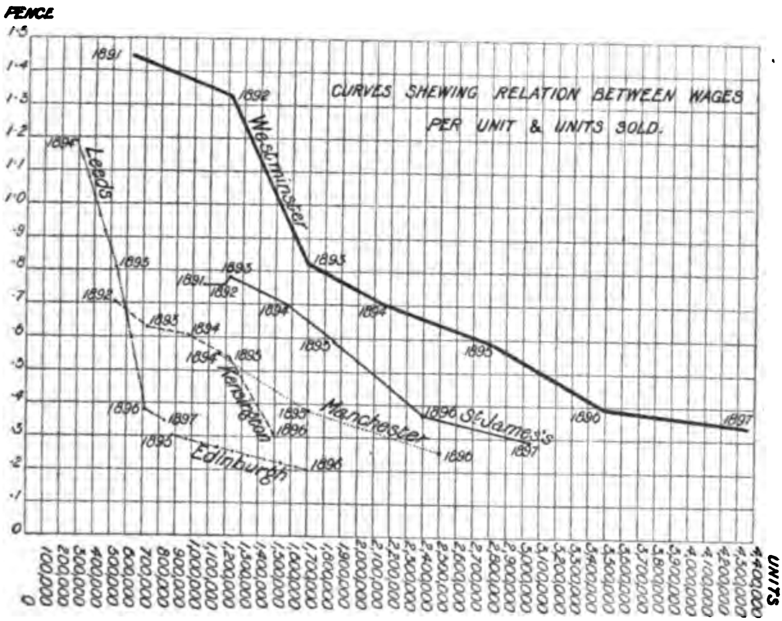


DIAGRAM VII. (a).—WAGES.

Mr. Hammond.

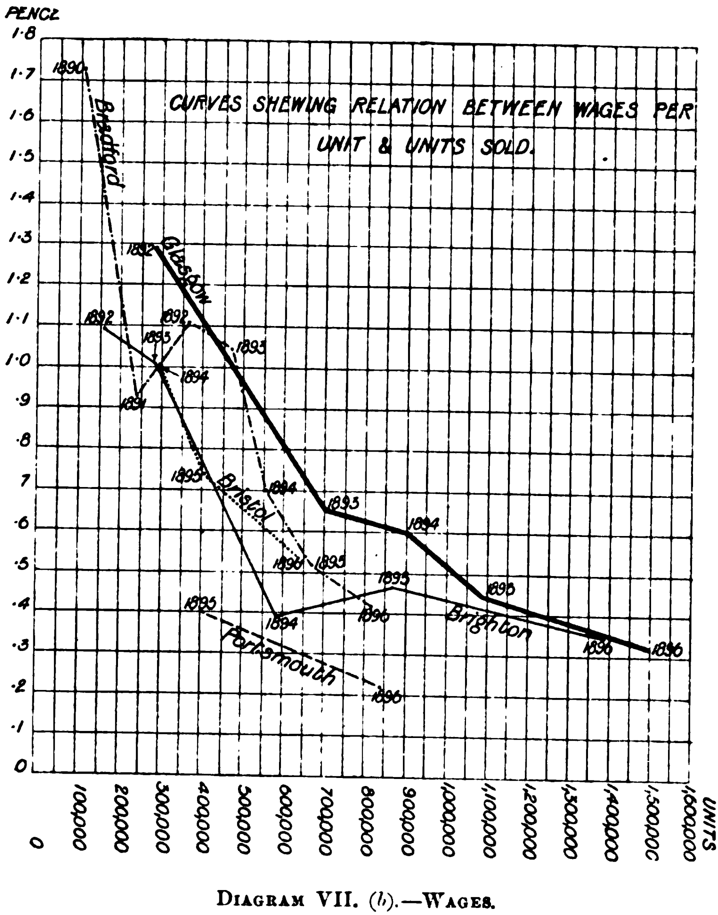


DIAGRAM VII. (b).—WAGES.

Mr. Hammon

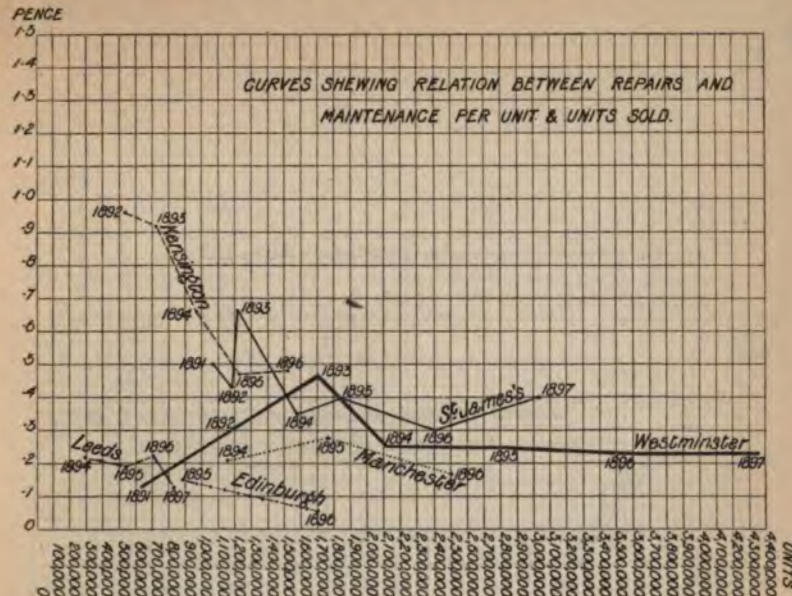


Diagram VIII. (a).—Repairs and Maintenance.

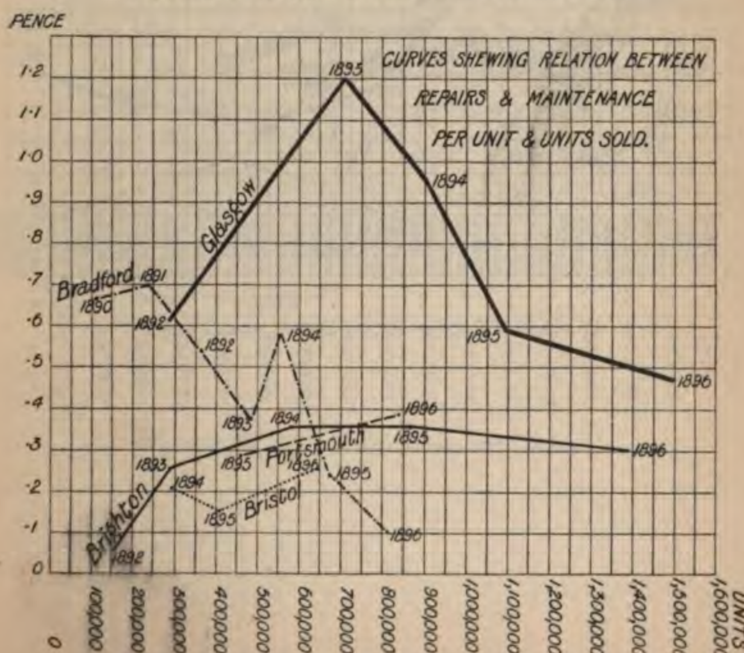


Diagram VIII. (b).—Repairs and Maintenance.

Mr. Hammond.

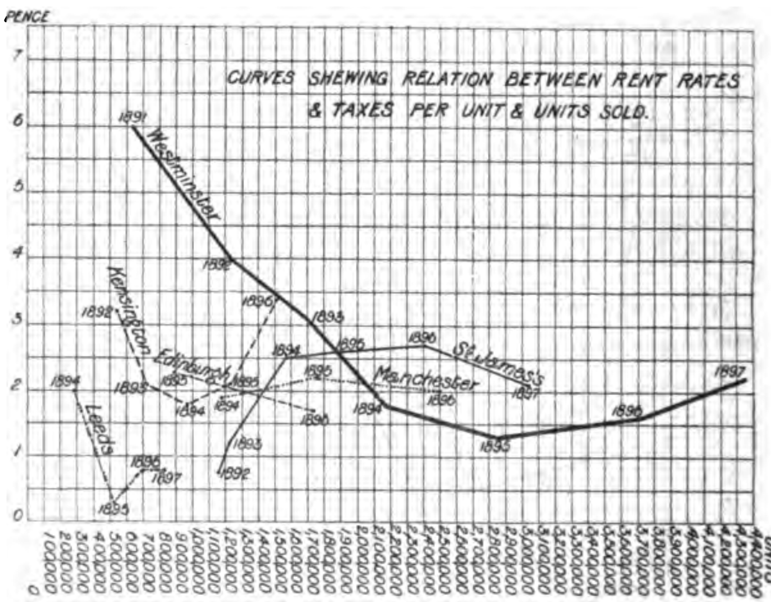


Diagram IX. (a).—Rent, Rates, and Taxes.

Mr. Hammond.

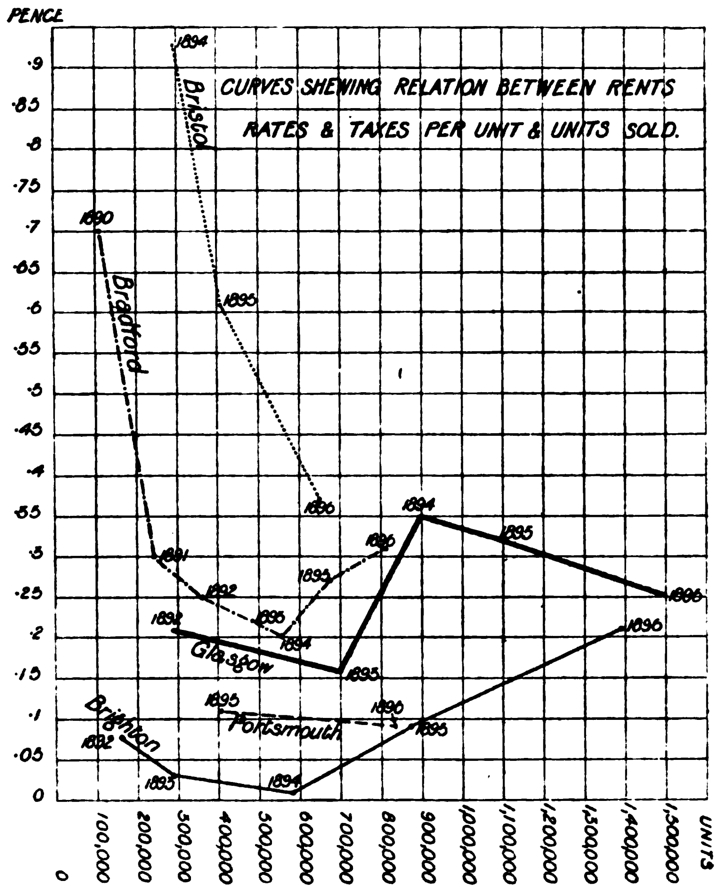


DIAGRAM IX. (*b*).—RENT, RATES, AND TAXES.

Mr. Hammond.

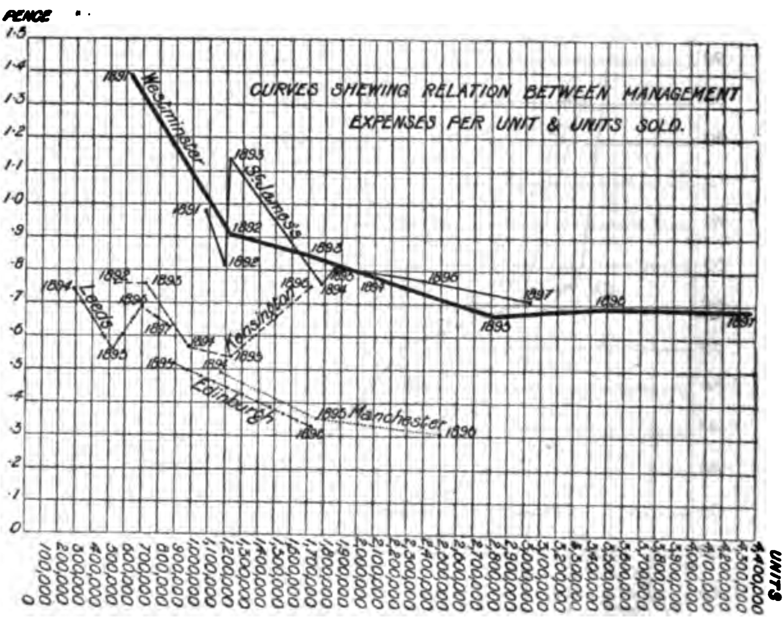


DIAGRAM X. (*a*).—MANAGEMENT.

Mr. Hammond.

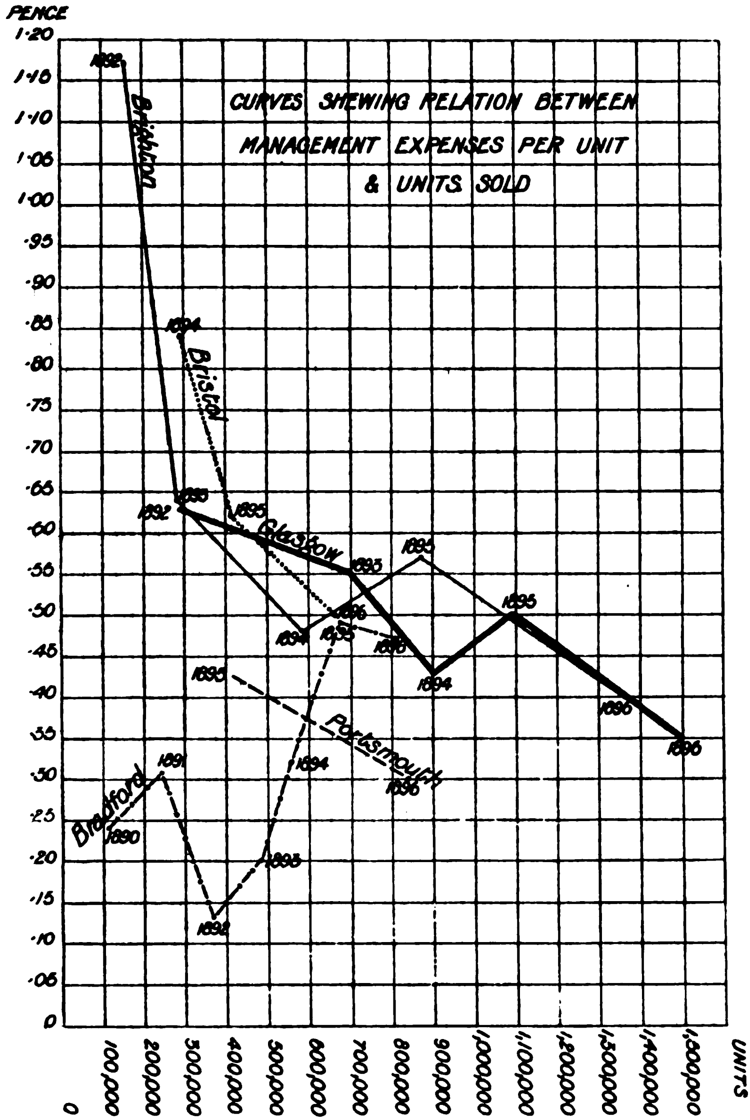


DIAGRAM X. (b).—MANAGEMENT.

Mr. Hammond.

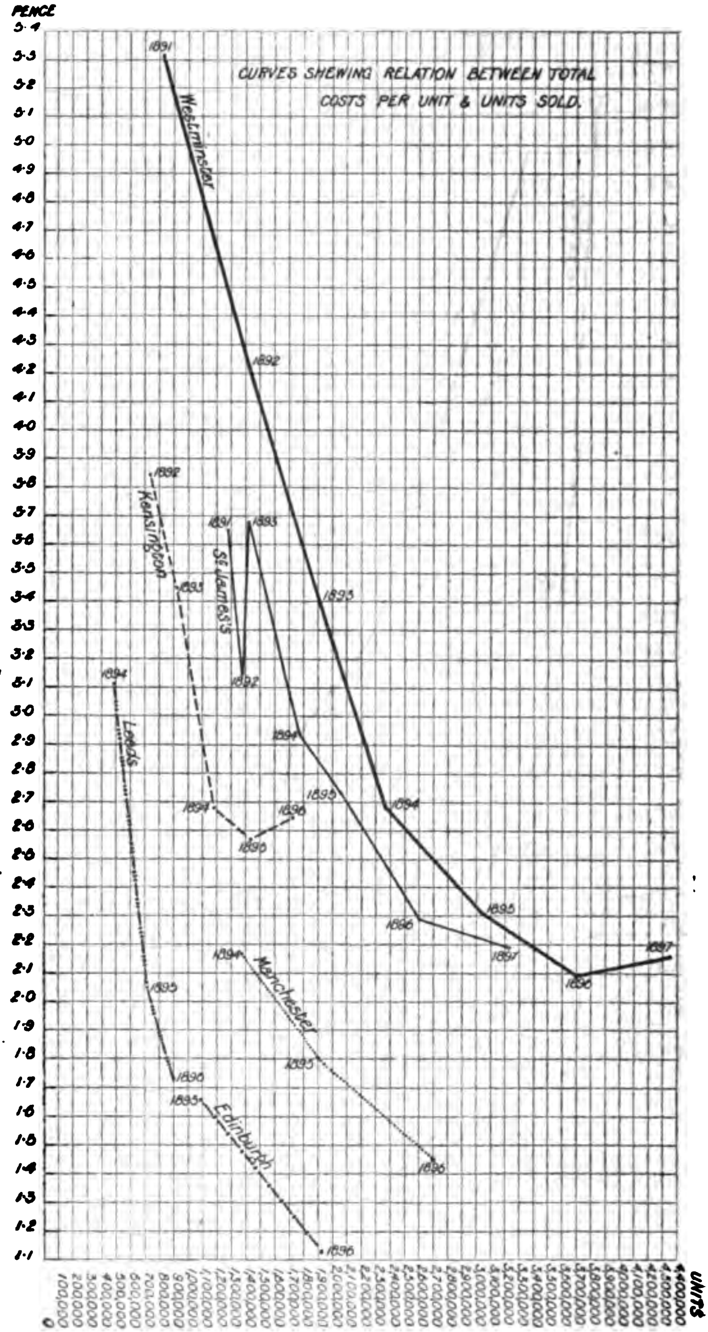


DIAGRAM XI. (*a*).—TOTAL COSTS.

Mr. Hammond.

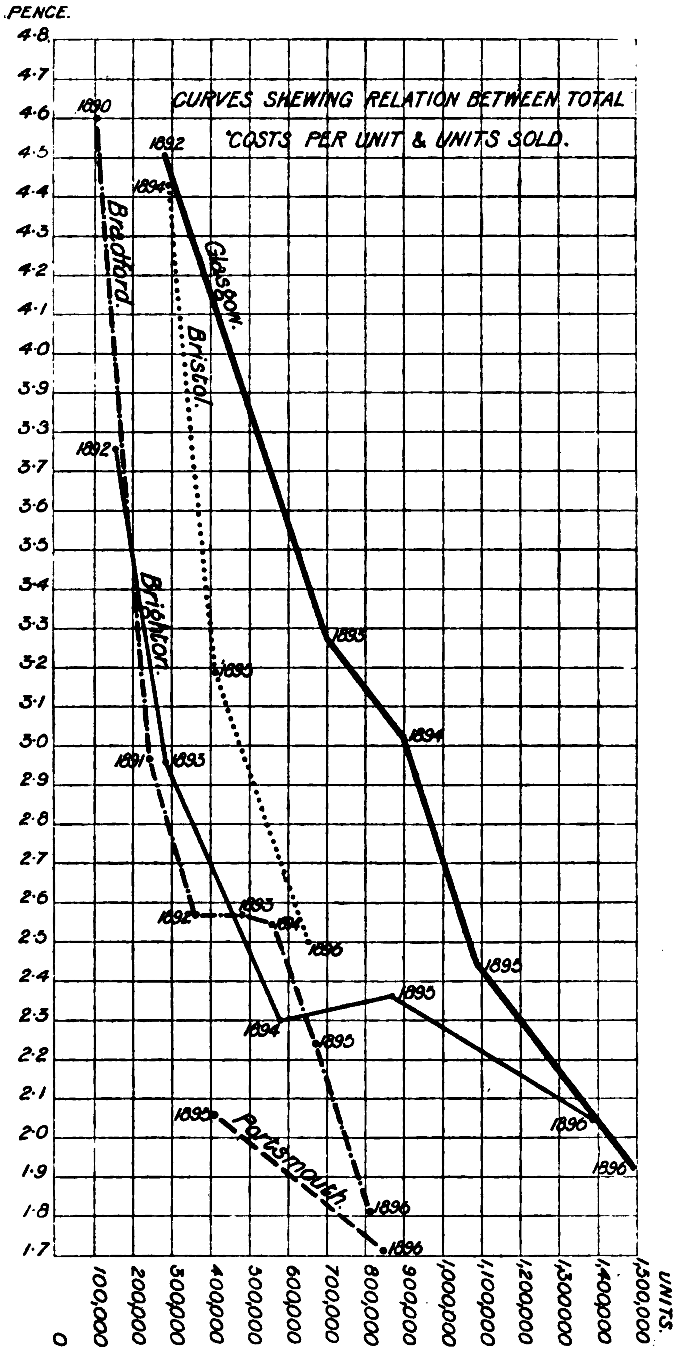


DIAGRAM XI. (b).—TOTAL COSTS

Mr. Hammond.

(*b*) *Load-Factor.*

56. Practically the whole of the costs set out in this paper are on the basis of the supply of electrical energy for lighting.

I have already pointed out that an increase in the load-factor would have a marked influence on costs.

The paramount business of the electrical engineer at the present moment is to discover "by-uses" for his plant.

In provincial towns and cities, if not in the metropolis, the supply of electrical energy for tramway traction would afford a splendid day load; and works that succeeded in reducing their costs on a lighting load to, say, 1·50 per unit (total costs) would probably have the costs reduced to 0·75 by the addition of one million units delivered to the tramway feeders between the hours of 6 a.m. and 10 p.m.

57. Some works engineers, in settling the price to be asked for electrical energy for tramway traction, have killed the business by demanding figures based upon the cost per unit for lighting.

It must always be borne in mind, in reference to lighting, that under the obligations of the Provisional Order "supply" has to be available constantly over the whole of the 24 hours, which involves the upkeep of a 24-hours staff for a full-load return of two to three hours.

A considerable increase in the load-factor would hardly have any effect on the standing charges, and would by no means increase the coal bill in proportion to the increased output. Works engineers would therefore always be pretty safe if they demanded for a "day load" supply a price that covered their "lighting" works costs.

They would get their profit out of the all-round reduction per unit which the day load would cause.

(*c*) *Reliability of Plant.*

58. My further remarks under this head may be brief. Obviously, the weeding out of unreliable plant, and its replacement by solid high-class apparatus, will result in decrease of costs of production.

Mr. Hammond

(d) *Engineer-Factor.*

59. The gradual improvement of status in the position of electricity works engineers is most encouraging, as it indicates the growing appreciation of the value of excellent management.

It is true that the higher salaries paid for greater efficiency will enhance "management" costs per unit; but this will be much more than counterbalanced by the lower works costs.

(e) *Efficiency of Generating Plant.*

(f) *Efficiency of Distribution.*

(g) *All-round Efficiency.*

60. Mr. Crompton, in his 1894 "ideal costs," made a forecast, on an output of 5,000,000 units, of a consumption of 2·5 lbs. per unit sold of Welsh coal of a calorific value of 14,500 B.T.U. per lb.; yet we find that no London works has yet succeeded in getting below 6·4 lbs. per unit sold.

I must confess that in works operating against the low load-factor presented by a lighting load, even in well-designed works, where the plant is so subdivided that, whatever the load, it is working at its maximum efficiency, there appears from the data before me little chance of the great reduction in the coal bill forecasted by Mr. Crompton.

The Westminster works had a consumption of coal in June last of 6·62 lbs. per unit sold; whereas in December, with a much heavier output, the coal only fell to 6·26.

The Charing Cross and Strand Company, blessed all the year round with a magnificent evening load, showed a consumption of 8·69 lbs. of coal in June, as compared with 8·76 in December.

In alternating works the relation between the June coal and December coal is of course much higher, as they have to contend with the fixed factor presented by the magnetising current.

There is one direction, however, in which coal economy will in the future be secured, viz., the provision of much larger and more economical plants than those already in use.

As long as the day load is limited, smaller engines working at their full output and at their best economy will be required for

Mr. Hammond. use during the day, leaving the heavy night load to be coped with by the larger and more economical plant.

The principle of an increase in the size of plant has been recognised at Manchester, where 1,500-kilowatt dynamos have been ordered; at the City of London works, where there is a 1,500-kilowatt plant at work; at Deptford, with its three 1,000-kilowatt and one 1,500-kilowatt plants; at Leeds, with its 600-kilowatt plant in course of manufacture.

H.—*Depreciation.*

61. Before closing my paper, I think it well to devote a few paragraphs to the question of depreciation. It will be seen by a reference to the Appendix that no provision is made by the Board of Trade in the "No. III.—Revenue Account" for a debit for depreciation; and wisely so, for that account purports to set forth those sums which have actually been disbursed, whereas "depreciation" is a sum determined upon by the undertakers themselves, and set aside in the form of sinking fund or reserve fund.

Accordingly, provision is made for the transfer of the balance of revenue account to "No. IV.—Net Revenue Account," and in this account will be noticed the two items of sinking fund and reserve fund. With regard to the former, the Local Government Board, in the case of the undertakings of local authorities, usually fixes 25 years as the limit of time over which the loan to cover the capital expenditure may be repaid; thus imposing an obligation to extinguish the whole cost of the plant by the end of the period named, and necessitating the setting aside annually of almost 3 per cent. upon the amount of the capital expenditure,—and in most cases this is considered a sufficiently important contribution towards depreciation.

62. In others, however, a further sum is set aside annually to a reserve fund, which is sanctioned by clause 5, section 52, of the Model Provisional Order; thus:—

Application of Moneys received.

Application of Revenue.

52. All moneys from time to time received by the Undertakers in respect of the undertaking, except (*a*) borrowed money, (*b*) money arising from the disposal

of lands acquired for the purposes of this Order, and (c) money not of the nature of rent received by them in respect of any transfer under the provisions of this Order, shall be applied by them as follows:— Mr. Hammond.

(1.) In payment of the working and establishment expenses and cost of maintenance of the undertaking, including all costs, expenses, penalties, and damages incurred or payable by the Undertakers consequent upon any proceedings by or against the Undertakers, their officers or servants, in relation to the undertaking.

(2.) In payment of the interest or dividend on any mortgages, stock, or other securities granted and issued by the Undertakers in respect of money borrowed for electricity purposes.

(3.) In providing any instalments or sinking fund required to be provided in respect of moneys borrowed for electricity purposes.

(4.) In payment of all other their expenses of executing this Order, not being expenses properly chargeable to capital.

(5.) In providing a reserve fund, if they think fit, by setting aside such money as they may from time to time think reasonable, and investing the same and the resulting income thereof in Government securities or in any other securities in which trustees are by law for the time being authorised to invest, other than stock or securities of the Undertakers, and accumulating the same at compound interest until the fund so formed amounts to one-tenth of the aggregate capital expenditure of the Undertakers on the undertaking, which fund shall be applicable from time to time to answer any deficiency at any time happening in the income of the Undertakers from the undertaking or to meet any extraordinary claim or demand at any time arising against the Undertakers in respect of the undertaking, and so that if that fund is at any time reduced it may thereafter be again restored to the prescribed limit, and so from time to time as often as such reduction happens.

The Undertakers shall carry the net surplus remaining in any year and the annual proceeds of the reserve fund when amounting to the prescribed limit to the credit of the local rate as defined by the principal Act, or at their option shall apply such surplus or some part thereof to the improvement of the district for which they are the local authority or in reduction of the capital moneys borrowed for electricity purposes.

Provided always that if the surplus in any year exceed five pounds per centum per annum upon the aggregate capital expenditure on the undertaking the Undertakers shall make such a rateable reduction in the charge for the supply of energy as in their judgment will reduce the surplus to the said maximum rate of profit, but this proviso shall only apply to so much of the undertaking as shall for the time being remain in the hands of the Undertakers.

Any deficiency of income in any year shall be charged upon and payable out of the local rate.

Application of Capital Moneys.

53. All moneys arising from the disposal of lands acquired by the Undertakers for the purpose of this Order, and all moneys not of the nature of rent

Mr. Hammond.

received by them in respect of any transfer of the undertaking under the provisions of this Order, and all other capital moneys received by them in respect of the undertaking, shall be applied by them as follows :—

(1.) In the reduction of the capital moneys borrowed by them for electricity purposes.

(2.) In the reduction of the capital moneys borrowed by them for other than electricity purposes.

63. Under the above section it is permissible, then, for a local authority to create a reserve or depreciation fund in addition to setting aside a certain amount annually to the sinking fund for the amortisation of capital. It will be noted, however, that this reserve fund is limited to 10 per cent. of the capital expenditure. The question arises whether there is any need for a local authority to set aside a sum for depreciation beyond that which is obligatory upon them in the form of sinking fund.

It is pointed out on the one hand that it is impossible to run electricity supply works efficiently without maintaining them at a high state of efficiency, which maintenance entails the expenditure of an annual sum out of revenue. On the other, as I pointed out at the discussion on Mr. Crompton's paper in 1894, plant tends to become antiquated, and if the sinking fund were deemed sufficient to cover depreciation it might be urged that a further sum should be put aside to cover *antiquation*.

64. The whole question turns upon the point whether an annual allowance of about 3 per cent. all round upon the capital expenditure is sufficient to provide for depreciation and antiquation. Most local authorities deem the sinking fund of about this amount sufficient for all purposes, with which view I concur; but Glasgow, for instance, makes deductions, on the following bases :—

	Per Cent.
Land and buildings	1
Machinery and plant	7½
Accumulators	10
Mains and cables	2½
Meters	7½
Electrical instruments	5
Furniture	5

The aggregate of the amounts so deducted for 1896 equals 6·36 per cent. upon the average capital for that year.

Mr. Hammond.

Aberdeen adopts the following bases :—

	Per Cent
Buildings	1
Machinery	5
Mains and services	1½
Electrical instruments	2½
Meters	5

which upon the average capital gave, in 1896, an all-round percentage of 2·60 per cent.

In addition to the sinking fund, Bradford set aside in 1896 for depreciation the sum of £3,060, which upon the average capital was equal to 3·46 per cent.

Several other local authorities, too, provide annually a sum for depreciation.

65. Companies, who are not burdened with an obligatory sinking fund for the redemption of their capital, pay less attention to this item, and are contented in setting aside either for reserve or depreciation much less sums than those set aside by local authorities.

On the whole, I am of the opinion that companies do not debit their net revenue account with a sufficient item under this head, and I venture to suggest that they would put their concerns into a sounder position if, before distributing dividend, they set aside annually to a reserve or depreciation fund at least 3 per cent. upon their capital expenditure.

The only companies that set aside this percentage in 1896–1897 are the following (see Table XX.) :—

Table XX.—AMOUNTS ALLOWED FOR DEPRECIATION BY SOME COMPANIES.

Name of Place.	Year.	Amount set aside for Depreciation and Reserve.	Percentage to Average Capital Outlay during the Year.
		£	d.
Norwich	1896	2,500	4·18
Leeds	1897	5,439	4·08
House-to-House	1896	4,704	3·96
St. James's	1897	9,656	3·89
Sheffield	1896	2,794	3·59
Kensington	1896	7,388	3·38
Westminster	1897	15,415	2·96

Mr. Hammond.

Taking Westminster as a basis, the setting aside of 3 per cent. for depreciation and reserve would have entailed a charge of 0·85d. per unit sold, so enhancing the cost from 2·19d. per unit to 3·04d. per unit.

A charge of 0·85d. per unit is indeed a heavy one, and if it were compulsory to add it to each unit sold it would render the price to the consumer prohibitive for power purposes.

The increase of the load-factor in any works, through the introduction of a demand for "power," would greatly reduce the "depreciation charge" per unit sold.

Thus, taking a case where the depreciation charge worked out 1·00d. per unit on a load-factor of 12·50 per cent. and "Units sold" of 1,000,000 units, the effect of doubling the load-factor and the units sold would be to divide the "depreciation charge" per unit thus;—

Units Sold.	Load-Factor.	Depreciation Charge per Unit.
	Per Cent.	d.
1,000,000 ...	12·50 ...	1·00
2,000,000 ...	25·00 ...	0·50
3,000,000 ...	37·50 ...	0·33
4,000,000 ...	50·00 ...	0·25

Conclusion.

66. As the Institution in 1894 had a prophecy laid before it of the reduction of costs in London works to 1·32d. per unit. sold, on an output of 5,000,000 units, I will venture upon a prophecy as to the reduction of costs in works situated in an industrial centre, and I will locate my prophecy at Leeds. My opinion is that when the Leeds output reaches the ideal figure of 5,000,000 units the costs of production and distribution will work out somewhat as follows:—

Prophetic Leeds Costs when Output reaches 5,000,000 *Units.* Mr. Hammond

	Per Unit Sold. d.
Coal and other fuel	0·20
Oil, waste, water, and stores	0·03
Wages	0·17
Repairs and maintenance	0·10
Works Costs ...	0·50
Rates and taxes	0·03
Management expenses, including engineer's superintendence	0·22
Total Costs ...	0·75

Before resuming my seat I would like once more to express my great thanks to those gentlemen who, during the past few years, have so cheerfully and fully supplied me with the figures, upon which I have been able to base the data that I have had the honour of bringing before the Institution this evening.

Mr. Hammond.

APPENDIX I.

Electric Lighting Acts, 1882 to 1890.

FORM OF ACCOUNTS PRESCRIBED BY THE BOARD OF TRADE FOR A LOCAL AUTHORITY.

______________________ELECTRIC LIGHTING ORDER (LICENSE).

THE *(Name of Local Authority)*

Year ending 31*st December*, 18 .

I.—Statement as to Loans Authorised for the Purposes of the Undertaking referred to in the above-mentioned Order (License) prior to the 31st December, 18 .

Amounts Sanctioned.	Amount Borrowed.				Amount Repaid.				Amount Sanctioned but not Borrowed.
	At %.	At %.	At %.	Total.	A %.	At %.	At %.	Total.	

Mr. Hammond.

Dr. CAPITAL ACCOUNT Cr.

No. II. for the Year ending 31st December, 18 .

	Expenditure up to 31 Dec., 18 . £ s. d.	Expended during the Year. £ s. d.	Total Expenditure to 31 Dec., 18 . £ s. d.
To expenditure to 31st December, 18 *Expenditure since that date.*			
1. To lands, including law charges incidental to acquisition.			
2. To value of lands appropriated for electrical purposes, as per contra.			
3. To buildings			
4 To machinery			
5. To accumulators at generating and distributing stations.			
6. To mains, including cost of laying the mains and services.			
7 To transformers, motors, &c. ...			
8. To meters, and fees for certifying			
9. To electrical instruments, &c. ...			
10. To general stores (cable, mains, lamps).			
11. To purchase of patents or patent rights.			
12. To transfer to sinking fund of value of lands sold, as per contra.			
13. To amount applied to the reduction of principal of borrowed money from value of lands sold, as per contra.			
14. To other items (to be specified)			
Total expenditure			
To balance of Capital account ...	...	...	
			£

	Receipts up to 31 Dec., 18 . £ s. d.	Received during the Year. £ s. d.	Total Receipts to 31 Dec., 18 . £ s. d.
1. By amount raised by Loans			
2. By sale of patents or patent rights, &c.			
3. By value of lands belonging to authority appropriated for electrical purposes.			
4. By value of surplus lands sold ...			
5. By other receipts (to be specified)			
		£	

Mr. Hammond.

Dr. **REVENUE ACCOUNT.** Cr.

No. III. **For the Year ending 31st December, 18 .**

	£ s. d.	£ s. d.
A.—*To generation of Electricity.*		
1. To coals or other fuel, including dues, carriage, unloading, storing, and all expenses of placing the same on the works.		
2. To oil, waste, water, and engine-room stores		
3. To wages at generating stations		
4. To repairs and maintenance, as follows :— (£ s. d.)		
Buildings		
Engines, boilers ...		
Dynamos, exciters, transformers, motors, &c.		
Other machinery, instruments, and tools.		
Accumulators and accessories.		
Less received for old material...		
5. To other items (to be specified)		
Carried forward ... £		

	£ s. d.	£ s. d.	£ s. d.
1. By balance from last account ...			
Less bad debts written off ..			
2. By sale of current per meter at per B.T.U.			
2. By sale under contracts			
4. By public lighting			
5. By rental of meters and other apparatus on consumers' premises.			
6. By sale and repairs of lamps, arc or incandescent.			
By sale and repairs of other apparatus ...			
7. By royalties, licences, &c.			
8. By rents receivable			
9. By fees for inspection of maps			
10. By other items (to be specified)			
Carried forward ... £			

Mr. Harmond.

Dr. Cr.

No. III.—*cont.* REVENUE ACCOUNT—*cont.*

	£ s. d.	£ s. d.		£ s. d.	£ s. d.
Brought forward			Brought forward		
B.—*To distribution of Electricity.*					
1. To wages and other remuneration to linesmen, fitters, labourers.					
2. To repairs, maintenance, and renewals of mains of all classes, including materials and laying the same.					
Less amounts refunded					
3. To repairs, maintenance, and renewals of transformers, meters, switches, fuses, and other apparatus on consumers' premises, together with cost of materials and lamps sold, as per contra.					
4. To repairs, maintenance, and renewals of apparatus at distributing stations.					
C.—*To Public Lamps.*					
1. To attending and repairs					
2. To renewals of lamps...					
Carried forward ... £			Carried forward ... £		

Mr. Hammond.

Dr. Cr.

No. III.—*cont.* REVENUE ACCOUNT—*cont.*

	£ s. d.	£ s. d.		£ s. d.	£ s. d.
Brought forward			Brought forward		
D.—*To Royalties, &c.*					
To Royalties, &c., payable for use of patents or patent processes.					
E.—*To Rents, Rates, and Taxes.*					
1 To rents payable					
2. To rates and taxes					
F.—*To Management Expenses.*					
1. To salaries, viz. :— Engineers' Department					
Accountant and Clerical Staff ...					
2. To salaries or commissions of Collectors ...					
3 To stationery and printing					
4. To general establishment charges					
Carried forward ... £			Carried forward .. £		

Mr. Hammond.

Dr. Cr.

No. III.—*cont.*

REVENUE ACCOUNT—*cont.*

	£ s. d.	£ s. d.		£ s. d.	£ s. d.
Brought forward			Brought forward		
G.—*To Law and Parliamentary Charges.*					
1. To law expenses					
H.—*To Special Charges.*					
1. To insurances, &c.					
2. To expenses for certification of meters ...					
Total expenditure					
Amount carried to net revenue account ...					
Balance carried to next account to provide for bad debts.					
		£			£

Mr. Hammond.

No. IV.

NET REVENUE ACCOUNT.

Dr.	£ s. d.	Cr.	£ s. d.
1. To interest on mortgage debt accrued due to date...		1. By balance from last account	
2. To instalments of principal of money borrowed ...		2. By balance brought from Revenue Account (No. III.)	
3. To amount transferred to sinking fund where such fund is authorised.		3. By interest on money at deposit	
4. To expenses of executing the Order (License) not included in III. and not chargeable to capital.			
5. To payments to reserve fund, where such fund is authorised by the Order (License).			
6 To sum applied to Local Rate			
To balance carried forward ...			
£		£	

Mr. Hammond.

Dr. Cr.

No. V. SINKING FUND ACCOUNT.

	Stock. £ s. d.	£ s. d.		Stock. £ s. d.	£ s. d.
1 To amount paid for purchase of (nature of investment to be specified).			1. By balance brought from last account ...		
2. To stock sold during period of account ...			2. By amount brought from Net Revenue Account.		
3. To amount of principal of borrowed money repaid.			3. By interest on investments		
To amount of balance to next account			4. By value of lands transferred from Account II.		
			5. By amount realised by sale of stock (nature of stock to be specified).		
			6. By stock purchased		
£			£		

Dr Cr.

No. VI. RESERVE FUND ACCOUNT.

	Stock. £ s. d.	£ s. d.		Stock. £ s. d.	£ s. d.
1. To amount paid for purchase of (nature of investment to be specified).			1. By balance brought from last account ...		
2. To stock sold			2. By amount transferred from Net Revenue Account.		
3. To sum transferred to Revenue Account ...			3. By stock purchased		
To amount of balance to next account			4. By amount realised by sale of stock (nature of stock to be specified).		
£			£		

Mr. Hammond.

Dr. Cr.

No. VII. GENERAL BALANCE SHEET.

Liabilities.	£ s. d.	*Assets.*	£ s. d.	£ s. d.
1 To Capital Account: Amount received as per Account No. II.		1. By Capital Account: Amount expended for works as per Account No. II.		
2. To sundry creditors		2. By stores on hand at 31st December, 18 :—		
3. To Net Revenue Account: Balance at Credit thereof.		Coal		
		Oils, waste, &c.... ...		
4. To Sinking Fund Account		General		
5. To Reserve Fund Account		3. By sundry debtors for current supplied to 31st December, 18 .		
6. To other items (to be specified)		4. By other debtors		
		5. By securities held (cost price)...		
		6. By other items (to be specified)		
		7. By cash with Treasurer ...		
		8. By cash in hand		
£		£		

________________ *Chairman.*

________________ *Clerk.*

No. VIII. STATEMENT OF ELECTRICITY GENERATED, SOLD, &c. (*see page 340*).

Major-General WEBBER: I did not expect, Sir, to be first called upon to make the few remarks which I propose to submit to the meeting. There are people whose *nom de plume* leaves us quite ignorant of their identity, and sometimes we wish to meet them in the flesh. So far as I am personally concerned, Mr. Hammond has this evening introduced me to "Chesterfield, "Junior." For many years I have wished to be face to face with that writer, and I take the opportunity of thanking him for the compilation of information that he has given us weekly in that excellent little newspaper called *Lightning*. I should like also to have the opportunity, if time permitted, of traversing a great many of the figures that are contained in the *Lightning* tables, much of which has been repeated in the paper before us this evening. As to their accuracy, I have little doubt that it is as great as that of many statistical tables with which the public is unofficially provided. But in this case, from the knowledge I have of the sources of information from which the tables in *Lightning* have been compiled, I must say they are not by any means of sufficient accuracy to give the engineer information from which he could draw deductions for his professional guidance. Maj.-Gen. Webber.

The want of explanation, or even analysis, of the tables—which, however, I hope Mr. Hammond will be able to afford in his reply—is emphasised, for example, on page 345, which gives the "coal consumption" of the metropolitan undertakings. We find that Charing Cross comes 50th in the order of merit of "coal," and (out of the 83 undertakings) 20th in its position of order of merit for "total costs." The Crystal Palace, curiously enough, is 82nd both in respect to "coal" and "total costs." Next we have Hampstead, 67th in "coal" and 45th in "total "costs;" Kensington, 29th and 31st; Notting Hill, 41st and 56th; St. James's, 24th and 19th; Westminster, 25th and 13th; and Woolwich, 77th and 72nd.

I submit that these divergences in order of merit suggest the complete *un*reliability such figures must have in enabling this meeting to form any opinion as to the causes for differences of efficiency in either respect.

Except in so far as the price of coal is given in each case—and

Maj.-Gen. Webber.

little can be learned from the statement that the coal used by Hampstead is 14s. 3d., and by St. James's 14s., and others in the metropolitan area 19s. a ton—this particular divergence seems to me to have no explanation in the paper. I think it shows the engineer that, however valuable, numerous, and however much an evidence of labour these tabular statements may be, they are nothing more than suggestive. Moreover, I think they can only be suggestive to the particular engineers who are responsible for the construction and running of those undertakings. Outside of them I do not believe that there is anyone who can, with their help alone, account scientifically to us for these divergences, or can read us any useful lesson on them for further guidance.

Perhaps in his answer Mr. Hammond will lead us to hope that he may be able to classify his information so that we shall be able to get some useful comparison, as to the important question, for instance, of coal consumption, by reducing the figures to percentages, which will be affected by the calorific value of the coal—a coefficient easily obtainable for the kinds of coal used in the metropolitan stations. There is also a question which arises, namely, the quality of the coal as to its visible smoke production. It occurs at once in respect to the prices reported to be paid by Hampstead, viz., 14s. 3d., and St. James's, viz., 14s. In both places we are bound to assume that nuisance to the neighbourhood of the generating stations is not permitted, any more than in the case of stations where best steam coal, costing 18s. or 19s. a ton, has to be used.

The paper no doubt will be discussed in respect to the hundred questions of this nature that arise, but I should like very much to have heard the subject of "unaccounted for losses" gone into, in the paper itself. When one comes to consider the great variety of causes for these, one cannot understand why no classification of those causes has been given. The fact is that it is one of the most interesting questions that presents itself to the electrical engineer. Now we know that, for instance, in one case, losses by resistance in distributing mains have been minimised by heavy preliminary expenditure in copper, rendered necessary by the system that has been followed for the con-

struction of those mains; and, again, in other undertakings, that, by the adoption of the use of storage on a large scale at its commencement, a preliminary expenditure of a totally different kind has been incurred in accumulators instead of in copper, which, while inducing losses under one head, lead to an improved "load-factor" during running hours under another. I know it is very difficult to get information from the responsible engineers on these points, but if this paper is regarded as a challenge for the production of really useful engineering statistics, and if they are forthcoming, then I believe this Institution will receive very great benefit. Maj.-Gen. Webber.

Mr. R. E. CROMPTON: I have to thank Mr. Hammond for writing this paper, as it relieves me from the duty of writing one myself. It may be in the recollection of members of this Institution that I have already contributed three papers on this subject at recurring intervals of about three years. So long ago as 1886 I commenced to prepare statistics from the working of the first large central station carried out by my firm—viz., that at Vienna—and the facts that I then observed gave rise to my paper on "Alternating *v.* Battery Transformers," read here in the year 1888. A large portion of that paper, and the discussion which followed it, turned on the question of costs. Three years later I read a paper wholly on costs before the Institution of Civil Engineers, and three years later a similar paper at this Institution. Mr. Hammond has, perhaps wisely, confined himself to the work of compiling from the mass of information which is now available certain statistics which cannot fail to be of immense value to all engineers who are studying the question of electrical supply. Mr. Hammond's facts I accept as substantially correct, and I can only thank him for the great pains and labour bestowed upon their compilation, and for the series of curves prepared therefrom. As a fellow-worker in this line I can testify to the amount of correspondence which such a compilation involves. My present remarks will be confined to criticising or enlarging on some of the deductions that the author has made from the information that he had before him. Mr. Crompton.

At first we engineers did not altogether appreciate the duties

Mr. Crompton. imposed upon us by the Board of Trade returns; but I think, now that we have had time to appreciate the great advantage which the publication of these returns has been to the whole body of engineers concerned, all must agree with Mr. Hammond in admitting that the publication of these returns has had the very happiest effects on the industry; and I think that we are indebted to Mr. Hammond, in his capacity of Chesterfield, Jun., for collating and putting together these statistics, as he has done for some years past in the columns of *Lightning*. From time to time I have found that suggestions have been made to Chesterfield, Jun., as to improvements or alterations in his tabular form, these all having the object of making the comparisons therein shown as far as possible, so as to make them as fair as possible; but, in spite of all that has been done, I am afraid that any effort to compile the statistics in the order of merit can only meet with disappointment, the conditions which prevail varying through such wide limits that the two points on which the information is most interesting—*i.e.*, first, that of the comparative merits of the systems used, and, second, the comparative figure of merit of the engineers in charge—cannot be obtained from them with any attempt at fairness. In my last paper I did attempt to make a comparison of coal and water used, and of the efficiency of the systems of distribution, but my attempts to make any such comparison were then rather severely criticised in the discussion. It is, however, quite certain that any engineer who studies this subject, and who wishes to make use of Mr. Hammond's or my own papers, must make calculations for himself. In order to make these calculations as simple as possible, I have in the past protested, and I still protest, against wasting our time in considering or discussing questions of cost over which we engineers can have no possible control. We engineers can control the design, the arrangement of the buildings, the style of boilers, steam pipes, generating machinery, switching machinery; we have the choice of using storage or doing without it; we have the wide choice of various systems of distribution, with or without transforming devices; we have the wide choice of plant in the generating stations and the very important

choice of materials for our distribution system; and we are anxious to know how our choice will affect the cost of production. Mr. Hammond in this paper, and the commercial man in general, always look first at the total cost; the commercial man will, after his attention be directed to it, possibly look at the works cost: in both cases the author's tables will be of great use to him; but neither of these figures are of real use to us, as they contain so many items which are so very variable in their nature as to be wholly misleading. Take, for instance, the cost of management—in one case by a highly paid board of directors, and in the second case by an electrical committee of a local authority which are not paid at all. Take, again, the instance of local taxation—in some cases a few hundredths of a penny per unit, and in other cases tën to twelve times this amount. The incidence of local taxation is now becoming so heavy on some of the companies—the assessment being at present carried out far too much on the lines of income tax, rather than on the lines of the magnitude of the works themselves—that in the case of some of the more successful companies the sum paid per unit for rates is already almost equal to the sum paid for coals; and if the present system is carried on, this item of rates will be the most important one which has to be considered when the possibilities of cheap electrical supply to a district are being discussed. The item which we engineers first look to as some criterion of the merit of the system and of the merit of the engineer in charge is the coal bill per unit delivered, as it *is* some gauge of the efficiency and suitability of the plant in general; and I would suggest to the author—or, rather, to Chesterfield, Jun.—that he should encourage by all means in his power some means by which this gauge could be made more useful to us. My own view is that works engineers should, wherever possible, insist upon the use of satisfactory water meters, and should as far as possible keep account of the water actually pumped into their boilers for steam generating purposes. This entails the use of one meter for the boiler supply, and another meter for water used for wash-out, sanitary, or other purposes. When the water is known it is an easy matter to observe, and consequently to work at the improvement of, the evaporative

Mr. Crompton.

Mr. Crompton. power of the boiler plant. With the water known it is also an easy matter to compare the efficiency of the steam plant per unit generated; and I think that, as soon as we get any extended list of the water consumption per unit of supply stations, it will give a great impetus to the wholesome competition which has already done so much to lower costs.

Although in most cases, as I have said, the arrangement of the cost of various works in order of merit affords little or no information, yet there are cases where comparisons can be made, and made satisfactorily. I allude to the London supply stations. The works engineers of these supply stations, whether they be under the control of companies or under local authorities, are fairly on a level as regards the cost of the fuel they employ. Of course those stations situated near the river enjoy the advantages of cheaper fuel, and of having condensing water available; but those away from the river are under fairly equal conditions; and I think the study of Mr. Hammond's statistics makes it quite clear to everyone that the figures of certain works can be very closely and fairly compared. I allude to those of the St. James's Company, the Westminster Company, Kensington Company, Notting Hill Company, and the Charing Cross Company—the last company to a less degree, perhaps, because the recent additions to their plant is on a new system, which must modify the results; but with this exception the other companies work practically on the same system, and it is curious to observe how very close the figures are. In the matter of the coal bill, Mr. Hammond shows that the St. James's Company, with an output of 2,500,000, used 6·4 lbs. of coal at a cost of 0·5d. per unit; the Westminster Company, with 3,500,000 output, used 6·5 lbs., at a cost of 0·53d.; the Kensington Company, with an output of 1,500,000, used 6·6 lbs., at a cost of 0·67d.; and the Notting Hill Company, with an output of only 230,000, used 7 lbs., at a cost of 0·71d. From actual knowledge, I should say that, if in each case the lbs. of coal were multiplied by 8½, it would be found to represent the actual lbs. of water used. I do not think, however, that these coal figures are representative. In hardly any of these companies has the condensing plant been got properly to work, or it has only

been used on a small scale; but, quite apart from condensing plant, some of the works have got down to very much lower figures, notably in the October and November months. Careful trials at one of the Kensington stations, made some years ago, showed that it was quite possible to keep the figure down to 3½ lbs. of coal per unit during the full-load hours, and for the average of the corresponding month the average figure was only increased to 4 lbs. in this same station. In spite of what Mr. Hammond has said about my having fixed the possible coal figure at 2½ lbs. per unit, I am still hopeful that this figure will be closely approached; as, if without condensing we have already been able to do 4 lbs., with better loading and with the aid of condensing plant, and with better appreciation of means for preventing the stand-by losses, we ought surely to get down to 3 lbs. at no distant date. Mr. Crompton.

I will now come to a very important part of the subject, and that is the reliability of the plant. This may be divided into two heads—first, that of the generating plant, and, second, that of the mains.

As regards the former, I have over and over again lifted up my voice against the single-station system, as, however much the plant in the single station may be subdivided and separated off so as to prevent an accident in one part affecting the working of the plant in other parts of the station, so long as a large system is supplied from one station there is always the risk of a total extinction of the lights.

Quite recently I have had experience of a new danger wherever high-pressure generating plant is used; any rupture of the steam pipes, or of parts of the steam engines, by which large quantities of steam are allowed to escape into the station, is liable to short-circuit the coils of the high-pressure generators, situated, as these are, very close to the steam engine. This is a real danger to be guarded against in future designs. One way of dealing with the matter is to adopt a system of generating at a pressure not exceeding 500 volts, up to which limit apparently there is no difficulty in insulating and waterproofing the coils so as to resist for long periods of time the short-circuiting effects of an atmosphere charged with steam.

Mr. Crompton.

Another method would be to interpose a partition extending down the stations and dividing the dynamo portion from the steam engine portion of each generator. This, however, presents considerable difficulties. Another method of securing reliability is that every station should have two chimneys. Accidents are liable to happen to the chimney, or to the plant connected therewith; and, although these cases have been hitherto comparatively rare in this country, they have not been so rare in America, and serious losses have been incurred therefrom owing to the interruption of supply.

As regards the reliability of the mains and distributing plant generally, a great deal might be said; in fact, it is such a large subject that it merits a paper to itself. It is quite certain, however, that, if the workman can handle the live mains with impunity, many sources of breakdown can be avoided. In connection also with this matter the handiness and convenience of the systems of joint boxes, and of dealing with the jointing, connecting, and disconnecting of the various classes of concentric cables, has a great effect on this question of reliability.

I do not quite understand Mr. Hammond's new load-factor proposal. As I first used this term in my paper at the Civil Engineers, I am at liberty to say that in the form I first used it I feel that it is somewhat difficult to explain, and consequently has disadvantages. I therefore find it useful to employ a simpler term. If I, an engineer, put down a supply plant of a certain size measured in kilowatts of output, and I wished to indicate what will be the income which can be derived from it under certain conditions of working, instead of saying that the plant will be worked at a certain load-factor, I find it convenient to say that its output in kilowatts per hour must be multiplied by a certain number of hours in order to yield the output per annum in Board of Trade units. For instance, the plant which, apart from reserves, can be worked at a maximum of 1,000 kilowatts, in one town might yield 1,000,000 Board of Trade units per annum—that is, the same as if the plant was worked at the full output for 1,000 hours—and I should call this town a 1,000-hour town. I find that the residential part of London served by the Westminster, Kensington, Notting Hill, House-to-House, and Chelsea Companies

very closely approaches this 1,000-hour figure; of course with the St. James's and Charing Cross Companies the figure is higher. Mr. Crompton.

I will conclude my remarks by agreeing heartily with Mr. Hammond in saying that, after all, the most important factor in low working costs is the "engineer factor"—in other words, the costs depend to a remarkable extent on the man—and so far as this publication of accounts, and the compilations in *Lightning* and other papers, tend to foster the sporting instincts of the English engineer, they are highly beneficial. It is on this account that I hope that Chesterfield, Jun., will see his way to simplifying his tabular statements so as to make them readily understood by the engineer, and so that they will bring the merits of the engineer more conspicuously to the front than has been the case up to the present time. I think we may congratulate ourselves that up to the present time the publication has certainly had a wholesome effect, and that the costs of the supply stations of the United Kingdom compare very favourably with Continental or American ones. Furthermore, it is highly desirable that the members of electric lighting committees or boards of directors should be able to learn from these statistics how largely their financial results are affected by the skill and energy of their engineers, and how well it pays them to encourage this by treating them liberally, both as regards their position and their salaries.

Mr. WORDINGHAM: I do not feel that I am at all entitled to come on at this early stage in the discussion, and I will make my remarks very brief indeed. I must concur in what has already been said about the value of this paper. The labour must have been enormous, and we require no better indication of the care with which it has been prepared than is to be found on page 323, where Mr. Hammond has not forgotten that last year was leap year. I think that shows the care he has taken in the whole thing. It is rather a mistake, in discussing the cost of supply, wholly to leave out of sight the capital cost. This paper only takes account of the running charges, and does not refer to the charges incurred by the large capital outlay. Those charges are very heavy—they often amount to half the total Mr. Wordingham.

Mr. Wordingham. cost of production—and I think that the term "total cost," as used in the paper, is by no means a happy one. The load-factor in the case of that portion of the cost is everything practically, because the higher the load-factor the greater the number of units over which the capital costs are distributed. It is not necessary for me to enlarge on that point, but I think attention should be called to it. Then there is a point which reminds one rather of those superimposed photographs which were in vogue some little time ago. I refer to page 305, where the lowest cost for all the different items is taken; I think that is very misleading indeed. For instance, to take the case of Whitehaven as a sample. The cost of management there is extraordinarily low, and that is because it is combined with a sewage-pumping station; and it is very difficult to say how much has been put down to the electric lighting, and how much to sewage-pumping. One cannot help thinking that the electric lighting has been rather mercifully treated. Reference has been made to the good effect of this keen competition in works cost. There is no doubt that it has been invaluable, but I do think that there is a danger of its being carried too far. There is a temptation to sacrifice everything, as it were, to obtaining low costs. In certain cases it may be sounder really to let the works costs for a given year appear higher than they might legitimately be made, in order to have the undertaking on a perfectly sound basis. Supposing, for instance, very heavy repairs are required, rather than draw upon the reserve fund, I think it is really better to charge the whole thing to revenue. One incidental advantage of that is that you do not make so much profit, and you have not such a heavy income tax—that is a thing one has to consider. Then as regards a quotation, on page 268, from the *Engineer*; of course, as Mr. Hammond says, that is perfectly unjust. The price has been enormously reduced. The Manchester figures are given in the paper, so that it is no use my detailing them. Special reference is made to the very low price charged at Brighton. I took occasion to work out for different hours of consumption how the Brighton charges compared with the Manchester charges, and, curiously enough, they agreed, except in

the second place of decimals; so that they are practically identical, except that the maximum charge at Brighton is 7d., as against 5d. at Manchester. I do not think, however, in Brighton they supply motive power at 1½d. a unit, as we do in Manchester. So far as I know, the lowest charge made anywhere to ordinary private consumers is in Manchester, where we charge 1¼d. per unit for 4 horse-power, used not less than 48 hours per week. As regards the question of units generated and units used, my own practice has been to give the units sold, the units used on the works, and the difference between that and the units generated as the units unaccounted for. I think that is the best way to put it. As regards the question of depreciation, I certainly think that all undertakings, whether of companies or local authorities, ought to put by an amount for depreciation. We have done that all through in Manchester, and we now have a very respectable reserve fund, amounting to some £27,000. I am not quite clear from the terms of the Provisional Order whether, when that reaches 10 per cent. of the total capital, we can take it, or whether we shall have to give it to public purposes. I am not quite sure whether it can be applied in reduction of our own capital, or whether it must be handed over for a park, or something of that kind. If the latter, I do not feel so strongly about depreciation. Mr. Wordingham.

There is one point, which is mentioned on page 364, as to the necessity for plant of larger size. Manchester is referred to as being a station in which two 1,500-kilowatt machines are on order. That is perfectly true. I should personally feel very much obliged to any manufacturers here who would give one an idea of what is the upper limit for the size of unit. Certainly a unit of 1,500 kilowatts in some stations is not going to be very large.

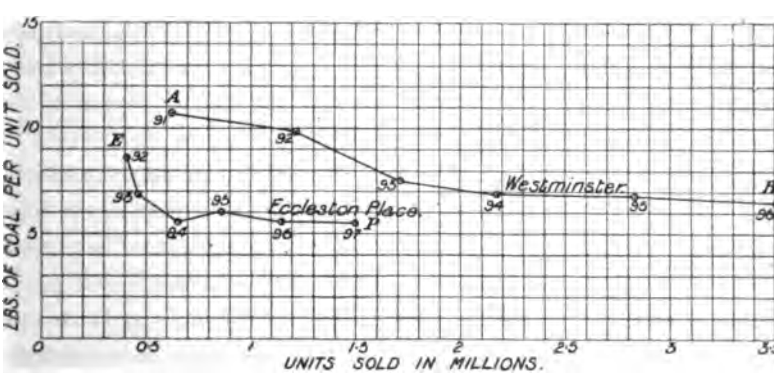
Professor FORBES: I will only make a very few short general remarks, Sir. These curves naturally have very great interest to us all, and the labour that has been bestowed upon them cannot be over-praised. At the same time, what I wish to say will perhaps prevent something being said frequently in the course of the discussion, and that is, that I am afraid each one of us will see there is something in these curves that we want to have shown Prof. Forbes

Prof. Forbes. by them that is not shown by them, and will find a fault with the curves for that reason. Of course that is wrong, and one cannot expect, however much time Mr. Hammond may have devoted to the thing, that he should be able to give us a set of curves which would supply all the facts we really do want. To take an example, General Webber has told us that from an engineering point of view there is not very much information to be gained from these curves. Mr. Crompton has told us that, looking at these curves, we cannot judge very well of the relative merits of the different stations on certain points. These are two points which they thought the curves were deficient in. I daresay the things I should like to see brought out in these curves, too, are not exactly there; but I wish to point out that, however each one of us may wish that some particular point was brought out in the curves, I think every one of us in the room will unhesitatingly say that this is the greatest effort that has been made in the way of placing such curves before electrical engineers. I think Mr. Hammond deserves all the thanks that we, I am sure, are willing to give him for having put before us a set of curves which, though they may not bring out every point clearly, have given us a basis upon which to go, and enable each one of us to work out our own line and the facts which we wish to find.

Mr. Hammond has drawn two conclusions from these various tables of total cost and so forth. He has drawn first the conclusion that as we travel from the left to the right of the diagram—that is to say, from small consumption to high consumption—the cost per unit diminishes. He has also drawn the conclusion that as we go from left to right of the diagram—that is, as we go from the early years of the station until the later period of its development — the cost of the units has been diminished. That is to say, the two things are interwoven; and he has actually made use of these curves to say simultaneously that the cost per unit has diminished with the advance of years, and also with the increase in the number of units. He is perfectly right; there is no doubt about it; but the curves do not differentiate the two things. For example, we may take such a case as where a station has gone from the two-wire

system to the three-wire system, or where improvements or economies have been introduced, such as putting 200-volt lamps on to what was previously a 100-volt circuit. All these improvements that have been gradually introduced are not shown, and we have no means of seeing how far these have acted upon the improvement in the cost per unit. I merely throw that out as one of the things which, personally, I should like to see worked out, but I think we ought not to waste time in the discussion by pointing out these little things which do not explain things which we would wish to explain. I do feel that the information which is contained in this paper will enable each one of us, whatever the line our inquiries may take, to get at the facts more fully than we were able to do before the paper had been produced and laid before us. I only wish, in conclusion, to endorse what the other speakers have said, and what, I am sure, everyone who has worked in this direction himself must feel—that Mr. Hammond's contribution here is the greatest step towards getting at the statistical facts, and enabling us to arrive at sound conclusions ourselves. Prof. Forbes.

Mr. C. O. Grimshaw: On page 363, the author mentions 6·4 lbs. of coal per unit sold, as probably the best to be expected. I have put a diagram on the wall which shows rather better results. Mr. Grimshaw.



The line A B is the lbs. of coal per unit sold for several years past of the Westminster Company. This is about 6·5 lbs. This company, as most of the members present know, is composed of

Prof. Smith. outlay is a fundamentally imperfect one. Of course the whole of the initial outlay cannot be considered as engineering expenditure. It is in all cases very difficult to separate that part of the whole prime cost which can fairly be dealt with as real engineering outlay from the rest; but, with regard to the *bonâ fide* engineering part of it, it is a universal law, not only with regard to electric lighting but with regard to all engineering work, that by wise and skilful increase of outlay, up to a pretty high limit, you can reduce working costs; and therefore it is always a question of policy as to how far you should increase initial outlay in order to get more economical and more thoroughly durable machinery and plant. The curves of increase of interest and depreciation on the one side, and of decrease of annual working costs on the other side, intersect at a certain place where you find the happy medium —the most politic quantities to adopt. This question of interest upon preliminary cost—of interest and depreciation beyond maintenance and repair—is one that essentially influences the proper estimation of working cost.

The PRESIDENT: The discussion will be adjourned to the next meeting. I have pleasure in announcing that the scrutineers report the following candidates to have been duly elected:—

Member:

Harold Thomson Lyon.

Associates:

Carl Augustus Astrom.
Harvey Cranmer Buchanan.
William Davis.
Douglas Gordon.
George William Spencer Hawes.
Moritz A. Immisch.
Otto Claude Immisch.
Frederic Arthur Knight.
Ernest James Marsh.
Thomas Henry Pope.
Percy Richard Rice.
Cyril M. Shaw.
Julius Leonard Fox Vogel.
Robert Wardell.

Students:

John Henry Johnson.
Ayton H. Read.
Reginald Savory.
Eustace Graham Sheppard.
Arthur Woolmore Wigram.

JOURNAL

OF THE

Institution of Electrical Engineers.

Founded 1871. *Incorporated* 1883.

VOL. XXVII. 1898. No. 135.

The Three Hundred and Thirteenth Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, April 21st, 1898—Mr. J. W. SWAN, F.R.S., President, in the Chair.

The minutes of the Ordinary General Meeting held on March 24th, 1898, were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council:—

From the class of Associates to that of Members—

Arthur Henry Preece.	James Edmund Edgcome.

From the class of Students to that of Associates—

Arthur James Abraham.	William Reginald Potter.
Frederic William Close.	Ernest Noblett Ruddock.
W. P. L. Harrison.	Ralph Rushton.
Alick James Newport Kennett.	R. M. Sayers.
Alfred Milligan.	E. C. Short.

Mr. E. K. Scott and Mr. F. Crawter were appointed scrutineers of the ballot for new members.

Donations to the Library were announced as having been received since the last meeting from Mr. A. A. Campbell Swinton, Member, and Messrs. Whittaker, to whom the thanks of the meeting were unanimously accorded.

The President.

The PRESIDENT: We will now proceed to resume the discussion adjourned from the last meeting. I am sorry to say that, instead of Mr. Preece being present, as I had hoped he would be, he has sent me this letter:—"I am very sorry indeed that I cannot "attend the meeting to-night, for I wanted particularly to thank "Mr. Hammond for the excellent work that he has been doing for "so many years in the columns of *Lightning*. I have never "lost one opportunity of expressing my great obligation to those "figures for assistance in many an awkward moment.—W. H. "PREECE."

The subject we have before us is one of such interest that it has already brought forward notice of the desire of a good number of members of their wish to take part in the discussion to-night; and, as this is the only night we can possibly devote to this subject, it will be necessary, in order that each may have an opportunity of stating his views, that the speakers should make their speeches short. I notice that Dr. Kennedy is present. I should be very glad to hear that he has something to say on the subject.

Prof. Kennedy.

Professor A. B. W. KENNEDY: I have read Mr. Crompton's remarks in this discussion, and, as to the value of Mr. Hammond's work, I will only say that I most cordially endorse what Mr. Crompton said. The only thing further on this head which I will add is that I think that, although figures will not show us everything, as some speakers observe, yet, to those of us who are in the habit of dealing with this question every day, I am sure that these figures do tell a very great deal, and are easily capable of being interpreted with considerable accuracy, and, consequently, with very great benefit. I am only one of the great number who feel much indebted, and frequently indebted, to Mr. Hammond for the care and the fairness with which he has dealt with these matters.

Going, without further preface, to the paper itself, there are, of course, an immense number of things that might be said, but I will try and confine myself to the few principal ones which have occurred to me. I am glad to see, in the first place, that Mr. Hammond does not encourage the rather favourite idea that you may take the most favourable points out of half a dozen places and lump them all together and say that is what ought to be done. How it is that fuel and oil, repairs, rent, and so on, are all mysteriously correlated I do not know, but it does appear that there is some kind of correlation between them which makes it impossible to get at the same moment, and in the same place, the very best results from them all. If there is one thing which the results of Mr. Hammond's paper tell us more distinctly than any other, it is that the efficient working of an electric lighting station is a problem of enormous complexity,—one which cannot possibly be dealt with by considering any one special point. He has shown us that different systems astoundingly widely apart, using different kinds of machinery, and different kinds of mains, may all under certain circumstances give us more or less the same result. He has, I hope, dealt for ever a final blow to that most disagreeable and uncomfortable type of person who thought he had discovered the one and only way by which an electric lighting station could be carried on, and that any other way was absolutely wrong. That person was much in evidence a few years ago. If he is not now dead and buried, he had better read Mr. Hammond's paper, and that, no doubt, will effectually end him. Prof. Kennedy.

Among other things, Mr. Hammond, in common with many other engineers, has given very great importance to the question of "load-factor." He says he places it second only to output. Of course it would be foolish to deny, with these curves in front of one, that the question of load-factor is a very important one. But I am not disposed myself to put it quite so high as Mr. Hammond does. I think his own figures thoroughly justify my position. Of course, if it were some question of getting, instead of the well-known peak, a nice comfortable rectangular load curve, we should know where we are; but we need not think of this. We are not going to get the rectangle in the

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time of any of us in this room, and we must just face the fact that we have to make the best of a peak, and to do what we can to reduce it. Even if we did get the rectangle, then in summer we should still at best have only a rectangle perhaps a quarter the height of the winter diagram. So that I think the uniformly rectangular ideal may as well be given up as a thing that is not much worth while talking about.

A very remarkable point about load-factor comes out in Table XII. Take the two Newcastle companies, for example—the Newcastle District and the Newcastle-on-Tyne—and you will see that one has a load-factor of 27 per cent., and the other of 17 per cent.; and yet, as everybody knows, the costs of the two companies are practically identical. Indeed, as a matter of fact, I see that the coal bill is very much higher per unit for the company which has the larger load-factor. There are other cases almost as striking. If you compare, for instance, the Westminster Company and the Charing Cross Company, you will see that the latter has 26 per cent. load-factor, and the former has 15 per cent., or not much more than half as much; and yet the costs do not differ so very much. The Charing Cross happens to be a little higher, but that may well be no more than corresponds to the greater output of the Westminster Company; practically the costs are of much the same order. There is nothing to indicate that the enormous difference in load-factor has any very great effect on the result, or, at any rate, any effect proportional to what one would expect at first. If we take Diagram II., we find the same thing to be shown very clearly. The points are so dotted about that they could be equally well represented by a horizontal line, a vertical line, or a line running up from right to left, or up from left to right; they show simply that the effect of load-factor—important and great as it must be—is absolutely masked by some other things,—so absolutely masked that you can see not even a trace of its effect. Diagram III. shows the same thing in another very similar case. That is the diagram which connects the units sold per lamp per annum with the works costs. There again there is absolutely no relation shown between the two, which means simply that the whole

problem of the supply of electrical energy is so complicated that even such important things as load-factor and the units per lamp per annum do not affect the matter sufficiently to leave their mark on diagrams as complete and exhaustive as those which Mr. Hammond has placed before us. Prof. Kennedy.

These matters have struck me very much in reading Mr. Hammond's paper, and if he had done nothing else than compel attention to them (he has, in fact, done a great deal more, of course), we should have been amply repaid.

There follows what he calls the "engineer-factor," and I rather agree with Mr. Crompton that this factor is a very big factor indeed. Unfortunately, it cannot be put in a diagram! I am sure that all of us who have to do with the practical running of stations have more and more reason to recognise this matter.

The author deals with the question of a form, for which I am more or less responsible, showing the quantity of electricity generated, sold, &c., subdivided in a certain fashion. I do not suggest that the form I use is better than any other, but I do think that if engineers in general used that form, either in private or for publication, they would find it would help them a great deal. You have a certain amount of energy sold to consumers which may be anything from 50 per cent. to 85 per cent. The whereabouts of the remaining 50 or 15 per cent. ought really to be well known to the engineer in charge of the station. He ought to know whether it goes on the works, whether it goes in batteries or in mains—his mains include his transformers, of course—and so forth; and when he has added up all his known quantities, they ought to balance with the quantity he believes he has generated. Questions as to exact nomenclature are not worth discussing here, but I am quite sure that Mr. Hammond would agree with me that it is worth while that the engineer should at least know these things. As my figures in these matters have been published, I may say that we do, at Westminster, succeed in making the quantities add up within from 1½ per cent. to 2 per cent. of the whole, which is not more than corresponds to the reasonable or unreasonable amount of errors of meters all round, want of correspondence between station meters and consumers'

Prof. Kennedy. meters, and so forth. I believe that it is very much worth our while to make sure of this check; and I believe anybody who does not know where 98 per cent. of his units goes will certainly find it much to his advantage to make the discovery.

There is one more thing I should like to call attention to, although, perhaps, it is rather a personal matter. It is with regard to Diagram IX. (*a*), which shows the rates. There are many items which decrease per unit as the number of units increase. I regret to say this particular item shows an uncomfortable tendency to increase. If you look at the curves of the Westminster Company, the Kensington Company, and the Leeds Company, you will see they all go up as the output increases. I do not make any remark about that, but it is rather hard on some of us; although, I daresay, the parishes in which we carry out our work like it very well.

Mr. Raworth. Mr. J. S. RAWORTH: I shall describe this paper of Mr. Hammond's as a magnificent culmination, but I hope not a conclusion, of his continuous labours on our behalf. We have all of us enjoyed the fruit of those labours, and to-night we get them summed up in a manner which will be of great use to everyone in the profession. I want personally to thank Mr. Hammond for taking up the cudgels on my behalf in connection with the onslaught which the *Engineer* made upon my proposition for supplying power from external sources to cities which are very much burdened with the smoke nuisance. I sketched a picture and gave figures which they derided; but they derided them too much—in fact, they went so far that I did not think it was worth while to go into an elaborate defence of my position. I thank Mr. Hammond very much for having taken up the matter, and for having made it clear to the editor—and I hope to his readers also—that he was wrong this time.

I have waded through many columns of Mr. Hammond's figures, and have tried to glean some comfort out of them, or a better understanding of some matters which have not always been quite clear. There is no doubt, as Dr. Kennedy has just said, that they do bring out this fact very strongly—that the personal element tells almost more than anything else. Take that

peculiarly elastic item called oil, waste, water, and stores. We find that Mr. Mountain, of Huddersfield, in 1894 broke all records (I do not think he will ever get anything better in his life) by coming down to the magnificent figure of 2-100ths of a penny for all those items. The most curious thing is that Mr. Mountain has under his care a lot of engines which I am not prepared to say for a moment are not properly credited with being wasteful engines in oil. They are open-fronted engines, and do throw the oil about, but in spite of that he has managed to get his oil and sundries bill down to that very remarkably low figure; and, although he has had certain changes in his plant which have caused him to use more oil, still he keeps at 5-100ths, which is among the remarkable records. Mr. Raworth.

Further on you will find there are six stations, all of them spending equal to or over 40-100ths of a penny in those matters—that is to say, 20 times the amount that Mr. Mountain was successful in using at Huddersfield. The next curious thing about it is that, if we turn back to the coal account, we find there are nine stations running which spend less on coal than these last six spend on oil and waste. They probably burn the oil and waste with a view of keeping the coal bill down. There are a great manny funny things in these tables, but I am not going to occupy your time by taking you all through them; but when you come to look at the question of staff—that is, the amount that is spent in management expenses—you find that London companies are probably much more liberal towards their *employés* than municipalities and others: that is to say, they do not carry the cutting down of salaries so far. Take the City of London Company, for instance. They began in 1894 with a small output, and spent 30-100ths of a penny on management. The next year they spent 54-100ths of a penny on management, and the year after that they spent 96-100ths on management. It shows you the great advantage of working for a company. Your salaries will probably rise much faster than they would under other circumstances. There is one thing in connection with the City of London Company which I should like to point out. They drop in for a great deal of the ink-pot, but they are one of those companies which will never be

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able to show any particular record figures, and for this reason: theirs is a daylight load practically, and as every cloud passes across the sky their load rises and falls, following the passage of the cloud; and if it happens to set in for a dark morning very suddenly, the load rises so rapidly that they would be utterly incapable of dealing with these conditions unless they kept a large staff at hand, and a large number of boilers under steam. I do not think that point has ever been taken sufficient account of before, because I believe the position is unique, and it will absolutely prevent them from making the record figures of some other companies. That brings me on one step—to the question of load-factor, which Dr. Kennedy has spoken very nicely upon; that is, that the moment a man becomes financially interested in an electric lighting company he never can be happy when the sun shines. The directors of the City of London Company perfectly gloat over a fog, and I can assure you if they had their own way the sun would never rise at all. Then they would get that magnificent parallelogram which Dr. Kennedy has just spoken of. Although Dr. Kennedy may be quite right in saying that the works costs are not improved very much by a certain increase in the load-factor, yet it must be obvious to everybody that the other item of expense—that is, interest and depreciation of plant—must come down almost as the load-factor rises; that is why it is, in dealing with large schemes such as this which the *Engineer* criticised, you are able to get the total cost of the unit down to a very small figure indeed. A gentleman who had seen the figures which I quoted at Manchester sent me word a few days ago that in a larger plant he was interested in—I think at Widnes or St. Helens—where they are using current day and night, the total cost of production—I do not think he included interest, but the figures are very interesting under any circumstances—was 0·28 of a penny per unit. I think that is a sufficiently striking fact to show that, if the increase of the load-factor be only large enough, it will, at all events, have a very strongly marked effect upon the commercial result.

Mr.

Mr. J. N. Shoolbred: There are so many points raised, and so much information given in this paper, that I think it is only

fair that one should confine oneself to a few points. The question to which I wish particularly to draw attention is the use of storage batteries in connection with a direct-current supply at low pressure, and the consequent effect thereof on the cost of generation and distribution. It is, I think, generally admitted that a judicious use of such batteries can effect a very important bearing on the financial economy of the working generating expenses of the supply; quite apart from the question of regularity of distribution, and also that of providing a reserve of power available to be drawn upon, either intentionally, or in case of accident. A town supply of electrical energy ought not, in my opinion, to differ in the matter of a reserve of power from other town supplies; such as the service reservoir with a water supply, or a gasometer with a gas one. Our friends the gas suppliers would never dream of distributing direct from the retorts, and without the intervention of a gasometer; as practically is the case with the somewhat analogous electric distribution from a high-pressure alternating-current supply. Moreover, the gas advocates hold distinctly that, until electrical storage is used, practically, to provide a reserve of power, they do not fear the financial competition of electricity. Be that as it may, it appears to me, from my own personal experience in central station supply, and extending over several years, that the use of the storage battery has a most important bearing upon the question of costs—of generation especially—and in a way that this paper, teeming as it is with valuable facts, does not draw attention to. As the question is one of interest to the meeting, I have brought for inspection several diagrams of daily ampere-output curves. From the Bradford Corporation Supply—with which I was connected in its early years—two diagrams in 1891—July 25th and December 23rd—and two in 1892—July 25th and December 23rd,—practically the maximum and the minimum daily output of each year; and from the Birkenhead Corporation Supply three output curves—one for December 31st, 1896, and in 1897 those for June 24th and December 31st. In each and all of these diagrams there is shown very distinctly the part which has been played during the 24 hours by the generating Mr. Shoolbred.

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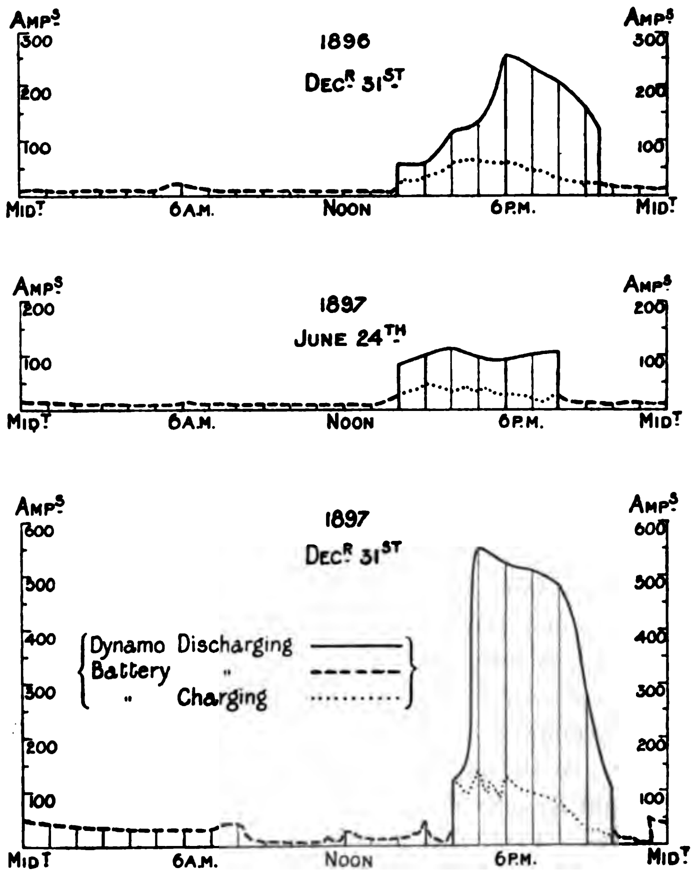
plant and by the storage battery; both while being charged, and while providing the supply to the town. The December curves of the Bradford Supply are the ones which contain that peak which excited the ire of Dr. Kennedy in his remarks.



Birkenhead Corporation Electricity Supply—Daily Output Ampere-Curves.

Unfortunately, this was the case in the early days referred to. I can only hope Mr. Gibbings, who now successfully pilots that supply of electrical energy, will be able to tell us that those excessive peaks in the daily curves no longer exist. The motor trade, which even in the early days was beginning to make itself felt, as a day load, will have a tendency soothing, no doubt, to Dr.

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Kennedy's feelings. In each of the diagrams for these direct-current low-pressure stations, the dynamo work would practically represent the load-factor for that day—with its limited number of hours; while, for a high-pressure alternating-current station, it might be almost represented by the entire supply curve extending over the whole 24 hours; as the loss in the overcharge to the storage battery would probably compensate for that due to the transformers. But though the load-factor would apparently be the same with the direct as with the alternating current, yet the actual cost, both in coal consumption, and, more especially, in wages, would be considerably less with the direct (when battery-aided) than with the alternating current. In the four Bradford curves, the load-factor resulting from the use of the generating plant sufficed to provide a supply to the town, in each case for 24 hours. But, in the case of Birkenhead—in its early stages, at least—when the demand was but small, as illustrated by the curves for December 31st, 1896, and for June 24th, 1897, the load-factor resulting from the use of the generating plant for about 7 hours, sufficed for a supply to the town for 72 hours (three days). The economy resulting therefrom has been very notable, owing to the fact that the steam engines were working almost always near to their full load, while the period during which the men were employed occurred at ordinary working hours, and did not run into the small hours of the night. In fact, the generating load was, thanks to the storage batteries, partially converted into a day load. Thus the distribution of the electric supply at Birkenhead during the first 12 months was practically entirely entrusted to the storage batteries, and effected at a mere nominal cost; while, also, in the generating expenses, the staff bill was kept low, and the coal bill showed the remarkably low figure (for a new installation) of from ¾d. to 1d. per unit generated.

It is not often that the opportunity is afforded of using storage batteries from the very commencement of a town supply, as was the case at Birkenhead. Therefore have I brought this example under the notice of the meeting, as illustrative of the considerable economy in working expenses arrived at by a judicious use of the

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storage battery in a comparatively small station, at present at least; and in its initial stages of supply and demand.

Doubtless, in a large and well-established central station, with a considerable output, the economical advantages are not so marked as in the cases just referred to. It is easy, therefore, to understand why storage batteries, in some stations in London, have not proved to be a success. But the success, or otherwise—financially, at least—in any central station, depends very largely upon the careful daily attention which the cells receive; as well as upon pains taken during the period of charging the battery. If these precautions be neglected, the upkeep of the storage battery becomes very large, and its life is considerably shortened. Here, in fact, comes in, in a very marked degree, the personal element of what has been termed "the engineer-factor."

I have already referred to the advantage, in the matter of town distribution of electrical energy, that a storage battery affords in the reserve supply thereby provided, to be drawn upon if necessary. This I may illustrate by describing an incident which occurred in February, 1892, at Bradford, when the supply was already a pretty large one. I happened to be myself at the time in the Town Hall, sitting under the electric illumination there, when information came from the works that the large 1,200-ampere dynamo-fuse, connected with a 300-H.P. ind. steam engine, had given out about half an hour previously. Although the above fuse was carrying 45 per cent. of the total current-output at the time, yet so instantaneous was the automatic action of the storage battery in the circuit, together with that of one or two small dynamos then running, that there was not so much as a flicker observable in the town. The chairman of the Electric Lighting Committee, and others, who were sitting at the time under the electric light at the Town Hall, would not believe me, except on the production of the broken fuse, on the next morning. What, may I ask, would have been the result in an alternating-current installation of the sudden abstraction of 45 per cent. of the supply?

A good deal of discussion has this evening turned upon the interpretation of the word "load-factor;" and several of the

speakers—Dr. Kennedy amongst others—have pointed out that there are other matters which should also be taken into consideration in the exact definition of "load-factor." I trust, from the examples which I have brought forward of the use of storage batteries, that the judicious employment of these adjuncts to the generating plant of a low-pressure "direct" central station, may tend to show the influence which they may have upon the "load-factor," especially in its financial aspects. Mr. Shoolbred.

Another matter, which has been admitted by several speakers as having a most important bearing upon the economical working of a central station, is the judicious selection of the size of the individual units of the mechanical generating plant. This, of course, must vary with the particular locality, and these sizes must be made to conform to the increasing demands which are sure to arise before long. Hence a variety of sizes of steam engines would seem necessary in most stations, in order to ensure each of these engines being always made use of at, or near to, its "full-load" capacity.

Here again the economic value of the storage battery makes itself felt, not merely by taking upon itself any supply which falls short in amount of the economical working load of the smallest type of steam engine, but also by again filling in the excess load-gap which occurs between the full load of the first engine put on and the lower economic limit of the next engine to be put on; and so on, but with an increasing, as with a falling demand. Thus conducing much to economy in the use of the generating plant.

Of the many other matters which bear materially upon the cost of generation of electrical energy, there are few which offer more scope for attention than the arrangement of the pipework, for the live-, as also for the exhaust, steam; and for the water supply. A careful consideration of the various parts is often not accorded to this very important department. Yet here it is, as has been pointed out by several speakers, that occur, and nearly continuously too, many of these small "unaccounted losses" whose aggregate forms a very important part in the economy of a central station.

Mr. Shoolbred. In conclusion, I must congratulate the author of this very valuable paper; not merely on the large mass of information, all bearing most directly on the subject, which he has brought before the members of the Institution, but also on his own appreciation (recent though it be) of the value of the "direct" current, at low pressure.

Mr. Patchell. Mr. W. H. PATCHELL: I hope I may be in order, as it touches the paper, to say how pleased I am personally—and I think a great many more of us—that at last we have got the authorised version of the minutes in our hands at the time the paper is being discussed. I received my copy yesterday morning, and it is the first time it has ever happened. I think we ought to pass a vote of thanks to the Secretary and the Publishing Committee. Dr. Kennedy and Mr. Raworth have already mentioned the salaries. I quite agree with the author that the salaries of engineers ought to go into Generation; under the present circumstances an analysis with salaries included in Generation masks everything. The statement of Mr. Wright that the Brighton "charge is 1½d. a unit "for all consumption beyond an average," &c., has been a most unhappy one. I quite agree with him in principle, but it is upside down. People read that "the charge for electricity is "1½d. a unit for all consumption," stop there, and then write hysterical letters to the papers, and think everybody ought to have electricity for 1½d. a unit. It has cost some of us considerable waste of time with some of our consumers. I do not like to call the "aggregation of the lowest costs in the United Kingdom" (page 305) "crazy patchwork," but really it is very much like it. We cannot put in our thumb and pull out a plum quite in that fashion. I have looked up one or two of them. Mr. Wordingham on the last occasion threw a little light on Whitehaven and one or two other places. Why should Newport get such exceedingly light repairs? On looking at the details for Newport—it is their second year of working—I find that they have Lancashire boilers and slow-speed engines, and that their capacity is 380 kilowatts, while the maximum load is 190; so that, having only had half load, they ought to have practically wanted no repairs at all. I hope the author will be able to give us the cost per ton of fuel in Leeds,

Mr. Patchell.

because when a man makes a record he ought to show all his hand. Mr. Hammond rather gave himself away on the last occasion by telling us he had the Leeds figures on his table every Monday morning, so he really cannot refuse it. I have not looked whether they have been corrected in the copy of the Proceedings, but I think some of the figures in the last column of Table XIV. are wrong. In Bristol, the figure 12·90 ought to be 12·99; the Westminster 13·63 ought to be 15·44, to agree with large table (XII.). That brings us to the load-factor. We have heard a great deal about the load-factor, but I think we shall have to hear a little more about it. What one may call Mr. Hammond's load-factor, or the gross load-factor, I think, does not affect the costs so much as the running load-factor—that is, the proportion which the plant which has to run during the day does, to what it could do if fully loaded. Our gross load-factor stands far ahead of everybody's else's; but, unfortunately, the calls we get on our plant are every day much analagous to the calls that Mr. Raworth says may come on the City Company when a cloud passes. I am fortunate if I get 60 per cent. load-factor day by day out of the plant running. We have a very large number of theatres on, and we never know when they are going to rehearse, so that we have always to stand by and be ready. That affects not only the steam consumption, but the boiler loss very materially. I quite appreciate Mr. Shoolbred's high appreciation of batteries, but I wish we could get a really large battery and rely upon it. At present we have hardly had confidence enough. On page 341 the question is raised whether the battery loss should be a distribution loss or a generation loss. I think the battery loss is a generation loss, generally speaking. Sometimes, as has been done, it is convenient to put the battery in a sub-station, to save putting in another feeder cable; but generally the use of the battery is to get a better load-factor on the engines—that is the running load-factor—and I think the losses should be charged against generation rather than distribution. With regard to the units missing, I think a great many of us would have our eyes open as to units missing if we really knew what we generated. A great many of us do not. There are

Mr. Patchell. several ways of estimating units generated. You can put a watt-meter on the dynamos, but very few people have it; you can put an ampere-hour meter on the dynamos; or you can—as has been general, I think, until lately—take the readings of the switch-board ampere-meters and multiply them by a constant for the voltage, which you take so many times a year,—and I think the number of times depends on how busy you are. I tried lately putting a watt-hour meter on one particular dynamo, and the result certainly astonished me. The observations covered a considerable period, and the units generated, as taken from the Weston ammeter readings—they were not critical readings, but taken by the ordinary switch-board attendant—multiplied by the actual voltage measured by recorders, were 30,178; and the units shown on an Aron watt-hour meter, which we tested and found correct, were 28,500. If we declare, as we have done till lately, to generate the units by switch-board readings, multiplied by a constant for voltage, I think a great many of us are declaring many more units than we ought. What we have to put stress on is the units sold, because really that is what we are there for. Whether a man generates a great deal more than he sells is also influenced by the way that he works his plant. Some people, especially nowadays, when we are getting our eyes open to steam-pipe condensation losses, are going in wholesale for electric motors all through the plant, and some are even saying they use no steam except through the main engines—they are going that length; so that, when we are generating for subsequent use, so to speak, in the boiler and pump room, with fans and all that class of work, the cost per unit generated is a little misleading. I think we should go by the cost per unit sold. I do not know whether Dr. Kennedy happened to have noticed this, but that sudden rise that he does not like in the rents, rates, and taxes synchronises with the new assessment in London, which has hit some of us very hard indeed.

Mr. Addenbrooke. Mr. G. L. ADDENBROOKE: There is a point I should like to allude to, mentioned by Dr. Kennedy. He spoke of the difficulty of recognising how the load-factor operated in certain circumstances, and how, with an apparently similar load-factor, the

Mr. Addenbrooke.

results would be different. I think it is worth while pointing out that the load-factor, on the basis in question, may be attained in two ways. You may get it with a very sharp peak or a broader peak; the quantity generated being the same in each case—I mean you might have the same maximum load, but you might get the rest of the load much higher, on an average, in one case than in the other, depending on the breadth of the peak. This might account for some of the discrepancies, I think, because it means that the plant would be differently worked in each case, and the labour required might differ also. I think the paper offers a very good illustration of one way of looking at the subject, in contradistinction to the way it has been looked at before by Mr. Crompton. Mr. Crompton has rather taken a few fundamental facts and correlated them with our experience and built up his conclusions. In this case we have an enormous number of results put in the aggregate, and we are reducing downwards, so to speak, from those. Of course it is valuable to have these; but my own feeling is, after having spent a great deal of time in looking through things of this sort, that you cannot take them too finely. It is all very well to have them by you as a guide, but I think there is rather a tendency amongst central station engineers to spend a lot of time in trying to appear in a high place on this list, when it is probable that the time would be better devoted to something which does not show at all in these figures, but which is very essential in central-station work, such for instance, as the life of cables, and other similar things, which are more neglected than they ought to be, if reports which come to me are correct. I think it is a pity we should judge everything from a point of this sort. Then, again, of course these figures refer to a certain set of conditions. Mr. Hammond has endeavoured in a rough way to forecast what is likely to be the case in future; but I do feel very strongly how conditions are changing, and I think that even in the next three or four years there will be changes which probably will make a very great deal of difference. The question of accumulators has been alluded to by Mr. Patchell. I think we want to treat this question of accumulators much more clearly. About 18 months or two years ago I had an

Mr. Addenbrooke. article in the *Electrical Review* on the subject. For instance, some large batteries in central stations had been set up within a year or so on something like this sort of arrangement— that they were to give a certain output for two or three hours, and the people who put them up agreed to maintain them for a certain number of years—say five years—at three-fourths of their maximum output. That is all very well, but it does not seem to me to be business. I should hope that a good accumulator in a central station would go on without any drastic repairs for something like five years; at any rate, five years is the sort of period when one rather expects that very heavy renewals may be wanted, whereas this contract—which is the sort of thing which accumulator manufacturers have been offering—expires just at the time when you want the help of these manufacturers. On the other hand, we do not want a battery maintained up to three-fourths of its output. If three-fourths is to be the output of the battery, I think that should be taken to start with, and this used as the fundamental figure. I deal with accumulators in this way: Supposing you charge accumulators at $\frac{3}{4}$d. a unit— which you can do easily— supposing you take the capital cost of these accumulators and lose 30 per cent. in them, this will make 1d. a unit for every unit going out of your accumulators as the pure generating cost. Say, then, for instance, that the accumulators cost you £20 a kilowatt on the three hours' discharge. I do not give that as the absolute figure, but merely for the sake of illustration. And suppose you get a certain output in kilowatt-hours from those accumulators in the course of the year. If you add the amount of labour required in looking after them, the interest on capital, and so on, you get a certain figure, which, I believe, if the accumulators are fairly used, comes out at between $\frac{1}{2}$d. and $\frac{3}{4}$d. per B.T. unit. Thus, if you can charge accumulators, including the loss, for 1d., then you have to add about $\frac{3}{4}$d. on for the capital cost, and looking after the accumulators; consequently you have current coming out of those accumulators at $1\frac{3}{4}$d., including all costs. Of course that is below a great many works costs, and it seems to me that if you can get substantial manufacturers to guarantee the life of accumulators on this basis, there is a very

large opening for them. And in that case the character of all these load curves and costs would be greatly altered. There is one other matter I should like to allude to, namely, that mentioned by Mr. Patchell with regard to Mr. Wright's ideas. I think Mr. Wright has done great service to the electrical industry in what he has said, and the bold way in which he has called attention to the incidence of works costs; but at the same time I think he is a little inclined, so to speak, to ride his hobby-horse to death. I cannot help thinking that the matter is being pushed, perhaps, in some instances, a little too far, and that the use of accumulators is very likely considerably to modify Mr. Wright's basis of calculation; in the interests of the consumers it is very desirable it should be modified. It is not desirable that we should penalise people who want to use a maximum current for a short time—possibly not at the time of heavy load—to such an extent as indicated now. For all these reasons, then, although these figures are valuable for comparative purposes, and as showing what is done now, I think we must not take them too much as absolute. Mr. Addenbrooke.

Mr. A. J. LAWSON: I also wish to bear testimony to the excellence of the paper which has been presented to us, and to its completeness; but I wish it had been possible, considering the vast improvements in costs which have been made in the year just ended, for the author to have included the figures of that year in his tables. There is, however, one point in Mr. Hammond's paper which has been misreported—he is not at all responsible for it—and from that the public have been led to believe that, the cost of production of electricity everywhere being about three farthings a unit, they ought to get it at something a little above that. I think we have fallen into a wrong way of putting cost when we speak only of the cost of fuel, oil, waste, salaries, wages, management, and so forth, as the cost of production. If we were to go a little further and say, for instance, that if we have a station with 40,000 lamps, on the average the units sold per lamp in a year will be 12, we therefore get an output of 480,000 units a year; and, taking the capital outlay per lamp connected at about £2 10s.—merely as an illustrative figure—we have £100,000 total capital, for which we require for interest and Mr. Lawson.

Mr. Lawson depreciation, or sinking fund, a certain margin, and putting that at the low figure of 6 per cent. all round, this would represent, on that output of 480,000 units, 3d. per unit. Therefore, even if you were producing your unit at three farthings works costs, to meet ordinary expenses you could not sell it at less than 4d. The staff and management expenses of London stations, which were referred to by Mr. Raworth, are undoubtedly exceedingly heavy. I think this arises to a large extent from the great number of stations which have been put down; and, roughly speaking, taking those that are now established and those projected, and putting the last half-dozen—the projected ones—on pretty much the same footing as the others of corresponding size in the metropolis, staff expenses of the amount of £65,700 a year, and management expenses of £49,600, make a grand total of £115,000 per annum, which I think will in course of time tend to prevent moderate prices being charged at all in some districts. The mistake has been—and I think it is one which Mr. Hammond will acknowledge—of having a local authority governing a small area, whether Vestry or Board of Works, putting in a station, or stations, for its own supply, when a few large stations, with very much less management and staff expenses, could have done the work more efficiently, and have been amply sufficient to cope with any demand they are likely to have to meet even in London.

As to the benefit derivable from a day load, I can speak with confidence of the very great advantage it is to any station. The Dover station with which I am connected has for the past eight months had a traction load, and I think it is one of the first stations in the country where the electricity supply company furnishes current to a Corporation to work its tramways. The result has been gratifying indeed. It has reduced the cost of production by at least a full penny per unit. Another thing which has lately reduced the cost of production there has been the use of motors within the station. The waste in long lengths of steam pipes, which were kept live the whole day to work the circulating pumps, has been avoided, and the coal bill has fallen very considerably in consequence. With respect to regularity of distribution, referred to by Mr. Shoolbred, that is not peculiar to

either the continuous- or the alternating-current system. I can point out two stations, one with continuous and the other with alternating current, in which the line of the recording voltmeter is practically straight: the one is Richmond, and the other Wandsworth. As to the losses of distribution, it is true that at the smaller station of Richmond the advantages are entirely on the side of the direct current, our distribution losses amounting to less than 10 per cent. per annum of the total output. We have, however, approached that very closely with alternating distribution, for, by switching out our transformers in the St. Luke's district after the heavy load has gone off, our last winter quarter's distribution loss did not exceed 15 per cent. With batteries in small stations the economy arising from their use is very great. In the Richmond station, with an output of less than 150,000 units last year, we ran the station practically with only one shift of men for eight hours per day only; we did not run the station at all on Sundays, the battery taking care of the ordinary Sunday load, as well as that of 16 hours of the week-days. Mr. Lawson.

Such a result, however, is only possible in stations of comparatively small size and output. A good load-factor obtainable by the use of units of plant of moderate size, which can be worked at maximum efficiency for any output, is productive of lower costs than anything else.

Mr. H. M. SAYERS: I cannot begin without adding my word of thanks to Mr. Hammond for the good work he has done during the last five or six years, which he has completed by aggregating his figures in this way. I will confine myself to one or two points which I have had occasion specially to notice and deal with. The first thing I will take is the question of the loss in distribution. A great many central stations neither know what they generate, nor what they lose in distribution; and they guess at it, or get at their generated units from ammeter readings, as Mr. Patchell said. Where I have had an opportunity of doing so I have always put wattmeters on the station switch-board, preferably on the machine side, because then the meters can be run pretty constantly at the most accurate load; but it is Mr. Sayers.

Mr. Sayers. sometimes of advantage to put them on feeders where the arrangements are not convenient on the machine side. These meters give the units generated. The meter on the station lighting and motor circuits gives the units used on the station, and the difference between that total and the total of the consumers' meters gives one the effective distribution losses. These include meter losses and discrepancies, and if the consumers' meters are ampere-meters, the loss includes any super voltage with which you may be favouring your consumers; but that is to some extent counterbalanced by the fact that you get the advantage of any lower voltage which may be supplied in overloaded districts. The analysis of the losses in distribution is sometimes a little complicated. It ought not to be very difficult in a direct-current distribution system, because the $C^2 R$ losses are probably pretty well calculable, and the leakage losses are usually negligible. With an alternating-current system it is a little more difficult, because, even if one gets by actual test the magnetising loss in the transformers when first put down, we know that the loss does not keep constant, but tends to increase; and, besides, it rarely happens that one has an opportunity of testing every transformer. I have found a very fairly approximate idea can be obtained of the transformer magnetising current losses by choosing a bright day, preferably a bright Sunday, and measuring the outgoing current with a Weston wattmeter—transforming down, of course, on the pressure side—and taking the difference between the watts and the ampere × volt readings: this gives one about one-third of the transformer losses, when the power-factor of the unloaded transformer is about 75 per cent., which is usually the case. I have found this to give very accurate results. The $C^2 R$ losses may be got approximately by the difference between the station voltage and the voltage delivered to the consumers. I think that, if the results were published as they were intended to be published by the Board of Trade, some of the records might be a little altered when they were taken on generated units, and some of our notions of distribution efficiency of various systems would also receive correction. That is very important to the engineer, because the actual cost of the unit at the station is

what he wants to know, in order to be able to reduce it; and the cost of the unit sold, although it is the thing of commercial interest, depends not only upon the station efficiency, but upon the distribution efficiency, and if he mixes up the two things he cannot attack them properly. I should like to call attention to the fact that the load-factor is very largely controlled by the tariff. I am afraid it is not always the case that the engineer can control the tariff; although, of course, if the engineer is one who has the confidence of his board or committee, the board or the committee will do as he advises. I have not found myself that boards or committees have always done what I have advised in that matter; sometimes they have given me half what I have asked, and that sort of thing. I had a very good illustration of the effect of tariff upon the load-factor at Bournemouth, where in 1895 the tariff was 8d. per unit, with a sliding scale of discount, under which only a few large consumers benefited. In 1896 the maximum charge was reduced to 7d. per unit, and the rebate system of Mr. Wright's was introduced, by which a large proportion of consumers benefited; with the result that the load-factor rose from 7·1 in 1895 to 11·2 in 1896, and 12·65 in 1897. Bournemouth is a place where the class of business has not varied very much, and is not likely to vary very much, but you will see that the tariff made a very great difference to the load-factor there. It made a great corresponding difference in the cost of production per unit sold. There is another point upon which Mr. Hammond has been asked to give further information, namely, as to the cost of coal at his record station of Leeds. I do not think it is likely that he will give it, but if he does, so much the better. It is obviously not very good commercial policy to tell people what you are paying for coal when naturally you will be sooner or later pressing other contractors for lower prices. I would like to point out that really it is impossible to compare the coal bill of different stations, and to conclude from the coal bill that one engineer is better than another. The question of the cheapest fuel at any particular station is entirely dependent upon local circumstances. If on the bituminous coal field, you will burn slack; you will endeavour to

Mr. Sayers.

Mr. Sayers. avoid smoke; but any way you will have a small coal bill. But if you are away from the coal fields, and the price is 60 or 70 per cent. made up of cost of transit, there is no doubt the best thing you can do is to buy the best coal you can get. I remember a particular case where I found 22s. a ton gave me a lower coal cost than 17s. per ton. That was owing to the circumstance that the cost of transit was a great part of the price of the coal. If you are close to a gas works, and the manager is reasonable, and will let you have his surplus coke or breeze at a low rate, you had best try that, unless close to the coal fields. Even the weight of coal used per unit affords very little information, unless the exact character of the coal is given. The calorific value of the coal also does not tell one its boiler efficiency, from the well-known fact that the calories due to the combustion of the hydrogen are very poorly utilised by the boiler. I have had to use a coal which gave a very high calorific effect—very little inferior to the best Welsh coal—but that coal contained only 54 per cent. of solid carbon, about 38 per cent. of volatile matters—principally hydrocarbons, of course—and about 10 per cent. ash. Although the calorific value of the coal was high, due to the hydrogen in it, yet its evaporative coefficient was not more than about 6·3 times its weight. Of course that was an example of what may be called low coal efficiency, and illustrates the difficulty of comparing coal costs or weight per unit sold.

With regard to batteries, I have had a little experience with them, and the conclusion I have come to is this—that if your daylight load is not sufficient to give a fair load to one of your plants, it is best to put in batteries; but as soon as it rises sufficiently to load economically one of the small plants, you should use the battery simply for the "top of the peak" and emergencies. This applies equally with alternating-current and direct-current stations, with the slight difference that the alternating station requires more plant and switch gear, and the battery is used at a somewhat lower over-all efficiency. There is another point to be noticed—that if your plant units are of really suitable sizes for the load diagram in the alternating station, the light load" will very soon get near the capacity of the smaller

units, and then your batteries will be of very little use indeed. Probably in most alternating-current stations the batteries are not worth putting in, unless the circumstances are such that the day load is likely to remain very small for a considerable number of years. Mr. Sayers.

To complete the description of test with wattmeter to ascertain the amount of transformer losses, it should be pointed out that the capacity current taken by the mains interferes with the accuracy of the test, diminishing the apparent amount of the loss. If the capacity of the mains is well known, this can be allowed for. Capacity should be measured for this purpose by measuring the displacement or capacity current at working voltage and frequency. Analysing wattmeters, separating the capacity, magnetising and load currents (*i.e.*, the leading, lagging, and co-phased currents), can be made on obvious lines, but they are not yet in the market, as far as is known.

The various meanings attached to the expression "load-factor" are a little confusing, and some general understanding is wanted. Mr. Crompton and Mr. Hammond use it in quite different but well-defined ways, each of value. I have found a "running plant "load-factor" useful on daily log sheets, as showing the amount of care and watchfulness used in running. This "load-factor" is the number of units generated during the day in relation to those which the plant used could have produced if run at full nominal load during the actual time of running, expressed as a percentage. With a log sheet designed to show graphically the running time of each set, the figure is easily got out by summing the product of "hours run × full load" for each set, and dividing the units generated by this number. It has been my custom to have the daily load diagrams show also the capacity of plant running at all times, which gives a graphic representation of this particular "load-factor." I have found the principal items of cost—*i.e.*, coal, water, and oil—per unit generated to follow this figure pretty closely, but inversely; and am inclined to think that, if it were ascertained and published for different undertakings, it would be found that "load-factor" in this sense has a greater influence on costs of production than when defined in any other way.

Mr. Sayers. Its value in the control of a station, and the education of the shift engineers, will be self-evident.

Mr. Gadsby. Mr. C. H. GADSBY: I should like to refer, in the first place, to Mr. Hammond's remarks on load-factor. He has defined his load-factor as the ratio of the mean output of the station to the maximum load during the year. This is all very well for the purposes of this paper, where capital charges do not come in; but when we have to take these costs in conjunction with capital charges, we shall be rather at a loss on the basis of a load-factor so defined; and I think, therefore, we ought always to take the load-factor as the ratio between the total units sold during the year and the number of units which would be generated if the whole of the plant installed were running throughout the year continuously at full load. Mr. Hammond has referred in one part of his paper to the supply of energy to tramways, and it is more particularly on this point that I am interested. If we take the mean load of the plant of a fair-sized tramway system at about 70 per cent. of the full load, and take its running hours at 16 per day, we shall have a load-factor (on the basis of Mr. Hammond's definition of load-factor) of about 50 per cent. This is equal to the load-factor that Mr. Hammond has taken as the improved load-factor in the case of Bristol to show what the cost of generation would be there, with such improved conditions, from which he brings out the cost at about one penny per unit. On the basis, therefore, of lighting figures with a load-factor corresponding to that of an ordinary tramway supply, Corporations ought to be able to supply tramway companies at about one penny per unit. But when, in addition to this, we take into consideration the facts that in a tramway supply there are no costs relating to interest on capital sunk in mains, or on the maintenance of the same, and that there are no losses in transmission, as the current is measured when it leaves the station, and that there will probably be only two shifts of workmen employed, instead of three, and that there will be no wages for meter-readers and no book-keeping expenses similar to those upon a lighting system where there are a large number of consumers,—taking all these things into consideration, I say that Corporations ought to be able

to supply, say, 500,000 or 600,000 units per annum at less than one penny per unit. I have recently had several agreements for the supply of current by Corporations to tramway systems through my hands, and in going into the figures I have arrived at an equation which I think represents very fairly the rates a municipal electric lighting undertaking ought to charge for current for such a supply. It is a simple straight-line equation, thus: Mr. Gadsby.

$$y = 0{\cdot}002\,x + 100,$$

where y = pounds sterling paid monthly for current;

x = number of units taken monthly.

On working this out, it will be found that on about 100,000 units per annum the charge per unit would be about 3d., that for 400,000 units per annum it would be about 1¼d. per unit, and for 700,000 units per annum about 0·9d., and for 1,200,000 units about ¾d. I think any municipal electric lighting undertaking ought to be able to supply current to a tramway system and make a fair profit at these rates.

Mr. Edw. W. Cowan: I should like, in the first instance, to say that I very heartily endorse what Mr. Patchell has said with regard to the difficulty of reading the units generated at the station. I have been trying for some years to do that, and, with the wattmeters I have used, have been unsuccessful with alternating currents and circuits of very large capacity. We commenced to work with a capacity of current of about 30 per cent. of our maximum current, and it is to-day over 10 per cent. I find the meters do not work accurately under these conditions. I have tried them from America and Germany and England, but the errors due to varying frequency or other causes make the results so unreliable that I do not think they are of any value. I wish to make a suggestion. It has occurred to me that the credit side of the revenue account—I am not speaking from an accountant's point of view—should be credited with something more than the price received for the units sold, and that is, the distance to which these units are distributed. The essence of supply lies, I believe, in distribution. We knew how to generate our electricity before we knew how to distribute it. I should not Mr. Cowan.

Mr. Cowan. like to add to the stupendous labour of Mr. Hammond in preparing this paper, but I should like to see a diagram on the wall showing the relation between the cost of the units generated and the square of the mean radius of supply, measured from the mean centre of supply. I think that in that way we should get a comparison which would be useful. The extent of distribution does undoubtedly influence the cost in many ways. I might have said something further about this, but at this late hour I will not do so.

The President. The PRESIDENT: Several gentlemen have sent in written communications; these will appear in the Transactions.

I will now close this discussion; but, before asking Mr. Hammond to reply to any observations which call for an answer, I must congratulate him on having brought before us a paper of exceeding interest—perhaps the most elaborate paper ever submitted to the Institution, certainly one containing a larger assemblage of figures than any other. Mr. Hammond has said that these figures had a fascination for him; he has certainly contrived to do all that was possible to make them attractive and fascinating to us. They show, among other things, the great effect of good design of apparatus, and of good management, in reducing the cost of the current supplied by companies, and to what a low point, under favourable conditions, it is possible to bring the cost of generating and distributing. Mr. Hammond has made us realise, perhaps for the first time, that the Electric Lighting Act of 1882 is not entirely destitute of merit. In requiring the publication, even in the limited form of a blue book, of the various particulars of cost of generating and supplying electric current for lighting, by local authorities and public companies, a very useful thing was done. It is due to Mr. Hammond that the details of cost supplied to the Board of Trade have been given wider publicity, and that they will be incorporated with our Proceedings and published in our *Journal* in such a form that we shall be able to get the utmost advantage from them. These eloquent figures proclaim some deplorable facts; among others, that in several instances the rent, rates, and taxes amount to as much as, and I think in some instances more than,

the coal bill. Mr. Hammond's figures are whips to lash us into activity to work, in order to reduce every item of cost that can be reduced. Mr. Crompton has reminded us of the limited scope of the engineer, and pointed out that, when he has done everything that can be done in his department to reduce cost, no great impression can be made on the total cost in those cases in which cost is largely increased by establishment and other fixed charges. The figures which Mr. Hammond has brought into such orderly array bring out the, to us, gratifying fact that the engineers have done very much, and that they are manfully vieing with each other for pre-eminence, in the most economical production of current. It is evident that what engineers can do to still further reduce the works cost, comparatively with total cost, is not very much, and that any large reduction of total cost must largely depend upon extension, and the consequent distribution of fixed charges over a larger volume of business. The paper brings conspicuously into view the increase that is going on in the number of generating works, the rapid growth in the use of electricity for lighting, and the continual lessening of cost. I will now ask Mr. Hammond to be good enough to reply to any observations that seem to call for reply. The President.

Mr. HORACE BOOT [*communicated*]: Central station engineers have to thank Mr. Hammond for having given them some work to do, but I am sure that I am echoing the general opinion of my colleagues when I say that the value of the paper more than compensates for the time spent in answering Mr. Hammond's numerous questions, and it will in the future be a most useful book for reference, to ascertain the results in other towns. Mr. Boot.

Paragraph 3.—Mr. Hammond states that "one-half of the "salary of the managing engineer should be charged to works "costs." I am quite in accord with him where Corporations employ a consulting engineer as well; but where the resident engineer undertakes both, it seems to me much fairer to charge half the salary of the engineer to management expenses, and the other half to capital account.

I am not overstating facts when I say that more than half the engineer's time is taken, in the consideration of, and drawing

Mr. Boot. up of specifications for future extensions; the time given to generating being very small, and usually undertaken by the principal assistant, supervised by his chief.

The Board of Trade recognise, that in companies it is usual to employ a manager and secretary, as well as an engineer, in which case the engineer would only have to do with the generating; so that they were correct in asking for the accounts of companies to be made up with the engineer's salary charged to works costs. In the case of Corporations, however, the engineer is usually manager, secretary, and often consulting engineer as well.

Paragraph 24.—Mr. Hammond considers the question of low costs, which may be summed up, as depending upon the following:—

1. The total number of units sold.
2. The load-factor.
3. The price paid for fuel.

However, I am of the opinion that sufficient attention is not given to keeping the capital outlay *down* in proportion to the *number of units sold;* and I would respectfully suggest that, the next time Mr. Hammond honours us with a paper, he should provide a *column* for the *capital* outlay per *unit* sold, as Corporations and shareholders of companies do not particularly wish to see a low works cost unless the *financial* result is as favourable, and it is useless for stations to go in for very expensive machinery to enable them to obtain a record works cost at the expense of the total financial result. There must be a balance between what might be called "capital spent to obtain a good financial result," and "capital spent to obtain a low works cost." Corporations suffer more severely than companies with respect to interest and sinking fund payments on capital, which, in most instances, amount to 6 per cent. on the total capital outlay; whereas, had it been a company, there would have been probably 4 per cent. debentures, 5 per cent. preference, and the ordinary shares, which certainly would not, when averaged, total out to 6 per cent. on the total capital, thus showing that every £100 spent by the Corporation affects their financial result more seriously than it would a company in its early years. For that reason I think more attention is paid to works costs than need be, and not sufficient

to the capital outlay being kept in proportion to the units sold.

Paragraph 29—*Load-Factor.*—I am surprised to hear that in many works the units generated are not arrived at by meter, as I thought that all works would have metered their generated units. I was also under the impression that in making calculations of cost per unit, the units *sold*, and *never* the units *generated*, were taken into account. Mr. Hammond seems to infer this is not always the case.

Paragraph 40.—Mr. Hammond is kind enough to set out his usual tests, which, however, seem to be rather unnecessarily complicated, and liable to error. The usual method of testing the weight of water condensed appears to me to be preferable to that of the water pumped into the boilers, when testing steam consumption.

Paragraph 43—*Units used in Distribution.*—I have always been careful to ascertain as nearly as possible the number of units lost in distribution, which loss is made up as follows :—

1. The loss due to consumers' meters not registering the full amount.
2. The magnetising loss.
3. The C^2 R loss.

I have tabulated these in something like their order of importance.

Paragraph 48.—Mr. Hammond makes an erroneous and unfair statement when he says, " A rough test of all-round efficiency of any " works can be obtained by the study of the coal bill." Had he said " by the study of the financial results and the price of selling the " unit," I should have agreed with him; and it appears to me hard on central station engineers who, unfortunately, have to obtain their goods by a company that charges an exorbitant rate, sometimes amounting to 250 per cent. on the value of such goods. This, unfortunately, is my position, being served by the South Eastern Railway, and I think, in justice to other engineers who are similarly situated, that his statement should be contradicted.

Paragraph 61.—I am glad to see that a consulting engineer who is responsible for several companies, considers, that they have

Mr. Boot not put sufficient amount aside for *depreciation.* I am even of the opinion that, unless circumstances are exceedingly favourable, 3 per cent., as paid by most Corporations, is not sufficient; and I think it good practice to form a reserve, or carry forward a large balance, before reducing the rates or declaring a dividend.

Mr. Geipel. Mr. W. GEIPEL [*communicated*]: Every engineer interested in the supply of electricity should be greatly indebted to Mr. Hammond for the vast amount of time and trouble he has taken in collecting the valuable data given in this paper.

There are several points upon which I should have liked to touch, but, with a view to condensing what I have to say, I shall confine myself to one or two of what appear to me the more important.

In the first place, I refer to page 268 of the paper, where the author says, "It may be objected that in going beyond the "analysis of the works costs I am stepping out of the path of the "engineer." This is an opinion which was held by Mr. Crompton, who in his paper, "The Cost of Electrical Energy," read before this Institution in 1894, in speaking of the costs outside of works costs, said we engineers have nothing to say in controlling these figures.

I beg to dissent entirely from these opinions, notwithstanding the authority of two gentlemen standing so high in the profession.

The amount of the standing charges is largely controlled by the policy adopted by the engineer in laying out the works. If the works are erected for a small outlay, the interest and depreciation, rates and taxes, will be small in proportion. If the site for the station is selected with judgment, the rent, and law actions for alleged nuisance, will be less.

When the capital outlay per kilowatt varies from £49 at Glasgow to £242 at Notting Hill, I think, gentlemen, that the capitalist or statistician will not accept the whole of the responsibility for such discrepancies. On the contrary, the engineer will bear the credit or discredit, as the case may be.

Let us see what effect such variation as this has on the cost of the unit. Mr. Hammond gives a figure for depreciation, viz., 3

per cent. on capital outlay; to this I add 4 per cent. for interest, making 7 per cent. This makes the item in question £3 per kilowatt per annum at Glasgow, and £17 for Notting Hill. Mr. Geipel.

With a 15 per cent. load-factor, each kilowatt of plant delivers 1,300 units per annum, so that interest and depreciation would then cost with the Glasgow outlay 0·55d., while for Notting Hill it would be 3·14d per unit.

I say, then, that this question is altogether too important to be placed beyond the pale of the engineer's jurisdiction. On the contrary, it cannot be too fully impressed upon him what a vital effect the capital outlay has upon the cost to the consumer, and how important it is that before deciding on every fresh outlay he should weigh carefully in the scales the earning or saving power of such outlay on the one side, and the increased standing charges on the other, so that he may see a clear balance in the favour of the former.

I am quite of opinion that in numerous cases vast sums have been spent on arrangements for the saving of coal, for example, while the monetary saving has been quite insignificant compared with the standing charges and upkeep of such arrangements.

Generally speaking, I consider that a certain portion of the generating plant—viz., that which is most constantly in use—should be made as efficient as possible. But the other portions—which need only be kept for use during the short periods of the year of maximum demand, or as stand-by, and which may be looked upon as the non-dividend-earning portion—should be planned more with a view to low first cost than for high efficiency. I am aware that in either case reliability should be a first consideration, but reliability and the complication involved in the steam-power plant for high efficiency are not necessarily in parallel; they are frequently out of phase.

On page 369 Mr. Hammond summarises the data in his paper, in prophesying that with a 5,000,000-unit output the works costs will be 0·5d. per unit, and the total costs 0·75d per unit, exclusive of interest and depreciation.

In the discussion on Mr. Crompton's paper four years ago, I put the works cost per unit at 0·6d., and the total cost, including

Mr Geipel. interest and depreciation, at 2d.; basing my calculation on a price of £60 per kilowatt, with a 15 per cent. load-factor.

I will now go one better in my prophecy—first, because the supply of power for tramway and other day uses will improve the load-factor in favourable cases to something like 25 per cent.; second, because I believe that the Glasgow record of capital outlay—viz., £49 per kilowatt—will be broken.

With an outlay of £45 per kilowatt and a load-factor of 25 per cent., the standing charges at 7 per cent. amount to 0·35d. per unit. Mr. Hammond's estimate of works cost is so near to my own of four years ago that I will take his figure for total costs at 0·75d., and prophesy that under most favourable circumstances the cost per unit to the consumer, including all charges and profit, will be reduced to 1·1d.

And where the supply is at 100 volts, enabling the use of lamps strained to $2\frac{1}{2}$ watts per candle, this price is, in my opinion, equivalent to gas at 6d. per 1,000 cubic feet.

I notice that Mr. Hammond strongly urges the managing engineer to tout for orders from users. In my opinion, gentlemen, long before the cost of supply has fallen to the limit I prophesy, he will find very little time for touting in other directions than for contractors to supply the wherewithal to enable him to keep pace with the demands of his clamouring customers.

Mr. Cooper. Mr. ARTHUR T. COOPER [*communicated*]: I have been greatly interested in the able and exhaustive treatise on costs which Mr. Hammond has presented to the Institution for the edification of electrical engineers.

In designing systems of town supply nowadays, a healthy output, as Mr. Hammond so aptly puts it, is a practical certainty, and it is curious that the engineers responsible for their design have not given more consideration to the question of load-factor when deciding on the system of supply.

To obtain a good load-factor with a single-phase alternating system is merely a matter of luck, as its warmest advocate cannot but admit that incandescent lighting is the only satisfactory use to which it can be put; and, even when helped by some "peak-

"reducing" tariff, such as that devised by Mr. Arthur Wright, a fair load-factor cannot be obtained without the aid of fogs and similar friends of electricity supply. Mr. Cooper.

What it is, therefore, that induces engineers to compare single-phase high-tension alternating with three-wire low-tension continuous supply on the relative feeder costs, is somewhat difficult to understand; and so convinced am I that continuous current is the most advantageous system of supply from the points of view of both the consumer and the undertaker, that I have decided to make all extensions to the Reading system (of which I now have control) on continuous-current lines, and I hope that the results for next year will be such as to remove the stigmas of lowest load-factor and highest works costs shown by Mr. Hammond's figures.

Mr. Hammond, to show the importance of improving the load-factor, takes the case of Bristol to experiment with; but, although he rather cruelly shows that beautiful results might be obtained were the load-factor increased fourfold, he does not offer any suggestions as to how this consummation is to be arrived at. Bristol has a single-phase alternating supply, and, I should say, a fairly representative list of consumers; and, unless some radical changes are made to enable a large power demand to be catered for, I fail to see how much improvement can be effected.

Mr. Hammond's paper forms a splendid reference library to central station engineers; and, although many of them may differ from him in the deductions to be made from the statistics he has so skilfully put into shape, yet I think that no one of them will fail to be grateful to him for his labour.

Mr. J. F. C. Snell [*communicated*]: These tabulated results of the lighting stations throughout the country will be much appreciated by central station engineers. I regret that Mr. Hammond has not completed his paper by the addition of the prices of the stations per kilowatt installed, and by stating whether each station is worked condensing or non-condensing. For those supply stations that are fortunate enough to be placed by a canal side, or river side, and are accordingly able to economise in coal, water, and efficiency by the use of condensing engines, have a great pull on others—as in Sunderland—which are obliged to

Mr. Snell. exhaust to atmosphere. The influence of the output is, I think, rightly put first, because of its influence on the standing costs, although the load-factor might as well be bracketed with it. Public street lighting is a great agent in the improvement of this load-factor, and that station is happy which can command an extensive street-lighting scheme from the first. A judicious adoption of storage cells of course helps the load-factor of the station, largely enabling the engine load-factors to be kept up as a rule to over 90 per cent. (where the units of plant have been judiciously stepped up). It would seem that this, with street lighting and traction load, will prove the solution of the difficulty of obtaining a decent load-factor, together with such a system of charging as Mr. Wright's, of Brighton, which induces the consumer himself to help in the attainment of this object. I think it is a difficult matter to compare stations until they have been running some three or four years, when the wear and tear will have proved the competency of the plant, and its influence on the repairs and maintenance item. This, and the question of condensation before mentioned, seem to me to be two of the chief engineering points which require careful inquiry when analysing and comparing costs of various supply stations.

Mr. Short. Mr. M. SHORT [*communicated*]: Mr. Hammond is to be congratulated on the very complete way in which his paper has been put before the Institution, and the amount of work involved must have been very great indeed. It is, therefore, with reluctance that I venture to make any remarks in criticism of the paper. The one point, however, that strikes me forcibly is, that it is scarcely fair to undertakings worked by companies to be compared with undertakings that are worked by local authorities. In some undertakings of the latter class there is perhaps little, if any, rent or taxes to pay,—certainly no rates,—and probably very little on account of fuel, especially where the local authority owns the gas works, and can thus obtain coke cheaply. It is not surprising, therefore, that the record in total costs should be held by local authorities.

Then, again, in some towns water is obtained free of cost. I believe this is the case with some companies as well as local authorities. If Mr. Hammond could add on some amount to

cover the items that do not appear in the ordinary total costs in these cases, it would form a very interesting comparison with undertakings that are not so fortunately situated. Mr. Short.

Mr. E. H. COZENS HARDY [*communicated*]: On pages 253–267, Mr. Hammond has given us tables containing very valuable information; but to base orders of merit on these figures seems most misleading. The efficiency of a station as regards consumption of coal and general supplies should surely be determined on the basis of cost per unit *generated.* It is unfair that the company which is unfortunate enough to have an inefficient system of distribution should be made to suffer for this all along the line, and necessarily to appear wasteful with its coal, oil, and other stores. On Mr. Hammond's basis of cost per unit sold, the efficiency of the distribution system is a factor which directly affects all his tables of costs. Mr. Cozens Hardy.

For instance, if two stations had identical plants and equally efficient staffs, but the one had a better distribution system than the other, Mr. Hammond's basis must necessarily give to the former a better place than to the latter in his list of merit for coal consumption.

Selecting from the "Coal" table for 1896, on page 272, those towns for which the necessary data for arriving at the cost per unit generated are available, and comparing the order of merit on this basis with that given by Mr. Hammond, we find very considerable changes:—

On Mr. Hammond's Basis.	Based on Cost per Unit generated.
1. Aberdeen.	1. Leeds.
2. Leeds.	2. Aberdeen.
3. Edinburgh.	3. Edinburgh.
4. Pontypool.	4. Newcastle.
5. Preston.	5. Pontypool.
6. { Bradford.	6. Manchester.
6. { Manchester.	7. { Southport.
8. { Southport.	7. { Sunderland.
8. { Newcastle.	9. Preston.
10. Whitehaven.	10. Bradford.
11. Sunderland.	11. Whitehaven.

Mr. Cozens Hardy.

In a complete list the changes in order would probably be still more noticeable.

The curious difference in the prices paid for coals of apparently the same calorific value, at stations with equal transport facilities, has already been alluded to by Mr. Crompton. If we take the St. James's and Westminster Companies for 1896, their load-factors are both about 15 per cent., they use about equal weights of coal per unit generated (St. James's, 5·7 lbs.; Westminster, 5·6 lbs. per unit generated), and, therefore, probably use coal of the same calorific value; yet the Westminster Company buys coal at a price nearly 10 per cent. higher than that paid by the St. James's Company. One is almost forced to the conclusion that, if the figures are to be relied upon, a good engineer is not always a good buyer.

Mr. Hammond.

Mr. R. HAMMOND, in reply, said: I notice the time allotted to me is about minus three minutes, and I suppose the only way in which I can accomplish the feat of making a speech in that period of time is to begin by sitting down; but if, even at this late hour, I may trespass upon the meeting for a short time, I certainly should like to have an opportunity of touching upon one or two points. I have to thank you, Sir, for the very cordial way in which you have echoed the many kind sentiments that have been expressed during the debate. I should have liked, if the exigencies of time did not forbid, to take each speaker in turn, beginning at the last and working backwards, and to comment on each speech; but I intend to hold myself in on this occasion, and not to do so. I must confess that I am greatly encouraged by the invitation of the last speaker to still more widely extend the scope of my researches. He puts before me the problem of working out a factor by which one can express the number of miles over which the electrical current, which is produced at so much per unit, is delivered to consumers. Just as I conceived all my labours to be finished, I certainly have something given me this evening, at the last moment, to ponder over, which will, I fear, cause me to somewhat curtail my rest to-night; for I shall not be satisfied until I have, at all events, endeavoured to hit upon some factor.

Mr. Hammond.

Turning from the last speaker to the first, it will be in the memory of the members that General Webber, who opened the debate, seemed to be rather doubtful as to whether these figures were going to be of any use to us at all. He said that possibly they were as correct as figures that were unofficially provided usually were, and that on the whole he could hardly see how they could be more than suggestive to the particular engineers who are responsible for the construction and running of these undertakings. I am inclined to think that, even if these figures are only of use to those engineers, they are of great service. The costs of generation and production, as we have found out from this debate, are largely under the control of the engineers in charge, and the 150 stations are made up of single units. Let the attention of the engineers in charge be drawn to particular facts connected with their own works, and the whole industry is benefited.

General Webber further says, from the knowledge he has of the sources of information from which the tables have been compiled, that he feels they are not by any means accurate. I have in every case taken the official returns filed with the Board of Trade, and have therefore felt on safe ground. Of course I cannot answer for the way in which the returns with which General Webber is acquainted are made up, but in the case of the works with which I have anything to do, I am able to state that the returns are made up most faithfully; and I think I may go further, and state that the central station managers and engineers throughout the kingdom will be prepared to declare most distinctly that the figures they fill in to the Board of Trade returns are the actual working figures, in the case of companies, audited not only by their own auditors, but vouched by the Board of Trade auditor.

I entirely agree with Mr. Patchell, and with the other speakers who have joined with him, in the expression of opinion that the real basis of cost is the cost of delivering the electrical energy to those who are using it, and that the cost of generation alone is misleading as compared with the combined cost of generation and distribution. On the other hand, I am sorry to find the bulk of the central

Mr. Hammond.

station engineers in this country are very careless in recording their units generated, for it is only by a comparison of the units generated and the units delivered to consumers that a record can be obtained of the units "lost" in distribution. If there be one thing that the gas industry has learned in these 50 or 60 years, it is to be most careful to note the units lost in distribution. No gas engineer considers himself worthy of the name unless he is able to tell you immediately the percentage of units lost in distribution; and I do trust that he who compiles for the Institution the next paper on this subject will be able to report that he finds that great care is being exercised throughout the country in the recording of the difference between units generated and units sold.

Reference has been made by many speakers to the question of load-factor. I said, with regard to it, that, next to output, it was the determining feature upon costs. In respect of that, Sir, it has been said outside, in one of the periodicals, that I ought to put load-factor first; that the load-factor is everything, and the output is nothing at all. I do not quite take that view, but I fear, with regard to the load-factor, that, though it has an immense influence upon cost, this is not made so evident as it should be because of the slipshod method of its determination. The records are not kept as they ought to be kept. Some go to the other extreme, and depreciate the influence of load-factor on costs. I would remind those who do so that there are certain works, working very cheaply, which are greatly benefited by the improved load-factors due to their extensive arc lighting. It is self-evident that, if you find a solid load arising from supplying four or five hundred arc lights, running the bulk of the night, you largely account for the decreased cost of such a station, compared with one with no arc lighting. To that extent the load-factor has an immense influence upon cost. I use the word "load-factor" in the sense of the ratio between the maximum demand of the year, multiplied by the number of hours in the year, and the actual annual output. In the course of the debate, some have expressed an opinion, which Mr. Crompton has supported, that the current method of determining the load-factor—of which term he was really the

sponsor, in the paper that he read before the Institution of Civil Engineers in 1891—was not so good as multiplying the number of hours of the year with the plant capacity. The comparison of this factor with the actual output gives a result that is a very useful thing to know, but I should call it "plant-factor," and not "load-factor." I did not work out my load-factor on this basis, because no works in this country have yet reached the point of finality in their capital account. That being so, it is futile to base a load-factor for comparative purposes on the capacity of the plant installed in the works, because those who are responsible for the capital account in some works take a brighter view of things, and put down more capital in advance, than others. In some works, also, the demand increases so rapidly that it leaves no margin of plant unutilised. Leeds is one of such stations, and comparisons of the ratio of the Leeds total kilowatts capacity from time to time to the output of the year, with places where a more liberal view had been taken of the future, would be futile. Therefore I say, Be content, as far as load-factor is concerned, to base it upon your maximum output, continued during the year, rather than upon your plant capacity. Mr. Hammond.

Some of the speakers express themselves as not quite clear as to the new method which I suggest of determining the load-factor as I understand it. My point is that, until a district has reached the point of saturation, the number of lights fixed at the end of the year will be much higher than the number of lights fixed at the beginning of the year, and that the resultant units delivered to consumer, compared with the maximum load at the end of the year, would not give so good a record of the load-factor as a comparison of those units with the mean daily maxima throughout the year.

Professor R. H. Smith is very doubtful whether the figures set forth by me and applied to Bristol, as the result of a greatly improved load-factor, are correct. He is doubtful, in the first place, whether the improved load-factor will have the effect upon the coal bill that I imagine; but I can assure the Professor that a very large proportion of the coal is used in making up stand-by

Mr. Hammond. losses, and I feel confident that the improved load-factor which I postulate at Bristol would lead to the reduction that I name in the coal bill.

With regard to wages, he does not think that a set of workmen can be got to produce four times the output with only 25 per cent. more bulk wage payment. Here I differ from the Professor. Works operated by three shifts of men all the year round would hardly have any appreciable increase in the wages payment, by the fact that the plant was producing electrical energy instead of running empty.

With regard to the engineer in charge, I think most of us will agree that he would be very contented with 50 per cent. increase in his salary, to balance the greater responsibility of the bigger output, which, according to hypothesis, is not employing a larger amount of plant.

In connection with load-factor, Mr. Shoolbred expresses surprise that I have not pointed out the very great advantage of the use of secondary batteries, but throughout my paper I was careful not to discuss systems, but simply to set forth the facts which the Board of Trade reveal in reference to cost of production; otherwise I might have had to make an invidious comparison between the Bradford works referred to by Mr. Shoolbred and those in the neighbouring city of Leeds.

Referring to Leeds reminds me that one or two speakers have been anxious to know what is the cost per ton of fuel in Leeds. Mr. Patchell twits me upon the fact that I have confessed to having the Leeds figures on my table every Monday morning. I am compelled to reply that the figures are supplied to me, not in my capacity as "Chesterfield Junior," but as the consulting engineer of the Leeds company; and, as long as the directors desire to keep to themselves the price which they actually pay for fuel, I am bound to respect their wishes. I may say, however, that the conjecture that we burn 5 lbs. of coal at Leeds per unit delivered to consumer is not correct. By means of the mechanical stokers, which we have in use at Leeds, we burn a low-class fuel, requiring as a corollary a good many pounds per unit. The engineer does not regard of paramount importance the number of pounds of coal

per unit, but the cost per unit, and this he hopes to reduce from his present record figure of 0·25d. to 0·20d. Mr. Hammond.

I have to thank Professor R. H. Smith for pointing out some conflicting figures which appeared in the proof of my paper laid before the members in the first instance, which figures have been corrected in the final copy.

Professor Smith is under a misapprehension, however, when he states, in reference to Table I., that the cost of the coal in pennies per unit, the price of the coal per ton, and the number of lbs per unit can be got independently each directly from the accounts of the undertaking. I wish that were so; as a matter of fact, the only one of the three items that can be so obtained is the cost of the coal in pennies per unit; and this column, as set forth in my paper, was worked out from the figures contained in the Board of Trade returns. The other figures were supplied by courtesy, and in some cases care was not taken by the engineers who supplied them in working out true averages.

In conclusion, I must express my complete conformity with those speakers who have pointed out that, to make a review of costs complete, a careful analysis should be made of the capital account of each undertaking. When, however, I found my paper ran to 132 printed pages, I felt that I should only weary the members if I ventured to embark upon such analysis, and I also felt that those engineers who had supplied me with data ought to be allowed a rest before I begged them for more.

The question of capital expenditure is a most important one, justifying a paper by itself, and I trust that in the next session a paper may be laid before the Institution showing how the results which I have tabulated in my paper are affected thereby.

The President: I have the pleasure to propose that we accord to Mr. Hammond a very hearty vote of thanks, and that we express this by acclamation.

Carried by acclamation.

The President announced that the scrutineers reported the following candidates to have been duly elected :—

Member :

Alfred Blackman.

Associates :

Samuel Harry Hill Barratt, A.M. Inst. C.E.
Henry J. S. Brownrigg.
Thomas Harding Churton.
William John Crampton.
Llewelyn Lloyd Foster.
Hugh Reginald Hearson.
Robert Wm. Jackson.
Frederick Wm. Lacey, M. Inst. C.E.
William Lund.
William McGeoch, jun.
Arthur Ernest Malpas.
Thomas Hugh Parker.
James W. Polley.
Cyril Probyn Napier Raikes.
Bertram Gurney Stewart.
Arthur Kepple Taylor.

Students :

Edward Domett Morgan.
Samuel Romilly Roget, B.A.
Joseph Frank Shoolbred.

The meeting then adjourned.

The Three Hundred and Fifteenth Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, April 28th, 1898—Mr. J. W. SWAN, F.R.S., President, in the Chair.

The minutes of the Ordinary General Meeting held on April 21st, 1898, were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council:—

From the class of Associates to that of Members—

Julian Money Vernon Money-Kent.	James Taylor.
J. T. Niblett.	C. T. Williams.
	Alfred David Williamson.

From the class of Students to that of Associates—

William Bunn.	Edgar Walford Marchant.

Donations to the Library were announced as having been received since the last meeting from Mr. Blackwell, Member, and from the Municipal Electrical Association, to whom the thanks of the meeting were unanimously accorded.

The PRESIDENT: At a recent meeting you were informed that there was to be a banquet in Brussels in honour of M. Gramme, and that a testimonial, to which you were invited to subscribe, was being organised in connection with it. It was also announced that an address from the Institution would be presented on the occasion. We have now an acknowledgment of the receipt of the address and the subscriptions. The following is a copy of the address:—

"To Monsieur ZÉNOBE THÉOPHILE GRAMME.

"The members of the Institution of Electrical Engineers of "Great Britain unite with the electricians of Belgium and of "all nations in congratulating you on the eminent services "which you have rendered to the electrical industry. By the

"improvements which you introduced at so early a date into "electric machines you placed the manufacture of the dynamo "on a truly commercial basis, and prepared the way for the "great development of electric lighting which has marked the "closing years of the present century. On behalf of our country "we beg to thank you, and beg to express our admiration for "the work which has secured to you world-wide recognition "and lasting fame.

"Signed on behalf of the Institution of Electrical Engineers:

"JOSEPH W. SWAN, *President.*

(Delegates) { "W. E. AYRTON, *Treasurer.*
"S. P. THOMPSON.

"W. G. MCMILLAN, *Secretary.*"

The medals due to the various subscribers will be received by the Secretary when they are ready, and distributed.

The following papers were then read:—

EARTH RETURNS FOR ELECTRIC TRAMWAYS.

By H. F. PARSHALL, Member.

Mr. Parshall. Considering the small difference of potential at which electrolytic action may take place, a matter of primary importance in electric tramway systems in which the rails are used as return conductors, is that in reference to the rate of fall of potential, and the difference of potential between the rails and the general mass of earth, the magnitude of which may vary according to such local conditions as the locality of gas and water pipes, the conductivity of the earth between the rails and such pipes, and the conductivity of the pipes themselves.

The exposed surface for leakage of the track is very great; thus in the ordinary four-track tramway system there is some 50,000 feet per route mile. With so great a surface, and with, as is generally the case, considerable conductivity of the concrete and earth, a large fraction of the current may be diverted from the rails, even in short lines, and with a maximum drop as small as that specified by the Board of Trade.

Thus, in tests recently carried out in a line some eight miles long, it was found, by cutting the track at the middle of the line and

inserting an ampere-meter, that some 60 per cent. of the current Mr. Parshall.
was returning through the earth itself. Tests made as to the conductivity of the earth return showed as a whole that it was about one and a half that of the rails, bonds, and fish-plates, which would indicate that on an average about 33 per cent. of the current was leaving the rails. In other words, the voltage drop in the earth return was but two-thirds of what it would have been had the current been wholly in the rails.

In laying the rails, therefore, it seems desirable to adopt such methods of construction as will, to a considerable extent, insulate the rails from the adjacent mass of earth. The conductivity of the earth is considerable, and with differences of potential up to the limit established by the Board of Trade is so great that, in the cases I have examined, stray currents are not diverted from the mass of earth by gas and water pipes. I have made tests by cutting the rails and measuring the currents at different points, and, so far as could be determined, the neighbouring gas and water pipes were not traversed by the current. In one special case two lines of the tramway formed two sides of an acute angle triangle, and a very large water main formed the third side, and, even though some 50 per cent. of the current did not come back through the rails, the tests showed beyond doubt that there was no current whatever coming across the third side of the triangle through the water pipe. Of course, with the small difference of potential common in practice in this country, the C.E.M.F. of polarisation which accompanies currents flowing between conductors when electrolysis takes place, is an important element in determining the law of current-flow.

The tests carried out by the writer have, in every case, shown that the joint conductivity of the rail and of the earth is considerably greater than that of the rails themselves. For this reason there exists the necessity of determining the conductivity of the rails, fish-plates, and bonds, before the track is laid in the earth, so that after a roadway is completed the measured drop may be taken as an indication of what percentage of current is straying from the rails; so that tests made from time to time may indicate the general condition of the bonding.

In general it is desirable that the earth return be isolated to

Mr. Parshall. the greatest degree practicable from any other metallic conductors liable to be affected by electrolysis. In some cases, however, where the drop in the earth return has been comparatively great, attempts have been made to prevent electrolysis by bonding the rails to the adjacent gas and water pipes. The results have been more or less satisfactory. It is obvious that, if the rails and adjacent gas and water pipes can be kept at the same potential, electrolytic action can be effectively prevented. Considering, however, the very considerable conductivity of the earth, it would seem doubtful whether such bonding would prove effective with any considerable drop in the rails, since in this case stray currents would flow from one part of the system to another, and at such a difference of potential as would cause electrolysis.

In the case of lead-sheathed cables running parallel to earth returns of tramways the results have been entirely satisfactory. and are conclusive, since, in the absence of bonding, the lead sheathing was rapidly eaten away. This instance, however, is not to be relied upon as an indication that it would be safe to carry out the same process in dealing with gas and water pipes. The lead sheathing is homogeneous, of comparatively high resistance, and with small surface exposed to the earth, whereas the reverse holds true with gas and water pipes as ordinarily laid down. I have no doubt that there are cases in which effective bonding of the rails adjacent to conductors might give entirely satisfactory results, but I should hesitate to make any general recommendation to this effect, since in very many cases a result directly opposite might be obtained.

There is such a difference in soils—first, as to corrosive properties; second, as to electrical conductivity—that a general rule which would prevent electrolysis in every case would be unnecessarily severe, and in many cases prohibitive. It is obvious that, where currents stray generally into the earth so as to enter metallic conductors, the difference of potential should not be allowed to exceed that at which electrolysis begins, + the drop in the earth itself.

In a given system of distribution the controllable features in the earth return are practically limited to the method of jointing the cross section of the rails, and to the chemical composition of the rails.

The chemical composition of the rails cannot be altered greatly, since rails low in carbon, but of high electrical conductivity, are found to wear away so rapidly that high carbon rails are a practical necessity. Mr Parshall.

The cross section of the rail is in practice largely determined from mechanical considerations, and in the best practice rails of from 80 to 100 lbs. per running yard are used.

The method of making the rail joints is practically, then, the only factor controlling the resistance of the rail return that is susceptible of wide variation in practice.

The electric welding of the rail joints has been tried in the United States, but thus far the results have not been such as to encourage manufacturers to advocate the use of the system, or the tramway companies to adopt it.

The joints in electrical tramway work are equally objectionable from either a mechanical or electrical point of view, so that a system of perfectly welded rails would meet with general favour. In practice the effect of temperature in causing expansion and contraction has been noticeable in long lengths of welded rails; but the effects thereof have not been of such a serious nature as might be expected from the range of temperature.

From the reports I have at hand, it appears that there were unexpected results of the welding process that made themselves evident in the course of time.

First, the electrical conductivity of the welded section was less than that of a solid rail.

Second, the portions of each rail near the weld were so softened as to wear away unevenly.

Another unexpected result was that, owing to the sudden increase and decrease in temperature, the rail took a very high temper at the weld, so that its power to withstand shock was decreased.

To the writer's mind it is not improbable that these mechanical difficulties could be overcome. Welding apparatus of sufficient capacity, however, is costly; and it is frequently difficult to arrange for the amount of power required; so far, therefore, the process has not been employed in this country.

Mr. Parshall. Another method of somewhat the same nature as the process of welding is that known as the "cast weld," or the "Falk joint." This joint is made by pouring molten metal into a metal mould clamped round the rail joint. The surfaces of the cast metal that come in contact with the mould and with the rail joint are chilled, and are thus prevented from forming a perfect weld. I believe it has been asserted that a weld is effected. It seems, however, extremely doubtful, since without the use of a flux a weld is almost impossible between cold wrought steel and molten iron. The rail expands after the metal is poured around it, and remains expanded until after the cast iron has set, and finally resumes its former size. This affords a slight clearance for expansion and contraction, and accounts for the mechanical success of the joint, which, if carefully applied, makes when new a perfect mechanical track; although, in the writer's mind, the difference of resilience between the part surrounding the casting and the remaining part of the track may eventually cause uneven wearing away of the rail.

The clearance above spoken of undoubtedly admits a certain amount of moisture, so that by the formation of oxide the resistance of the joint increases in the course of time. From the results of tests which I have at hand, it also appears that the electrical resistance of this joint, even when new, varies considerably; so that, considering the low voltage restrictions in this country, it should be used in connection with an efficient form of bond. Owing to the rigidity of the joint, however, copper bonds will undoubtedly be found more durable in conjunction with it than with a fish-plate form of joint.

Bonds.

The bonds generally used up to this time are of the pressure-contact type, and in making any general statements this is naturally assumed as the basis.

In the discussion of a paper read some time ago before this Institution, the writer pointed out that, according to experience with pressure contacts in central station work, 100 amperes per square inch had been found the limit in best central station practice; and that, considering the trying conditions to which

bonds are subjected in the earth, one-half of this value would more likely be satisfactory. Mr. Parshall.

In actual practice I have found it advisable to work to a still lower limit, and in most of the systems which I have designed the current-density at surface of contacts does not exceed 25 amperes per square inch. Experience shows this limit a safe one, and that the contact resistance is negligible as compared with the resistance of the rails.

Considering the complicated phenomena accompanying a junction of copper and iron, in respect to the difference of potential caused by the contact of dissimilar metals, and the effect due to a current passing between dissimilar metals, it seems in the normal case that all E.M.F.'s would balance each other, since, in the case of the current keeping uniformly through the rails, the E.M.F.'s at the positive ends of a bond are balanced; and in the case of one end of a bond losing its contact the additional resistance would be greatly in excess of the unbalanced contact E.M.F.

The design of copper bond should be largely in reference to the permanency of the contact surface. If there is any working between the surfaces, sooner or later there will be a film of oxide, so that the value of the contact will be destroyed. The working of the surfaces may be caused by heating from excessive current-density, or by lack of flexibility in the bond. Numerous types have been forthcoming. Many of the bonds brought forward during the last two or three years have been designed with a recognition of the importance of greatly increasing the area of the contact surface, as compared with the cross section of the body of the bond itself.

It is beyond the scope of this paper to discuss all the different types of bonds that have been brought forward from time to time. Samples of many of the different types are exhibited. The copper bonds that the writer has tested, since they have been more generally used in this country, are either of the "Chicago," "Crown," or "Columbia" type, samples of which are before you.

Flexible bonds are found desirable for use where the mechanical conditions are such that short bonds can be used, in which case the added resistance of the bonds to the track can be made as low as 5 per cent., or less. Bonds of this type have been frequently used in the United States, and with good results when

Mr. Parshall. the ends are made of drop forged copper. When, however, the ends have been made of cast copper, and cast on to the conductors, the results are not generally satisfactory. The resistance of cast copper is so much greater than that of drawn copper that it is not best suited for use in bonds. Further, the union between cast copper and drawn copper wires is imperfect, so that the electrical resistance is much higher than between two pieces of pure copper fused together.

The remaining type of bond that I propose to discuss is that known as the "plastic" bond, which was invented by Mr. Edison several years ago. From the results obtained from a line bonded over five years ago, it appears that this plastic alloy, which consists of mercury and other ingredients, as to the nature of which I am uninformed, is much more permanent than might be expected from its mechanical nature. The bond is placed between the fish-plate and the rail, in a cork receptacle, which is compressed to about half its thickness when the fish-plate is drawn up tightly.

The amount of copper required materially to increase the conductivity of well-bonded rails is so great that, in ordinary practice, auxiliary track feeders are not commercially practicable, unless they be connected in circuit with a source of E.M.F. to compensate for the drop in the feeder, so that this may exceed that in the track return.

I believe Major Cardew was the first to suggest employing E.M.F.'s in feeders to compensate for the drop therein. In the arrangement, however, of the earth return as originally devised by him, it was necessary to use generators of different E.M.F.'s in the generating station. I have used in my work a generator that is separately excited through a coil in series with the trolley feeder, so that the voltage generated by the armature is directly proportional to the current-output, provided the field magnet be not saturated. The armature is in series with an insulated feeder connected with the rail at whatever point it is necessary to take off current. The results in practice are most satisfactory. It has been found that the machine works perfectly automatically, and limits the voltage drop in the earth return to any desired amount by an adjustment of a rheostat in parallel with the field-magnet coil. Fig. 1 gives a diagrammatic representation of the system.

In a system that I have recently designed to carry some 250 cars, I propose to employ several earth generators feeding in from several points in the system. Pairs of test wires are run back to the station from various points, one of the test wires being connected to the track return, and the other to adjacent earth plates. The earth generators in the station will be adjusted from time to time, according to the difference of potential between the earth plates and the earth return. As far as possible the adjustments will be made so that the two may be kept generally over the system at the same voltage. Whatever Mr. Parshall.

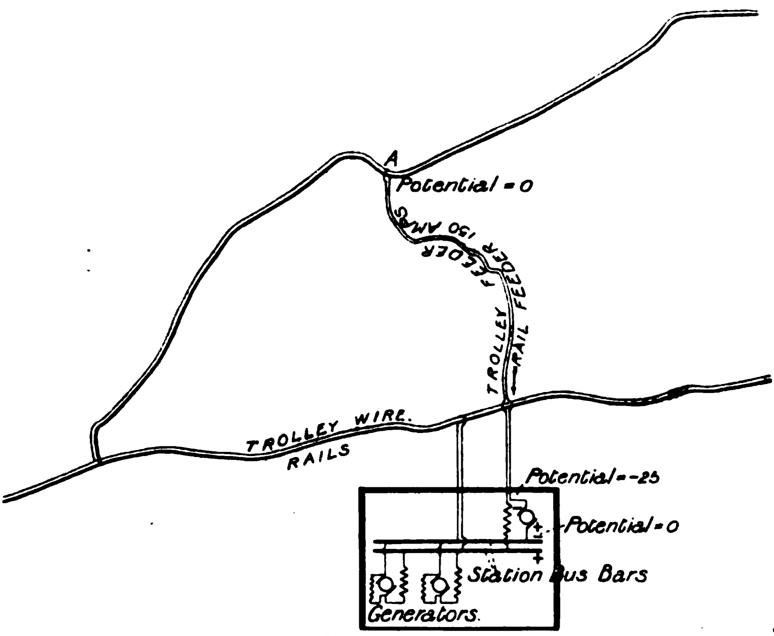


Fig. 1.—Return Booster System.

difference of potential there may be between the two, will be such that the earth return is, in general, positive to the neighbouring water or other pipes, since in this case whatever electrolysis takes place will be in the track return itself.

Steel Rails.

The percentages of carbon, manganese, &c., in steel rails have varied considerably at different times; and there are, even now, wide variations in the practice of different companies, and in different countries.

Mr. Parshall. It may be said that English rails some years back would commonly contain the following:—

Carbon	0·25 to 0·35
Manganese...	0·8 „ 1·0
Silicon	0·05
Phosphorus	0·06
Sulphur	0·06

Of late years the percentage of carbon has increased. One large railway company specifies:—

Carbon	0·4 to 0·5
Manganese...	0·95 „ 0·85
Silicon	0·10 „ 0·06
Phosphorus	0·10 „ 0·08
Sulphur	0·08

In American practice the carbon runs still higher, as will be seen from the following:—

Carbon	0·45 to 0·55
Manganese...	0·8 „ 1·0
Silicon	0·10 „ 0·15
Phosphorus	0·06
Sulphur	0·06

In France yet higher percentages of carbon have been tried, running up to nearly 1 per cent.

The results are shown in the following table of trials, of sample sections of steel rail of varying compositions which were furnished for testing purposes:—

Carbon.	Manganese.	Silicon.	Phosphorus.	Sulphur.	Resistance compared with Copper 20° C.	Resistance of 1 Mile 1 sq. in. Sectional Area at 20° C.
0·378	0·550	0·181	0·040	0·041	10·8	0·468
0·446	0·568	0·188	0·046	0·044	11·1	0·482
0·536	0·592	0·201	0·051	0·059	11·3	0·490
0·568	0·608	0·204	0·053	0·061	11·4	0·495
0·588	0·632	0·214	0·056	0·065	11·5	0·499
0·610	0·650	0·220	0·062	0·071	12·9	0·560

Eight 76-lb. track rails, tested in place after 2½ years' use, gave the following results:— Mr. Parshall.

	Resistance compared with Copper 20° C.	Resistance of 1 Mile 1 sq. in. Sectional Area at 20° C.
Test No. 1	11·3 ...	0·490
" " 2	10·3 ...	0·447
" " 3	10·1 ...	0·438
" " 4	10·7 ...	0·464
" " 5	9·65 ...	0·419
" " 6	10·07 ...	0·437
" " 7	10·25 ..	0·445
" " 8	10·50 ...	0·455
Average ...	10·4 ...	0·45

Two old 65-lb. rails, much worn, tested in place:—

	Resistance compared with Copper 20° C.	Resistance of 1 Mile 1 sq. in. Sectional Area at 20° C.
Test No. 1	11·7 ...	0·508
" " 2	12·3 ...	0·534
Average ...	12·0 ...	0·52

High values would be expected owing to the wearing of the rail, which is not allowed for in the calculations.

Two new 90-lb. rails, tested in place:—

	Resistance compared with Copper 20° C.	Resistance of 1 Mile 1 sq. in. Sectional Area at 20°C.
Test No. 1	10·6 ...	0·460
" " 2	10·4 ...	0·451
Average ...	10·5 ...	0·455

A 66½-lb. rail not laid:—

	10·0 ...	0·434

Bonds.

The current flows across the joints partly through the fish-plates, and partly through the bonds. The resistance of the fish-plates is a variable quantity, but all tests on rails in use have shown that they contribute considerably to the conductivity of the joint.

For the bonds themselves the following tests have been made:—

Mr. Parshall.

(1) Conductivity tests on bond copper.

(2) Resistance due to contacts.

(3) Resistance due to current "gathering" from other sections of rail to enter the bond terminal.

1. *For Conductivity* the Chicago bonds in the different tests have shown practically 100 per cent. of the conductivity of pure copper. A flexible Crown bond showed only 93 per cent. conductivity. The Columbia bonds in the cases tried showed about 90 per cent. conductivity.

2. *Resistance due to Contacts.*—Measured from the potential difference between two points very close together, one on the bond terminal, the other on the steel. Experiment showed the following results:—

——	Test.	Resistance per Bond (2 Terminals).	Resistance of 176 Joints, or per Mile with 30-ft. Rails.	——
		Ohms.	Ohms.	
Chicago Bonds ... ⅞-in. terminals in ½-in. web. 1·37 sq. in. contact area.	1	0·00000197	0·000347	Bond and hole very clean.
,, ,, ,,	2	0·00000215	0·000379	,, ,,
,, ,, ,, ,,	3	0·0000025	0·000440	Bond not cleaned; hole freshly reamed, but oily.
,, ,, ...	4	0·0000080	0·00141	Bonding not supervised.
Crown Bonds ... ... ⅞-in. terminals in $\frac{7}{16}$-in. web. 1·2 sq. in. contact area.	5	0·0000080 0·0000028		,, ,,
Total ...		0·0000108	0·00190	
Crown Flexible Bond... ⅞-in. terminals in $\frac{7}{16}$-in. web. 1·2 sq. in. contact area.	6	0·0000422 0·0000518		Bonding not supervised; bonds afterwards found to have been put in rusty hole.
Total ...		0·0000940	0·0165	
Columbia Bond ... In ⅞-in. hole in ½-in. web. 1·37 sq. in. contact area.	10	0·0000072	0·00127	Hole clean; bond untouched.
,, ,, ,,	12	0·0000095	0·00167	,, ,,
,, ,, ,,	13	0·0000077	0·00136	Hole 4 days old; bond untouched

Tests 4, 5, and 6 show that want of care in bonding may lead Mr. Parshall. to serious increase in contact resistance.

From the tests made it may be said generally that bonds properly applied—that is, clean bonds in bright reamed holes, put in with a proper fit with a drift driven square—have practically negligible contact resistance. Experiments showed that, at 100 amperes per square inch at least, the drop in the contact surface was inappreciable compared with that in the bond and in the rail. The same was found true with bonds—samples of which are exhibited—that have been in use for over two years, when the current-density has been limited as stated. Experiments on this point have been carried out to a considerable extent, since it has been frequently stated that the contact resistance is a very appreciable factor, and that it can be greatly lessened by amalgamating the surfaces. This will not be the case except when there is carelessness in putting the bonds in place.

3. *Gathering.*—The current may be supposed to flow uniformly through the rail at all parts, a foot or so from the ends or from bonds. At a bond, however, it has to gather, and it is scarcely to be expected that, say, 16 inches of rail terminating at a bond, should show the same resistance as 16 inches in the middle of the rail.

Tests on a bar of steel 3 inches × ½ inch showed "gathering" at the two bond terminals added resistance equivalent to a total of about 1 inch of the bars.

Tests on an 83-lb. rail showed "gathering" resistance equivalent to 3·4 inches of rail at each contact, or a total of 6·8 inches per joint.

JOINTS.

The conductance of the joints depends, as stated, on both bonds and fish-plates.

The first have been discussed already.

The second have a very appreciable effect, even with rails that have been in use for some time.

The following table shows the results of a number of tests made partly in the laboratory and partly on track in use :—

Mr. Parshall.

Laboratory Tests.	Additional Resistance due to Joint.		
	Ohms.	Inches of Rail.	Resistance of 176 Joints per Mile, or with 30-ft. Rails.
83-lb. rail; six tests; no bonds, fish-plates uncleaned, and not fully tight	0 0000095 to 0·000081	10 to 87	0·0017 to 0·0143
Average	0·000039	34	0·0068
Single 0000; 30-in. bond only (calculated)	0·000101	109	0·0178
83-lb. rail, with one 30-in. Crown 0000 bond, plates well tightened	0·0000024	3	0·00041
Same with fish-plate removed	0·000106	114	0·0187
This bond had too great contact resistance. See Contact Test No. 5.			
Tests on Rails in Use.			
76-lb. rail; one 30-in. 0000 Chicago bond and fish-plates	0·0000307 to 0·0000622	32 to 65	0·0054 to 0·011
Four tests made without disturbing track, *average*	0·000043	45	0·0076
76-lb. rail as above (track 2½ years old); four tests	0·0000275 to 0·0000843	28 to 80	0·0048 to 0·0148
Average	0·000046	48	0·0081
Single 30-in. 0000 Chicago bond only (calculated)	0·000103	114	0·0181
Old 65-lb. rail; one 30-in. 0000 Chicago bond, fish-plates not tight	0·000069	57	0·0121
Above with fish-plates removed	0·000090	74	0·0158
Above with fish-plates replaced and well tightened ...	0·0000473	39	0·0083
New 90-lb.; two 32-in. 000 Chicago bonds and plastic to one fish-plate	0·0000081	10	0·0143
	0·0000040	5	0·0071
Average	0·0000060	7½	0·0105
Fish-plate added to conductivity.			

The above values show that the contacts had not deteriorated in any way in the two and a half years of use. Some of the rails were very old, but the fish-plates, which were not fully tight, showed bright patches of metal at places of contact with rail. On replacing plate and re-bonding, the joint was equivalent to 39 inches of rail. Mr. Parshall.

A second rail, tested without fish-plate, showed also *no* deterioration of the bonding.

Some 66½-lb. rail laid on another line recently bonded, showed joint resistances equivalent to 9½ inches to 28 inches in four different cases.

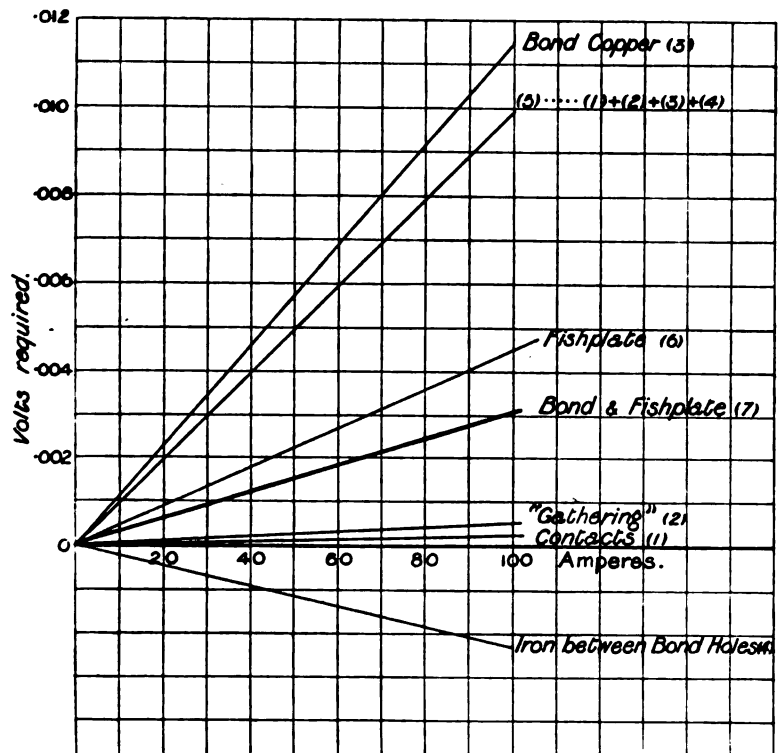


FIG. 2.

Volts required by various elements of joint in 80-lb. rail bonded with a single 30-in. 0000 copper bond, with ⅞-in. terminal in $\frac{7}{16}$-in. web.

Mr. Parshall.

PLASTIC BONDS.

1¼-inch hole in the cork receptacle between fish-plate and rail filled with plastic material.

——	Increased Resistance due to Joint.	Inches of Rail.	Increased Resistance of 176 Joints, or per Mile with 30-ft. Rails.
	Ohms.		
83-lb. rail bonded to one plate only; both plates separated by paper from rail ...	0·0000213	24	0·00375
Do., but bonded to both fish-plates; plates not very tight	0·0000126	14	0·00222
Do.; plates a little tighter	0·0000123	14	0·00217
Do.; plates very tight; brown paper still between plates and rails	0·0000117	13	0·00206
Do.; brown paper removed; plates tightened very hard up	0·0000083	9	0·00146

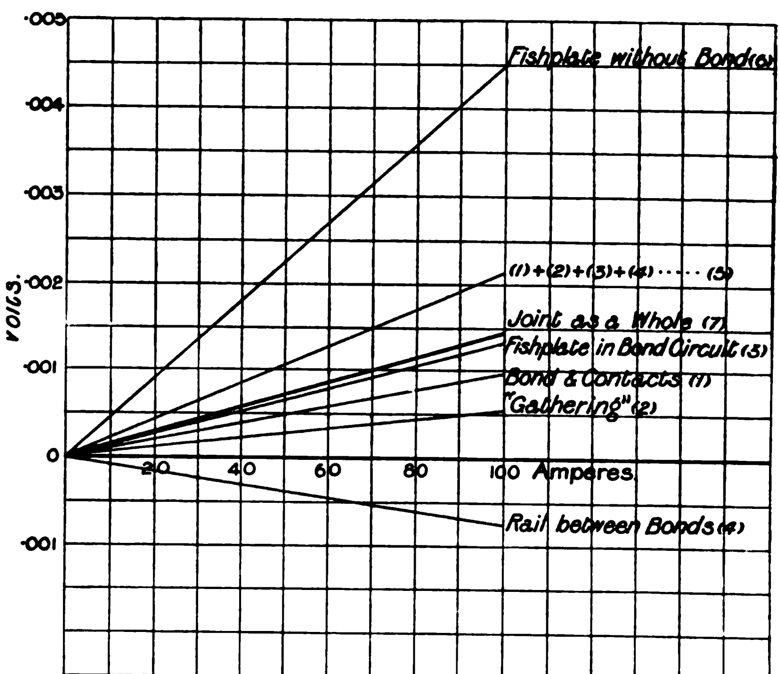


FIG. 3.

Volts required by various elements of joint in 80-lb. rail bonded with 1¼-in. plastic bond to one fish-plate only.

From the above, it seems safe to take the resistance through fish-plates as equivalent to some extra 50 inches of rail, and to take this resistance as in parallel with the copper or plastic bonds used in addition. Curves can then be constructed for any particular system of bonding similar to those of Fig. 2, which gives P.D. for the various elements of a joint of 80-lb. rail bonded with a single 0000 B. & S. copper bond 30 inches long with 7/8-inch terminals. Mr. Parshall.

The contact and gathering resistances are added to the bond copper resistance, and the resistance of the iron between the bond holes deducted. This gives Curve No. 5. The resistance so found is taken as in parallel with the fish-plates' resistance and curve (7) calculated for the whole joint. The volts so found must be multiplied by the number of joints per mile, and added to the volts required to drive the current through a mile of jointless rail.

APPARATUS EMPLOYED IN TESTING.

All resistances were found by measuring the potential difference between two points on the rails when a constant current of 30–150 amperes was passed through the latter. A standard resistance of 0·0000398 ohm was placed in the same circuit, and the fall of the potential across this compared with that across the two points on the rail. The places at which current was led in and out of the rail were always at some distance from the points between which the potential difference was taken. Where measurements were made upon the actual track, current was supplied from an accumulator placed upon a car brought up to the spot. Current was led from this to a point in the middle of the rail to be tested, and was led out some 5 or 6 feet on the other side of a rail joint. The fall of potential was then measured between two points inside those by which the current was led into the rail, and also between two points on the same rail outside the places at which current was led into it. The standard resistance was included in the circuit, and comparisons taken with this at each stage. From these two measurements the resistance of the rail could be calculated as long as no cross

Mr. Parshall. bonds occurred upon the part of the track actually under test. To measure the resistance of the joints, a joint was included between the two points between which the potential difference was taken; and this was compared with the potential difference between two points at a similar distance apart on the continuous rail. It was found extremely important in some cases to reverse the current both in the rail and the potentiometer, since with the small potential difference measured, thermo-electric effects were very liable to disturb the results.

In certain experiments a current was passed into the rails at one end of the track, and taken out at the other. The current in the rails at intermediate points could be measured by taking the difference of potential between two points on the same metals which had been tested for resistance as above This had, of course, to be done for all four lines of the double track. The volts used to drive current through the whole length of track were measured by making use of the test wires. The potentiometer was employed for this purpose also, and the results may be taken as correct, within the limits of correctness of calibration of the instrument itself, which was supplied by Elliott Brothers.

NOTES ON ELECTRIC TRAMWAYS.

By Major P. Cardew, R.E., and A. P. Trotter, Members.

Major Cardew. The accompanying note, on return feeders for electric Tramways has been forwarded to me by Mr. A. P. Trotter; and, as it contains a neat graphical method for determining the fall of potential in the return with uniform distribution of current, and the proper points of application of return feeders, I think it may prove interesting in connection with Mr. Parshall's paper.

As Mr. Trotter alludes to previous suggestions of my own on this subject, I also forward a note which was prepared by me in May, 1894, and sent to the South Staffordshire Tramways Co., advocating the automatic regulation of this fall of potential.

P. CARDEW.

NOTE ON RETURN FEEDERS FOR ELECTRIC TRAMWAYS.

By *A. P. Trotter*, Member.

While great ingenuity has been expended in designing bonds for electric tramway rails, and while these bonds, assisted in some cases by bare copper conductors laid between or near the rails, form a considerable item in the cost of building a line, little attention has been paid to the use of return feeders. The use of return conductors provided with a small dynamo was suggested by Major P. Cardew several years ago, and it has been independently proposed by Mr. G. Kapp. The system has been in use for some time in Geneva, and has recently been applied with success to the extension of the Bristol tramways. Mr. Trotter.

The best mode of arranging such return conductors does not appear to have been described, and the present communication is intended to afford an opportunity for discussing it.

Assume a tramway line with passing places, five miles long, and ten cars running. The most even distribution will of course be when they are equidistant, and a less even distribution is not likely to occur than when all the cars are in pairs at passing places. Let each car take 20 amperes, and let the resistance of the bonded rails be 1/20 ohm per mile. When the cars are evenly distributed, half a mile apart, the rail resistance between each pair is 1/40 ohm, and with 20 amperes the drop on half a mile of rails is ½ volt.

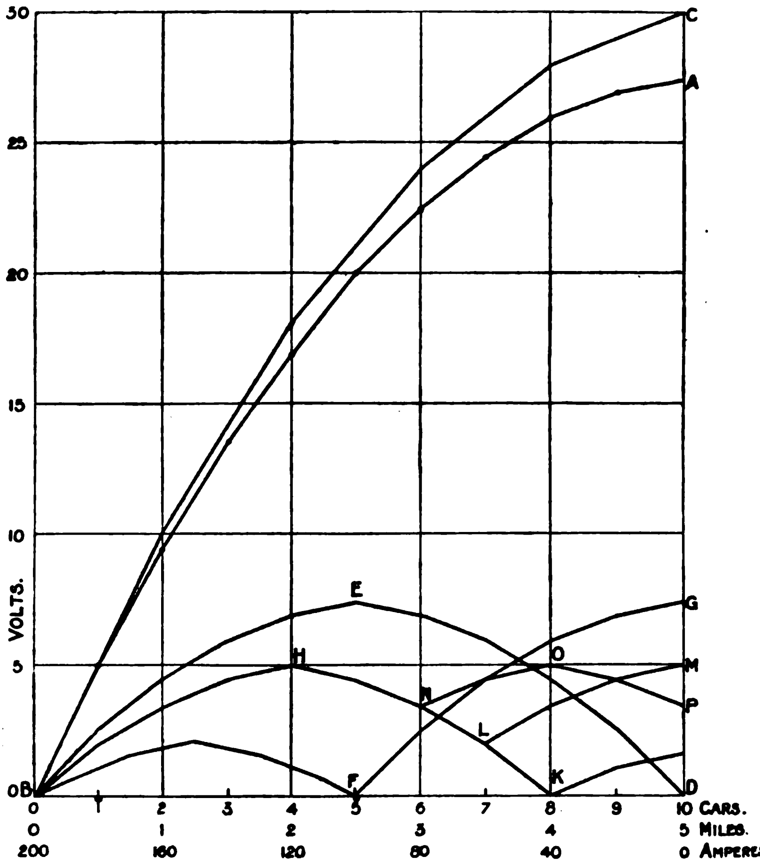
The series is as follows :—

Cars ...	1	2	3	4	5	6	7	8	9	10	Works.
Volts ..	0	½	1½	3	5	7½	10½	14	18	22½	27½

The first car is supposed to be at the extreme end of the line. The case is an extreme, but not an imaginary, one. The large total fall of 27½ volts over five miles should of course be reduced in the first instance by more ample bonding, but the example serves the better to illustrate the problem.

When the cars are all passing, in pairs, at a mile apart, the drop due to 40 amperes over one mile is 2 volts. The diagram shows the distribution for these two cases; the line A B showing the fall of volts for 10 cars evenly spaced half a mile apart, and the

Mr. Trotter. curve C B the fall for cars in pairs a mile apart. Mathematically, the point A is the origin of the curve to which the line A B is an approximation; but, as it is not intended to treat the problem mathematically, the point A is for convenience placed at the top right-hand corner.



The volts in the two cases differ so little, compared with the fluctuations of energy on an electric tramway, that this question of distribution of the cars will not be referred to again, but the line A B will be considered as typical.

The return feeder method by which this fall of volts may be reduced, consists in connecting a feeder to some point on the rails, and tapping off some of the return current. The conduc-

tivity of the feeder is not relied upon for this, but a dynamo, acting as a negative "booster," may be said to suck the current back. By this means the point at which the feeder taps the rails may be brought down to zero-potential, or might be made negative to the generating dynamo. Mr. Trotter.

The problem to be considered is—(*a*) To reduce the volts below a fixed maximum; (*b*) to use as little copper as possible; and (*c*) to use as little energy as possible.

Disregarding the two latter conditions, a simple plan would be to run a feeder the whole length of the line, and to reduce the volts to zero at the far end of the line, D. The distribution is then symmetrical: half of the current goes to the generating dynamo, and half to the return feeder. To draw the curve of distribution, cut out a piece of card to the shape of the curve of volts A B, and, fitting the vertical axis to the ordinate 5, place it so that it passes through the point D. Turn the card over, and complete the curve through A in the same way. The maximum volts, at the point E, are 7½.

But there is no occasion to reduce the volts at the end of the line to zero, and there is evidently a maximum expenditure of copper and of energy in the feeder. The middle point of the line is evidently not the best point to tap, for the volts would be distributed as shown by the line B F G, which may be easily drawn by means of the template. Here the maximum is, as before, 7½, and the volts near the works are unnecessarily low, viz., 2 volts at 1¼ miles out. It is clear from the line B F G that the feeder would draw off three-fourths of the total current. It would be still worse to tap the rails at the point at which the volts rise to one-half the maximum, viz., at about 1½ miles from the works.

Starting now in a different manner, let it be given that the maximum volts are not under ordinary circumstances to exceed 5, allowing a margin of two below the Board of Trade limit. Draw the line B H by means of the template, and fitting the template so that its axis is vertical, that the top touches the line of 5 volts at the point H, and that it passes through the point B. Turn it over and draw the line H K. But as it is not

Mr. Trotter. necessary, from the "undertakers'" point of view, to reduce the volts to zero at the point K, set the template again, allowing 5 volts at the end of the rails at the point M, and, drawing the line backwards, it is found to intersect the line H K at L. The volts at this point are 2, and this is the best that can be done with a single return feeder. This feeder will be 3½ miles long, and will draw off 0·65 of the current.

NOTE ON ELECTRIC TRAMWAYS.

By *Major P. Cardew, R.E.*, Member.

Major Cardew. It is, I believe, generally admitted that where the rails are used for the collection and partial transmission of the return current, the best means of preventing injurious action on pipes is to minimise the difference produced by the current between the potential of the uninsulated return at different points, and between any part of such return and the earth. On account of the resistance offered by all conductors to the current, the transmission of a current by means of a conductor causes a fall of potential throughout the length of the conductor, the difference of potential being greatest between the ends of the conductor.

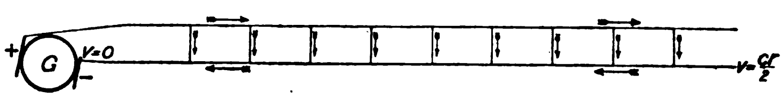


Fig. 1.

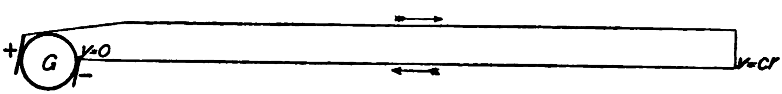


Fig. 2.

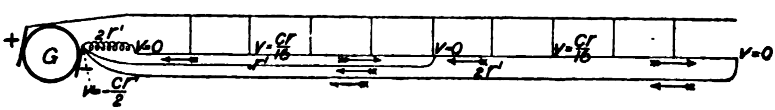


Fig. 3.

Note.—In the above figures *v* indicates potential with regard to earth.

This is the case whether the whole current is transmitted throughout the length of the conductor, or is fed

Major Cardew.

in (as in the case of a tramway line) at different points along the length, provided that the direction of the current throughout the length of the conductor is the same, which must be the case when this conductor forms the only path for the current back to the generating machine.

But if additional conductors are used to take current from the main conductor, which receives the current distributed along its length back to the generator, the greatest difference of potential in this main conductor may no longer exist between the ends of the conductor, and the amount of this difference may be greatly reduced. The extent of the reduction will depend upon the position of the junctions effected and the resistance in the auxiliary conductors.

If we assume, for example, n auxiliary conductors, all of equal resistance, connected to the main conductor at equal distances throughout its length, and one from the extreme end of the main conductor of twice the resistance of the others, a resistance equal to this last being interposed between the generator and the near end of the main conductor, then with a uniform distribution of current all the points of junction will be at the same potential, and the extreme difference of potential between any points of the main conductor will be reduced to $\frac{1}{4\,(n+1)^2}$ of what it would be without these auxiliary conductors or feeders.

Thus with one feeder to the distant end alone the fall of potential in the main conductor can be reduced to one-fourth, and with a feeder to the centre as well, to 1-16th, of that due to the same distributed current without feeders; and it will be seen that under such conditions the variation of potential in the main conductor can be reduced to any required limit.

But, unless these feeders are of very large cross section and conductivity compared with that of the main conductor, there will still be a considerable fall of potential in them, and in consequence a considerable difference of

Major Cardew.

potential between the main conductor and the terminal of the generator to which it is connected by means of the feeders.

In place of adjusting the resistances of all feeders to equality, varying E.M.F.'s may be introduced into each feeder, proportionate to its resistance, and thus the potential of all feeding points may be kept the same as that of the terminal of the generator, if desired.

In considering the application of the feeding arrangements described above to the special case of minimising the leakage to earth from the rails of a tramway used as a return circuit, it must be borne in mind that, although the load under normal conditions may be fairly uniformly distributed, yet the exigencies of traffic may require far more current to be supplied to one section of the line than its proper share, other sections at the same time being lightly loaded.

The position and slope of the various gradients on the line also considerably affect the distribution of current in the rails. The number of cars at work, and therefore the total load, also generally varies during each day's running, and from day to day.

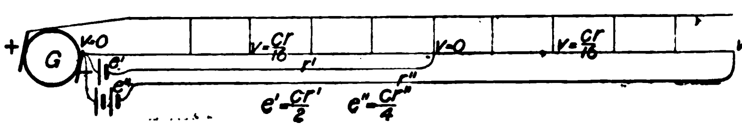


FIG. 4.

NOTE—v indicates potential with regard to earth; e' and e'', auxiliary E.M.F.'s.

The disposition shown in Fig. 4 can be adapted to meet the special requirements; but unless the auxiliary E.M.F.'s are continually adjusted to the variations of load, both as regards amount and distribution, the arrangement must be defective at times.

In order to provide auxiliary E.M.F.'s for the efficient working of the feeders to the return, automatically adjusted to the requirements, I would suggest the following arrangement:—

Let the tramway be divided into several sections

according to its length and the amount of traffic gradients, &c. Major Cardew.

Let there be two insulated feeders for each such section—one for the line, and one for the return; the latter being connected to an uninsulated conductor as provided in Board of Trade Regulation 4.

Let the currents in these feeders pass through a "motor generator" at the generating station, the "field "magnets" of which are excited by the current to line alone, while the armature is wound with two circuits—one for each current, so as to oppose each other—the circuit through which the current to line passes being made slightly the more powerful. The motor generator will then revolve as urged by the line current, and will generate an auxiliary E.M.F. for the return current.

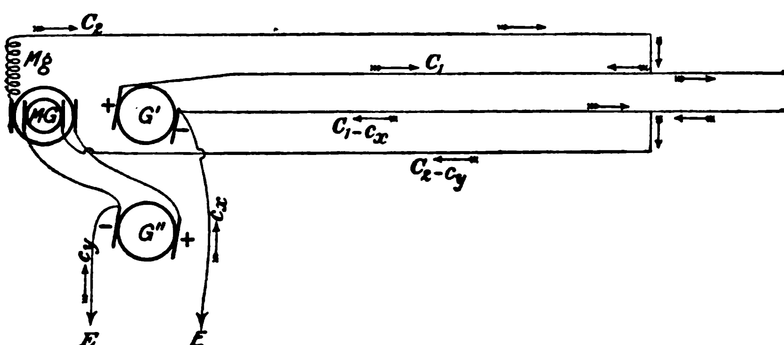


FIG. 5.

G′ = generator; G″ = auxiliary generator; MG = motor generator; Mg = magnetising coils of motor generator.

The generator for each feeding circuit should be of rather higher E.M.F. than that supplying the near end of the line and return; but, as the extra volts will be taken by the motor generator in the station, there will be no need to exceed the limit allowed by the Board of Trade on the line outside.

The expense involved may probably prevent the adoption of any such system in its entirety at present (in 1894), but it possesses the advantage that it can be adapted to

Major Cardew. existing tramways, and a pair of feeders run to any part where the difference of potential from earth of the rails is found to be excessive.

Such an arrangement with one pair of feeders is sketched in Fig. 5.

The PRESIDENT: We will now proceed to discuss these three papers in common. It seems unnecessary to separate them in any way.

Mr. Gadsby. Mr. C. H. GADSBY: I should like to make one or two remarks upon Mr. Parshall's paper. In the first place, he has mentioned the use of lead-sheathed cables for tramway purposes. As he has pointed out, there have been serious difficulties with such cables, and he has mentioned a method of overcoming them by bonding the armouring and the sheathing to the rails. I have had some experience with lead-sheathed cables in connection with a conduit system, and I quite endorse Mr. Parshall's remarks; they are very unsatisfactory indeed. In many instances I have found holes as large as a shilling eaten out of the lead sheathing of the cables, evidently by the return currents. This was in a tropical climate, where the portions were dry for a great part of the year; but when the rains came on, and it became very wet, all these places broke down, and there was a great deal of trouble. Mr. Parshall also mentioned a method of cast-welding tramway joints. I am quite of the opinion that this method will be the method adopted for nearly all tramway work during the next few years. He has mentioned the difficulty of forming a weld between the cast iron for these joints and the rail. I think there should be no difficulty in making such a weld, if a head is put on the mould and the molten metal is run through and allowed to run out by means of a runner on the other side, in the same way that one would do in burning a piece of cast iron on a casting. But I think it is a mistake to try to get this weld on; I think it is better just to run the metal in in the ordinary way, and allow it to take its natural contraction and to have a small amount of slack in the rail joint. The slack is so small that the mechanical advantages for the support of the joints are obtained; but of

course there is only poor electrical conductivity, and therefore it is always advisable to bond over even a cast-welded joint. Mr. Parshall has mentioned in connection with this joint that he thinks there might be a difficulty in consequence of the variable resilience of the joint and the body of the rail; but I think perhaps Mr. Parshall has in his mind more particularly the American tracks, which are nearly all laid on white oak sleepers and broken stone, instead of being laid on concrete, as nearly all the tramways in this country are; so that I do not think there is likely to be very much difficulty in that respect. I might perhaps mention that I recently designed a rail to get over some of these difficulties which Mr. Parshall has set forth in the paper, with a head separate from the body of the rail. The head is made somewhat of the same form as the old-fashioned tramway rails that were laid on longitudinal wood sleepers, and is bolted upon the body of the rail, which is made in the form of an I-shaped joist of iron. The advantage of this is that the part of the rail which is worn more rapidly than any other portion can be renewed at any time without removing the paving of the tramway. It also has the advantage that the head of the rail can be made of steel with a high percentage of carbon to resist the wear of the wheels, whilst the joists underneath can be made of low-carbon steel, or iron with a much higher conductivity. Another advantage is that the joints in the head of the rail and the joints in the joists underneath can be laid alternately—that is to say, the joint in the head can be made in the middle of the portion of the joist underneath—so that a more or less continuous rail is obtained. This, of course, would also improve the conductivity, as the head would bond the joists, and the joists would bond the heads. Mr. Parshall has explained the apparatus and methods used for measuring the resistances of the joints. In the case of measuring the resistances of joints and rails in position, he has said resistances have been measured by taking the potential difference across a given length with a constant current and comparing it with the corresponding potential difference against a standard resistance. I should like to ask if those rails in position were laid as in ordinary Mr. Gadsby.

Mr. Gadsby. circumstances, so that a portion of the current would pass through the earth between the two points of contact, or whether means were taken to secure the whole of the current going through the body of the rail.

Referring now to Major Cardew's paper: The author has described in his paper a method by which a seperate generator with a different voltage would have to be used for each feeding circuit; but of course this was a method that he proposed some years ago, and I do not suppose he would advocate it now, as on some of the complicated tramway systems of to-day I think it would be quite impracticable. He has described another method by which the difficulty of the drop in the return can be avoided by means of feeders of varying resistances; and in Fig. 3 of his paper, on page 460, I see there is an arrangement by which a resistance is inserted between the negative terminal of the generator and the near end of the return circuit. It seems to me that there are some cases in actual practice which correspond very closely to this, and cases which the Board of Trade do not appear to have anticipated. I know a tramway system where the tests taken as set forth by the Board of Trade regulations show the drop in the return circuit—it is a tramway of three miles in length—to be not more than about 1 volt; but the leakage from the whole of the tramway rails to the earth plate, through the Board of Trade instruments, shows, I think, something like 10 per cent. of the total current, which is very much outside of the Board of Trade limits. The explanation of this is—although I do not know that it has been recognised by the people on the tramway—that the generating station is about 300 or 400 yards away from the line, and that there is a copper insulated main from the rails to the negative 'bus-bar of the same section as the outgoing main, it apparently having been thought that a copper cable which was sufficient to take the current out to the line was also sufficient to bring it back again. But it is quite evident that such is not the case; because in this instance, upon calculating the drop on this return cable, I found that under ordinary working circumstances there are 5 or 6 volts drop between the tramway rails and the generator. The result

is that the whole of the rail-system is generally some 4 or 5 volts above the ordinary potential of the earth, and that there is always a leakage from those rails to the earth-plate, which is connected to the negative terminal of the generator. Mr. Gadsby.

Mr. W. MURPHY: I do not profess to be at all an expert on these matters. I know very little about electrical affairs, but I should like to say one word to confirm what the gentleman who last spoke said about the question of the construction of the joint. No doubt the use of the Falk joint is likely to come into operation. It seems to be very perfect as regards absence of jar and the absence of wear at the joint; but I agree that it is not sufficient for electric conductivity, and that you want, in addition, a bond of some kind. I would also like to say, with regard to the system of rail which the last speaker has advocated, that I have had some experience of working a rail of a similar kind, where the head is distinct from the body of the rail; and I must say that it has not been at all successful. What happened was this: At the end of the rail the head sets up a slight motion, no matter how tightly you pin it down; and when that motion is once set up, the head cuts into the rail underneath, and it is almost impossible to keep it tight. My experience in the construction of tramways is not chiefly in connection with electricity, and therefore I did not think I should be called upon to speak, so that I shall not offer any criticisms upon the papers. As far as I can gather, they are highly instructive, and contain information of a kind very valuable to people, like myself, who are engaged not only in the construction, but in the finance of tramways, and who want to get them made in the best possible way. They give evidence of a disposition to take great trouble to find out the best ways to get over very difficult problems, which have not, perhaps, hitherto been solved in the way one would desire. I think the suggestions made in both those papers are extremely valuable to those who wish to prevent electric leakage into the earth, causing damage to their neighbours' pipes. Mr. Murphy.

Mr. A. W. HEAVISIDE: May I be allowed to make a few remarks upon an experience of the universality of the earth not being a thing to be reckoned without? In 1890 I was aware of Mr. Heaviside

Mr. Heaviside. a leaky outer in a concentric cable, and therefore I took a triangle of wire, and connected the terminals of the triangle at one of its angles to a telephone. The triangle was 5 feet upon each side, and you could very conveniently hang it upon your shoulder with the telephone close to your ear. Travelling along with this triangle, listening to the vibrations produced by the leaking 2,000-volt alternating current, if you went into the neighbouring streets wherever there was a pipe or anything of a metalliferous nature, the direction of that pipe was very easily found. The distance from the primary would be, probably, in some cases, 300 or 400 yards, and sometimes on an ordinary horse tramway the rails themselves were found to be a portion of the earth return. Therefore it is very evident that unless the rails are insulated you will never get a perfect protection from earth return. Of course it is evident that by the use of such a device it would be possible to plot the extent of the leakage from electric tramway rails in any place, and thus judge of its importance.

Mr. Lawson. Mr. A. J. LAWSON: I should like to say a word about the Dover installation, to which Mr. Gadsby has referred, because the remarks he has made on that show that he, for one, does not recognise, as Mr. Heaviside has done, the effects of pipes in the subsoil. The leakage at Dover is not entirely due—it may be partially due—to the fact that the cable bringing the return current is not of very great section. But whatever section it has, I am not responsible for it. Tests, however, conclusively show that there is a very great leakage along the lines of the tramways from the rail to large water pipes which are within a few inches of the rails, practically throughout the whole length of the tramway. Those water pipes, which are connected across to various premises, and are laid through various soils—chalk in one place, clay in another, shingle saturated with water at another point—all lead up to the generating station, and a considerable percentage of the current that is taken up into the generating station consequently passes through these various returns.

Prof. Perry. Professor JOHN PERRY, F.R.S. [*communicated by Professor Ayrton*]: I am sorry to say that I am suffering from a cold, and

shall not be able to be present to congratulate the meeting on having heard the very excellent papers of Mr. Parshall, Major Cardew, and Mr. Trotter. Prof. Perry.

I think that the most important information in Mr. Parshall's paper consists in this—that well-made bonds do not appreciably increase in resistance with time, and that at no time do they considerably increase the resistance of the line.

But there is another startling bit of information, namely, that the exposed surface for leakage is 50,000 square feet per mile; so that, when Mr. Parshall tried, he found that 60 per cent. of the return current may come by the earth. This is startling and serious, and I think that its consequences ought to be very carefully considered by electrical engineers, in view of the fact that this is a thickly inhabited country, with many interests which may be affected by large earth currents. Again, in the interests of a company, I would say that, with the maximum drop of 7 volts allowed by the Board of Trade, this means that the rail as a return conductor must, at great expense, be made a very much better conductor than would be needed in an insulated return.

I wish to ask the members of the Institution to consider whether there ought not to be a rule to insulate all electrical conductors. I know that the Board of Trade rule now in force will prevent the excessive deterioration of gas and water pipes, which so alarmed people in America a few years ago; but such deterioration must always be going on, and I feel sure that in the long run the Legislature will intervene to compel all companies to use insulated returns. In New York and Boston the alteration has been insisted upon already. Now, whether overhead conductors are employed so that it will be necessary to use two trolley contacts, or in the easier case of conduit conductors, the extra expense is not excessive.

I have considerable pecuniary interest in electrical enterprise, and, even if I had not, I am not one of those who think that an electrical enterprise likely to be of great value to the community ought to be burdened with expensive restrictions because it happens to interfere slightly with scientific experiments at some college. On this point I venture to disagree with Professor Ayrton

Prof. Perry. a little, but, after all, it is only on a question of the degrees of importance of the college teaching interfered with, the amount of the interference, and the importance of the new electrical enterprise to the community. With all the remarks made by Professor Ayrton in the discussion on Mr. Trotter's paper (vol. xxvi., *Jour. Inst. E.E.*) I am in perfect agreement, and I am sure that those remarks ought to be very carefully considered by the members.

I have a special interest in the protection of magnetic observatories. I am not only a member of the Royal Society's committee which is in charge of the Kew Observatory, but I am one of the two members who have been asked to consider what effect will be produced upon its work by the working of electrical tramways. Now the results of the work of the Observatory at Washington have already been destroyed, and the Observatory at Toronto is useless. From observations made at Washington, we are satisfied that an electrical tramway worked in the ordinary American fashion, two miles long, three-quarters of a mile away from an observatory, will produce effects which are about one-half or three-quarters of the whole diurnal magnetic change which it is our main business to observe; whereas, if the line, not coming closer than three-quarters of a mile, has a branch on the other side of the observatory from the generating station, very much larger disturbances must be expected, much of the return current coming underneath the observatory itself. I think it probable that, if every part of a tramway line within two miles of such an observatory is provided with an insulated return, very little harm is likely to be produced.

The members of the Institution will notice that, although we may defend a laboratory from outside magnetic disturbances, we cannot defend an observatory like Kew, for it is the outside magnetic effects that we desire to observe.

Of all engineers, electrical engineers must be most interested in the furtherance of scientific investigation. Their industry was created, and is still greatly helped, and will probably be enormously developed, by the discoveries of workers in pure science, and I hope and believe that the members of the Institution will take a little trouble in the interests of a magnetic observatory.

Even the magnetograph records now being made are continuous for 42 years. If you interfere with Kew, you cannot remedy the evil by paying any sum of money in compensation; for, if the Observatory were removed to Dartmoor, the future observations would have no link with the past. We expect that great results will be arrived at by an examination of the records at some future time. We know of no other way at present of getting at the great secret—of which we really know no more at the present day than Gilbert knew three hundred years ago—what is the cause of the earth's magnetism? Prof. Perry.

I do not ask the Institution to interfere in behalf of any particular observatory. But I do ask the members carefully to consider the amount of harm which non-insulated returns are capable of doing; to say if it is beyond the powers of an electrical engineer—not the engineer of a mere pioneer line in a new country, but, rather, of lines in a thickly inhabited district, where there are many pipes and telegraph and telephone lines which are likely to be interfered with—to insulate his return conductor at a cost which will not seriously interfere with the new enterprise. As for my own views, I consider, with Prof. Ayrton, that in the long run a tramway company will find that it pays to insulate the return conductor, not only because there is less chance of a breakdown, but that the actual cost of a well-arranged system will be less.

By sending our electricity to the earth to return how it may, we are only imitating the early sanitary authorities of towns who cast their sewage into rivers. There are endless examples of this unnecessary interference with the general rights of the community, but they always end in the interference of the Legislature, and the stoppage of the nuisance at the expense of the thoughtless ones.

Professor AYRTON: Professor Perry has referred to my own views on the subject, which you all know more or less well. I had an opportunity of speaking on this subject at the reading of a previous paper of Mr. Trotter's last year. What I said then I repeat now. I am inclined to think that the use of an earth return is evidence of the conservative character of men. We used an uninsulated return for telegraph wires, for submarine cables, Prof. Ayrton.

Prof. Ayrton.

&c., and we had two continuous pieces of bare metal laid down for a railway train to run on; therefore it seemed natural that when electric traction was introduced we should bring back the current by those uninsulated pieces of metal. Nobody suggested doing anything of the kind for electric lighting. We not only do not use the earth, in the sense of two earth plates, as is done in telegraphy, but we do not even use the sheathing of our electric light cable as our return conductor, or as forming a part of it.

The whole of the papers to-night have been directed, more or less, to one end, namely, to keep the potential of the rails of a tramway at zero-potential. But why should they be at anything else than at zero-potential? Why should we electrically interfere with them at all? Why not use the rails as the railway companies use the rails for the train to run on, and, if you want to do something else, then use something else to do it with? It is a very old-fashioned idea that you must do everything by the same means. We used to have a hole in the roof of our houses to let out the smoke and let in the light and do the ventilation, but now we have abandoned this universal method; we differentiate. Why then not make up our minds to do the same thing in electric traction—use the rails for their proper purpose, have an insulated going and an insulated return, and then employ whatever drop of pressure in that return conductor that seems best, just as you do in the case of an electric lighting system?

I mentioned on the previous occasion that all the companies that had Bills before Parliament at the time at which I was speaking, for running trains electrically under London, had undertaken to insert in their Bills clauses for employing an absolutely insulated system. The City and West End Company, the Brompton and Piccadilly Circus Company, and the District Electric Company last year inserted clauses in their Bills undertaking that, whether a two-wire or three-wire system was used, whether a direct or an alternating current was employed—in fact, no matter what system was adopted—every bit in the system should be insulated to the satisfaction of the Science and Art Department and of the City and Guilds of London Institute; and, further, the right of inspection was granted, in order

that it might be seen in the future how far this undertaking was carried out. Prof. Ayrton.

Now the companies were willing to enter into such a compact, not merely for the interests of science, but because we were able to convince the engineers of the proposed lines that it was to the interest of the shareholders to have a system which was insulated throughout, which had, therefore, greater freedom from breakdowns, which required no special device for keeping the return conductor at zero-potential, or for preventing the potential of any part of it differing from zero by more than 7 volts, and which enabled the companies, in fact, to construct their lines in the best method from a purely engineering point of view. Therefore I cannot help re-echoing these remarks—which I have heard to-night for the first time—of Professor Perry's, and supporting his suggestion that those who are interested in the development of electric tramways and railways in our country should put their heads together and consider whether it is not possible to devise a system with a completely insulated return, which will not only prevent their disturbing other people, but which will be of real economical benefit to the railways themselves.

I know that I shall be met with the objection on the score of cost; but, curiously enough, nobody strongly urges that there is any great disadvantage in wasting the conductivity of the sheathing of an electric light cable. The sheathing, we are told, is put on to mechanically protect the core, and therefore an entirely distinct cable is employed to bring the current back again. For it is stated that the advantages gained by this method more than compensate for the loss arising from wasting the conductivity of the sheathing. Is it, therefore, wholly impossible that a similar consideration may not lead to a similar result as regards the iron rails on a tramway?

Before sitting down, I should like to ask Mr. Parshall a question about his diagrams which show graphically the results he has obtained from a most interesting series of measurements of the resistances of different parts of the electric coupling between two rails. I am not quite clear, regarding these diagrams,

Prof. Ayrton. whether the P.Ds. indicated are intended to represent what was found when all the electric connections were there *together*. For instance, there is the P.D. for bond-copper, for fish-plate, for bond and fish-plate, for "gathering" contacts, and so on. Does that mean that the bonding was complete—that a current of 100 amperes, for example, was sent from rail to rail when *completely* coupled together as in practice, and that when the potential wires were touched on the two sides of the bond a P.D. of 0·0115 volt was found, and that when they were touched on the two sides of the fish-plate a P.D. of 0·0045 volt was found, and so on? Does it mean, further, that a negative P.D. was found when the wires were put between the bond-holes? or are the diagrams intended to show what were the various P.Ds. respectively when there was only *one* path at a time for the current—when, for example, *only* a bond-copper connected the rails, and there was no fish-plate, then next *only* the fish-plate and no bond-copper or any other connection, and so on? Perhaps he would kindly answer this question in his reply.

Prof. Thompson. Professor SILVANUS THOMPSON: I was somewhat surprised, Mr. President, when, on reading these papers, I found there was no reference whatever to the vexed question of interference of earth return currents with the instruments that might be in laboratories, for that is certainly one of the pressing problems connected with the development of electrical traction. Here let me frankly say that I entirely differ from my friend Professor Ayrton in the magnitude which I attach to this particular question. I am entirely in agreement with him on one point, namely, the great advisability of maintaining continuous and undisturbed records of the magnetic elements in a standard laboratory. It is, unfortunately, true that, so far as Toronto is concerned, the magnetic elements could no longer be observed in that Observatory. The prevalence of earth currents from the tramways in Toronto has for the present absolutely destroyed the possibility of making the magnetic records there; but I understand that the Scientific Committee which has had the Toronto Observatory in hand has now discovered that, by the proper use of insulated return feeders and negative boosters to keep down the potentials at the requisite

points, and by attention to other matters of care and detail, it is quite possible to maintain intact the conditions that are necessary in an observatory, provided a tramway does not come within a nearer distance than 1½ miles. It is, therefore, clear that we can protect Kew Observatory from such disturbances if we do not allow electric tramways within 1½ miles of Kew Observatory to operate with earth returns. If for that small portion of the tramway which lies within 1½ miles of Kew we insist on some method employing absolute insulation of copper, going and returning, there will be no interference with the Kew Observatory. As for the laboratories where students are being taught, it seems to me to be a most excellent thing that they should have practice with electric tramways and their insulation, for it is much more important that they should know all about these things than that they should be treated as hot-house plants to be put into a conservatory. Prof. Thompson.

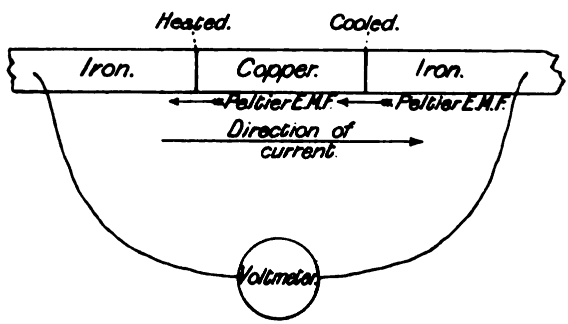
Having said that, I want to refer to one or two points that are in the papers. In the first place, there is no mention, I think, of the circumstance that many of the electrolytic troubles which have arisen, or which might arise, in not very well designed systems, could be eliminated by bonding to the line the pipes that are likely to be damaged at one particular point, namely, that which is nearest to the generating station; but that it would be disastrous to bond the pipes to the rails at almost any other point, because you are inviting a leakage flow out of pipes if you bond them to the rails anywhere else. It is the flow out of the pipes to earth which does the damage, not the flow into the pipes.

I would have liked to say something about the absolute inadvisability of using quicksilver in any permanent joint or bond.

One other point near the end of Mr. Parshall's paper is of some interest, and deserves a little attention. It is where he says that thermo-electric effects interfered with some of his measurements. Now suppose we had this experiment on hand. Let the accompanying figure be a piece of a conductor made up of iron for a certain part of its length, and then copper, and then iron again.

Prof. Thompson.

Now, if we are to pass a current through this, say from left to right, the current will produce a heating effect where it passes from iron to copper, and a cooling effect where it passes from copper to iron. The very heat that occurs at the first joint sets up a back E.M.F.; the cooling at the second joint also sets up an E.M.F. The two joints will both, when they are thus heated and cooled by the current, set up a back E.M.F., which will, for certain purposes, act like a polarisation. I suppose everybody knows that if you pass an electric current through a thermo-pile, one face is heated and the other cooled. From the thermo-pile you can always get a back E.M.F. during the time that it is coming back to its previous condition of temperature; as if it acted temporarily as an accumulator. Now, if these joints in the cross section are



equally well made, these two E.M.F.'s ought to be absolutely equal, and it ought not to matter which way you send your current in order to measure the apparent resistance. If you are sending a current through the system, and are employing some kind of voltmeter, tapping off a little current on the resistance to measure the fall of potential from one piece of iron to the other, it ought not to matter which way the current goes. If you reverse the current you also reverse these E.M.F.'s, and there will be still a back E.M.F. of thermo-polarisation; and, if everything else is equal, of precisely the same magnitude as before. Hence the apparent resistance ought to be alike in both instances. Mr. Parshall did not find it so. If you do not find it so, that, in itself, is evidence of something which is not mentioned,

namely, that those two joints are not equally well made; one or the other of them must be greater. If one of these joints is badly made, with a greater intersticial resistance there, then that joint will heat by ohmic heating, whether it is being heated or cooled by the Peltier effect. Prof. Thompson.

One other interesting scientific point is the question of "gathering" near the end of a rail. It is a well-known principle that the resistance of a conductor is a minimum when the current is distributed equally through the cross section. Hence, if there is "gathering" of the current in the rail toward the spot where the copper bond is inserted, there is certain to be an increase of resistance. But I was certainly astonished to learn that the "gathering" effect in the rail-ends, and the resistance of the copper bond, might together be so great that the resistance of the well-bonded joint was as great as 39 inches of good steel rail.

Lastly, let me thank the authors of the other papers, especially Major Cardew, for the extremely clear account that is given—I think the clearest that ever has been given—of this method of keeping down the rise—or undue drop, one ought rather to say—of potential by the use of properly designed return feeders and negative boosters. There are some of us who, years ago, were disposed to think that the Electric Lighting Act of 1882 was a deplorable event. We now know that the Electric Lighting Act of 1882 was a blessing in disguise, because it kept back the electric lighting industry from going ahead at a time when machinery was immature, when lamps were not perfected, and systems were in a very chaotic condition. It saved us from a great deal of abominably bad work. Equally so now, when electric traction is at last beginning to make a little headway among us, must we recognise how the Board of Trade rules on this very question of drop of voltage in the return, and so forth, have come as a blessing; and not this time in disguise, for it is found by the tramway electric traction contractors that it is quite possible to work well within the Board of Trade limits, and yet gain in economy thereby—in fact, that if the Board of Trade had chosen to draw those regulations even tighter, we need not have quarrelled with them on the score of economy.

Mr. Wordingham.

Mr. C. H. WORDINGHAM: I think these papers which we have had to-night are very opportune at the present time, because the question of bonding is exercising the minds of a good many of us; and where one has the responsibility of advising in connection with the bonding of a great many miles of rails, in the absence of any definite experience, it is very useful indeed to have so much exact practical information brought before us. The discussion to-night has wandered a good deal from the immediate subject of the papers, which was chiefly the question of earth returns and their practical operation, while the discussion has been principally concerned with a comparison between earth return and insulated return. I must say that, personally. when I first considered this question, I thought that the Board of Trade regulations were so onerous that it would be impossible to comply with them with large systems of tramway working, and that there would be no alternative but to insulate the return. On going into the question, however, the difficulties of insulating the return seemed to me so enormous that one was practically driven to use the overhead system with uninsulated return. Certainly the reasons for adopting that are not those suggested by Professor Ayrton, namely, trying to make the rails do everything. There are very many practical considerations, which it is not necessary to enter upon here, which affect the question. One very important thing is that these conduit systems require that the street in which the underground work is being done should be stopped for a month, or three weeks at the very least. In the case of an important business thoroughfare, that alone, apart from all scientific considerations, is quite sufficient to preclude the use of the conduit system; and I believe that wherever an insulated overhead return has been attempted it has resulted in failure. The question of observatories, which has been mentioned to-night, is no doubt a very important one, and I think that those responsible for designing tramway systems would be very grateful if they were told exactly what ought to be done to avoid disturbance. In the case of an Observatory like Kew, I think even so drastic a remedy as

prohibiting the tramway altogether would not be unreasonable; but in the case of colleges and technical schools, I really do not think one can do more than attempt to minimise the evils. [Professor THOMPSON: Why not ignore them?] One has to consider people's susceptibilities more or less, and I do not think that everybody agrees with Professor Thompson. I know in the case of Manchester we have had anxious deputations from Owens College begging us not to take the tramways down the streets near their laboratories — [Professor THOMPSON: I wish they would bring a tramway down the City Road]—and where these opinions are expressed, one is bound, I think, to give due weight to them. I should imagine that a good deal of disturbance might be introduced, quite apart from the earth currents, by the proximity of heavy currents in the conductors, and also by the motors on the car. There are one or two practical points upon which I should like to ask Mr. Parshall for a little information. I should like to ask, first of all, whether there is any objection to punching the holes in the rails instead of drilling them. That is an important point, because when ordering new rails it is convenient to have holes punched at the same time as those for the fish-plates. In the case of either punching or drilling some time before the rails have actually to be bonded—that is to say, when they may have to be in stock for several months—I should be glad if he would say how much difference he thinks ought to be allowed between the diameter of the hole originally punched and the final size after rimering. Mr. Wordingham.

[*Communicated.*]—Had time allowed, I intended referring more fully to the Board of Trade regulations respecting uninsulated returns. As I stated at the meeting, these appear to be onerous, but it cannot be asserted that they are unnecessary or unreasonable, and it is evident that they must be complied with. One method of doing this has been described in the papers discussed, and it is no doubt successful; I have myself seen the booster working at Bristol, and the effect was most marked. It appears to me, however, that such a system might become inconvenient and expensive when applied to an extensive tramway network with many cars and a frequent service. Another solution is to be

Mr. Wordingham. found in the employment of a number of stations feeding into the lines at frequent intervals. This is the method adopted on the Douglas, Laxey, and Snaefell line, in the Isle of Man, where some of the stations are generating stations, and others battery sub-stations. In a large city it is not always practicable to multiply generating stations; and in Manchester, where there are some 500 cars and 75 miles of track, with extremely dense traffic in parts, I have recommended the use of sub-stations placed about a mile apart, in which high-pressure current—probably three-phase, at a pressure of 5,000 volts—is to be transformed into continuous current at 400 volts. A similar system is in use on the Dublin tramways, and it appears to me that it offers very great advantages, especially where the traction and general supply are in the hands of one authority, as it is intended that they shall be in Manchester. With the system proposed no point will be more than half a mile from a sub-station, and there should be but little trouble with excessive drop in the return conductors of the tramways; and for the same reason regulation on the lighting and ordinary power network should be easy.

Mr. Wood. Mr. W. Wood [*telegram communicated*]: Sorry circumstances prevent me attending your meeting, but wish to express my high opinion of the efficiency of Mr. Parshall's booster. The difference in drop of potential is: Booster in circuit, 3 volts; booster cut out, 8 volts.

The President. The President: I am going to ask Mr. Parshall and Major Cardew to reply to the observations which have been made upon their paper, but before I do so I should like to say that I am glad that these papers have called forth strong expressions of opinion from Mr. Heaviside, from Professor Ayrton, and from Professor Thompson, on the advantage of insulating both the conducting lines. I do not know why the uninsulated rails should always be called an earth return. Mr. Parshall says he prefers to make the rails positive. So long as the common practice is employed of using the rails as conductors and not insulating them from earth, there cannot be a question as to the seriousness of the danger of destructive electrolytic action on the neighbouring water pipes and gas pipes. Therefore a full consideration of the danger, and of

the means for its prevention, more especially at the present comparatively early stage of electric railway construction in England, is extremely desirable; more than that, I think it is absolutely necessary. The President.

Mr. Parshall has mentioned the important fact that electrolytic action, such as we are considering, occurs at a very low potential difference. A volt, or even less, is quite sufficient to generate electrolytic action of a destructive kind; hence the value of such devices as those described in the paper of Major Cardew, and that of Mr. Trotter, which have for their object the diminution of potential difference in that part of the circuit formed by the rails to an almost negligible quantity. Mr. Parshall has said: "In general it is desirable that the earth "return be insulated to the greatest degree from any other "metallic conductors liable to be affected by electrolysis. In "some cases, however, where the drop in the earth return "has been comparatively great, attempts have been made to "prevent electrolysis by bonding the rails to the adjacent "gas and water pipes. The results have been more or less "satisfactory. It is obvious that if the rails and adjacent gas "and water pipes can be kept at the same potential electrolytic "action can be effectively prevented." The point, I think, is this: If the length—parallel to the line of current-flow—of any detached mass of buried metal, such as a gas or water pipe, is such that the potential difference between the points of entrance and exit of current exceeds that which will generate electrolytic action, then you must have corrosion at the anode, and that, proportionate to the current traversing the metal. Therefore the position of a buried mass of metal relatively to the line of the conductor or flow of current is all-important. The dangerous position is, of course, the position parallel to the conductor. On page 447, Mr. Parshall, after mentioning that he makes the rails positive, says: "Whatever difference of potential "there may be between the two will be such that the earth return "is, in general, positive to the neighbouring water or other pipes, "since in this case whatever electrolysis takes place will be in "the track return itself." I think you will see that that will

The President. not always happen, and that you cannot protect the water and gas pipes in that way; for, if the potential difference between two points along the line at some distance apart is sufficiently large to generate electrolytic action, it will take place whether the rail is positive or negative: the only difference will be as to whether it will take place at the point of entrance or exit of the current from the pipe. If the rail is positive, then the pipe will be protected at the point of entrance, but corrosion will go on at the point of exit. I will not, at this late hour, detain the meeting any longer, but will ask Mr. Parshall to reply.

Mr. Parshall. Mr. H. F. PARSHALL, in reply, said: Many of the important points touched upon in my paper have not been taken up in the discussion.

Regarding Professor Perry's remarks, he speaks of the disturbances to magnetic observations by electric tramways. I think Professor Perry has summed up the merits of the case in that, after many years of research, magnetic observers know no more of terrestrial magnetic phenomena now than they did three centuries ago, in the time of Gilbert. Indeed, I have discussed this matter with many persons interested in magnetic observations, and I have never met anyone who has professed to attach any great importance to what might be discovered in connection with observations of terrestrial magnetism. There exists no doubt as to the usefulness of electric tramways to a great number of people. It seems, therefore, an injustice to hamper the progress of an institution generally useful to the public, for the sake of making observations that never have given, and never may give, to the world any important results.

Professor Ayrton spoke of the bad practice in engineering matters of trying to use one thing for two purposes. He likened this to a hole in the roof of a hut to let in light and let out smoke. When we consider, however, in dealing with the 7-volts drop fixed by the Board of Trade, the rails in the ordinary case are equivalent to some 6 square inches of copper, it would seem that, unless we combine the electrical with the mechanical function of the rails to revert to Professor Ayrton's simile, the

hole would exceed the roof, since a conductor of so great importance cannot well be ignored in commercial practice. Mr. Parshall.

Professor Ayrton asked as to the meaning of the diagrams on the wall. The diagrams are the results of tests, and the paper clearly sets forth the methods of such tests. Each element that went to form the conductance of the joint was tested independently—that is, the bond was tested alone—and the resistance in each element of the bond, such as the contact resistance and the resistance of the copper in the bond, determined separated and collectively. The fish-plate was next tested without the bond, and the separate elements of resistance ascertained. The conductance of bond and fish-plate together was further tested, and compared with the sum of the conductances of bond and fish-plate as separately determined.

Professor Thompson raised some doubt as to the accuracy of the statement on page 456. The statement there is the statement of a result which has been found in testing long lines, and is not confined to tests carried out on individual bonds. It should be borne in mind that with the current-densities stated in the paper the heating at the contact is inappreciable. In the case of tests on actual bonds, single contacts have been tested with the current flowing first from the bond into the rail, and secondly from the rail into the bond. Subsequently the bond as a whole has been tested, and a statement as it occurs is entirely justified by the results obtained. By referring to the description of the tests, it will be seen that the means employed were such as to ensure the greatest possible accuracy

Professor Thompson wrote me many months ago, asking me to write this paper for the Institution. I then made a collection of all the existing tests and information bearing on the subject of bonding. I was considerably surprised to find that in the average case the bonds were found by the makers to have negative resistance. After looking into the matter, I found the reason was that the joints had been tested, and that the rail had been taken as having a resistance six and a half times that of copper. In order to put this matter right, I have determined the resistance of tram rails of varying percentages of carbon and other impurities.

Mr. Parshall. These tests have been extensively carried out on rails manufactured by different makers, and the results given may be taken as representative for each particular analysis. It will be seen in every case that the resistance of the rail is some 50 per cent. greater than is commonly taken.

Professor Ayrton asked as to the meaning of the negative line put down in the diagram as "iron between bond-holes." For the purposes of making an accurate graphical representation of the elements constituting the joint resistance, the bond itself is given credit of the amount of rail between the holes, this iron not being in circuit so far as the current which passes through the bond itself is concerned. This iron is more or less in circuit so far as the fish-plate is concerned.

The method adopted was that thought most practicable. It is scarcely rigorous, since the resultant drop depends upon the current-distribution between the current and the fish-plate.

For all practical purposes, however, the graphical method adopted may be taken as correct, since the various tests were found to go together well on the assumptions made. I do not attach so great an importance to these diagrams as I do to the statement of individual tests. The diagrams, however, are certainly useful to illustrate the meaning of the tests.

Major Cardew. Major P. CARDEW, R.E., in reply, said: I have only a very few remarks to make. As regards my own paper, I think the only criticism was with regard to the use of a separate generator. That, as the speaker pointed out, would probably not be used to-day; but, of course, the equivalent, or another booster, to make up for the volts lost, could easily be put on to the feeder. Where you have a pair of feeders it is obvious that they should be supplied by some method with a greater difference of potential than is required from the ordinary 'bus-bars. It would hardly do for me to go into the question of insulated returns, on account of the time, and for other reasons; but I think, with regard to Professor Thompson's remarks about the distance at which a magnetic observatory should be considered to be completely protected, it would be rather dangerous to attempt to lay down

any absolute distance. It must, to some extent, depend upon the angle that the tramway subtends, as regarded from the observatory. That is to say, if the tramway subtends a small angle, and then goes away in a direct line, it would not have the same effect as if it subtended a larger angle from the observatory. With regard to the President's remarks, I was very glad that he pointed out that corrosion of pipes is not entirely got over by ensuring that at one point the rails are positive to the pipes, if there is such a fall of potential in the rails as to induce the current to go out of the pipes at other points. In mitigation of that, of course, it has been well understood—and we have always encouraged the idea—that the pipes should, where it can be arranged, be connected metallically to the negative of the generators. That, at any rate, gives a metallic path out for the current, without danger of electrolysis. It is the best we can do, I think, at present in that way. Major Cardew.

I do not think there are any other points, except that I should like to express my great appreciation of Mr. Parshall's valuable paper. The information it contains is of great importance to all who may be connected with the working of electrical tramways.

The PRESIDENT: I have now the pleasure to propose, and ask you to pass, a vote of thanks to Mr. Parshall for his very interesting and useful paper; and also to Major Cardew and Mr. Trotter for their papers.

Carried by acclamation.

The PRESIDENT announced that the scrutineers reported the following candidates to have been duly elected:—

Associates :

John Russell Bedford.
Edmund Walter Beveridge.
James Hampton Brennand.
William R. Brown.
William Burr.
Henry Arthur Campbell.
Edward Coote.
Leslie Foster Davis.
John Herbert Durant.
Frank Fairley.
Samuel C. Gibson.
Frederick Shakespere Hanning.
Henry Hartnell.
Richard J. Hughes.
Frederick Hutchins.
Edgar L. Ingram.
Harold W. Morisset.
Alfred E. Pepper.
Norman Smith.
Rochfort Henry Sperling.
Arthur Edward Tessier.

Students :

William Arthur Del Mar.
Alfred N. Dixey.
Donald Hills.
Herbt. H. Lorraine Prendergast.

The meeting then adjourned.

The Three Hundredth and Sixteenth Ordinary General Meeting of the Institution was held at the Society of Arts, John Street, Adelphi, on Thursday evening, May 5th, 1898—Professor SILVANUS THOMPSON, F.R.S., Vice-President, in the Chair.

The Minutes of the Ordinary General Meeting held on April 28th, 1898, were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council:—

From the class of Associates to that of Members—

Alfred Heinrich Jackson.

From the class of Students to that of Associates—

Arthur Thomas Gordon-Smith. | Laurence Moore Peel.

Edward Ernest Tasker.

Mr. R. W. Weekes and Mr. W. C. Goodchild were appointed scrutineers of the ballot for new members.

Donations to the Library were announced as having been received since the last meeting from Messrs. A. Constable & Co., Mr. W. Silver Hall; and Mr. Charles Bright, Mr. A. P. Head, and Dr. E. Obach, Members; to whom the thanks of the meeting were unanimously accorded.

The CHAIRMAN: I have now the pleasure of calling on Mr. Leonard Andrews, Associate, to read his paper.

THE PREVENTION OF INTERRUPTIONS TO ELECTRICITY SUPPLY.

By LEONARD ANDREWS, Associate.

It is probable that some central station engineers will remark, on reading the title of this paper, that it is several years behind Mr. Andrews

Mr. Andrews.

the times; that interruptions to the supply from a properly equipped modern station never now occur; that at their own particular stations the supply has never once been interrupted since it was started, &c.

It speaks volumes for the progress of electrical engineering during the past few years that there are several existing central stations that can show an absolutely clean sheet in this respect since their commencement, and everyone will agree that their engineers hold a very enviable position. It is very doubtful, however, if any one of them can say that they have not a consumer connected to their mains who has during the past 12 months ever had his supply disconnected; and, if that is so, surely there is still sufficient room for improvement to make the matter worth discussing. After all, it is these local interruptions that are so irritating to consumers. Our experience has been that we get far more abuse from a consumer whose lights fail when his neighbour's lights are burning satisfactorily than we do if they are both suffering together.

Some of the engineers who have achieved such an excellent record attribute their immunity from failures to the fact that they use fuses made of copper of the same sectional area as the mains. There can be no doubt that a large majority of the interruptions that do occur are caused by fuses blowing when they have no business to do so. Yet it does seem rather risky to use no safety devices at all. We have already heard of more than one case where an arc of a few thousand horse-power has been started under the pavement, and would not be quieted until the supply had been switched off from the works. On the other hand, when one remembers upon what a number of fuses the continuity of an average consumer's supply is dependent, it is really wonderful that he is not more often left in darkness. It is no exaggeration to say that there are often from 15 to 20 fuses between the generators and the lamps they supply. Is it, then, to be wondered at that we are so often told that electricity supply is not to be relied upon? It would be different if we could always depend upon fuses blowing at approximately the current they are set for. But we cannot. It is no uncommon case to take two

similar fuses that have been in use for some months and find that one requires about 100 per cent. more current to blow it than does the other. The fuses used on alternate-current circuits appear to be particularly erratic in this respect. Mr. Andrews.

The *Electrical Review* drew attention to this fuse trouble in one of its leading articles a few months ago. Still more recently, Mr. W. B. Sayers, in an article in *Lightning* on the subject, says :—

" In a city less than 100 miles from where I live there is an " electricity works which, so far as I am aware, has not failed to " maintain its supply for a single minute during the last four or " five years; and yet the popular belief that the 'electric light " 'is not reliable' is maintained to this day, and with good " reason. . . . . Now the only proper cause, in my opinion, " for a main fuse 'blowing' is a short-circuit on the mains, and " yet I have no hesitation in saying that less than 1 per cent. " of the cases of main fuses 'blowing' are due to this cause."

The conclusion that we have come to at Hastings is that the only reliable conductor of electricity appears to be a copper cable; and, consequently, it seems advisable to reduce all fuses, switches, safety devices, and mechanical connections of any description to a minimum.

If any fuses that it is customary to use can be omitted, everyone will admit that they are a source of danger, and, consequently, better omitted. Take, for instance, the fuses between alternate-current generators and the 'bus-bars: what are they used for? They cannot be necessary to protect the machines from being overloaded, because all modern makers claim that their machines may be short-circuited with impunity. Presumably they are intended to prevent a generator that fails short-circuiting other machines working in parallel with it; but everyone knows that if two or three machines of an equal output, and equally fused, are working together, it would be the fuses of the healthy generator that would blow, and not those of the faulty one, because the former have to carry sufficient current to blow the latter, in addition to all the useful work on the mains at the time.

Now what should we think of an omnibus driver who cut the

Mr. Andrews.

traces of one of his horses because it attempted to do more than its share of the work, or who, when one of them fell down dead, made the remaining horse drag the dead one along in addition to the extra work thrown upon it by the decease of its comrade? This sounds absurd, but it practically represents the manner in which we alternate-current station engineers have been educated to treat our machines; for are we not taught carefully to equip them with safety devices to cut them out of circuit just at the time when all their energies are required to burn out a short-circuit on the mains? whereas any device to prevent a failing machine from short-circuiting others is considered quite an unnecessary piece of apparatus.

In continuous-current stations zero cut-outs, or discriminating cut-outs, are generally used in preference to excess-current cut-outs—the word "discriminating" being used to designate a cut-out that operates only when the current is flowing through it in a reverse direction to its normal.

Magnetic cut-outs of any description have not hitherto been looked upon with much favour in this country. The majority of those now in use require too careful and delicate treatment to be popular. Only people who have attempted to design a simple and trustworthy discriminating cut-out can realise the number of difficulties that have to be overcome in doing so. It is easy enough to make an apparatus that will operate under certain specific conditions in the workshops, but it is a very different matter to construct a cut-out that can be relied upon to open the circuit of a failing generator with a very small return current, and that can be guaranteed never accidentally to operate at any time when it is not required to do so. In the first place, the sectional area of the winding must be large enough to carry the maximum current of the generator without undue heating; at the same time the apparatus must be small and compact, consequently the turns must be few; and, finally, it must operate with a return current of only a small percentage of the maximum current, therefore the ampere-turns or magnetising force must be small. This generally involves the use of delicate releasing mechanism or relays, which require careful treatment, or they will operate at the wrong time,

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and not when a failure occurs. These are only workshop difficulties. The more serious are those which confront us when the apparatus is in use under actual working conditions.

Take, for instance, the case of zero magnetic cut-outs. Every-one knows that these can be made to operate only when the current falls below a predetermined amount; and yet it is also well known that if a short-circuit occur on a system of mains supplied by a number of generators equipped with zero cut-outs, several of the generators will be promptly cut out of circuit. This is simply a specimen of the many troubles which it is impossible to foresee and guard against in the manufacturer's workshop.

Between three and four years ago we realised that a reliable discriminating cut-out was badly wanted, and since that time considerably over 100 different combinations of compound wind-ings and releasing mechanisms have been experimented with. Many of these have only reached the experimental stage, but a fair proportion have had several months' actual use under working conditions before some unforeseen difficulty made it necessary to scrap them for some new and improved arrangement. The result has been that we have at last been able to secure a cut-out that appears to be perfect.

It appears at first sight impossible to design a satisfactory discriminating out-out for use in connection with alternate-current machines in which the current is reversing in direction some thousand times a minute. So long as one considers these reversals in relation to a constant polarity, it is, of course, impossible; but as soon as the direction of the current through a particular machine is considered relatively to the direction of the current in all other parts of the system, the problem becomes a comparatively simple one. Fig. 1 illustrates diagrammatically what we have found to be the most satisfactory method of applying this principle. The operating device in this arrangement is practically a shunt-wound motor, the thick winding of which is connected in series with one of the leads from the alternator it is intended to control, and the shunt winding is connected across any transformer excited off the

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'bus-bars. Now it is obvious that the direction of the current in the shunt winding, S H, will pulsate synchronously with the current in the 'bus-bars, and will be quite independent of the direction of the current in the series winding, S E, whereas the direction of the current in the latter relatively to the current in the 'bus-bars will depend upon whether the machine to which it is connected is generating current or is being driven as a motor. If both machines are generating current, then the direction of the current throughout the whole system at a given moment will be represented by the arrow-heads shown full.

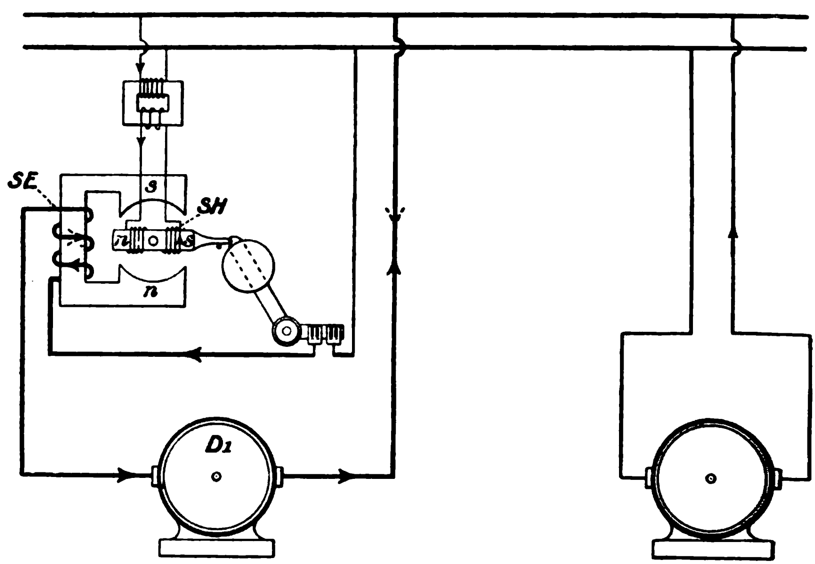

Fig. 1.

But if, say, alternator D_1 fails, it will tend to short-circuit the rest of the system, and the current will rush back into it in the direction shown by the dotted arrow-heads, whereas the direction of the current in the other circuits will remain the same. In the former case the relative direction of the shunt winding to the series winding in the cut-out device will be such as to tend to make the armature rotate in a clock-wise direction, and so to lock the switch securely; but when, as in the latter case, the direction of the series current relatively to the shunt

current is reversed, the armature will rotate in a contra-clockwise direction, and so open the circuit. Mr. Andrews.

Fig. 2 is a sectional elevation of a mechanical application of this principle to a low-tension cut-out suitable for use with continuous-current generators, transformers, and low-tension

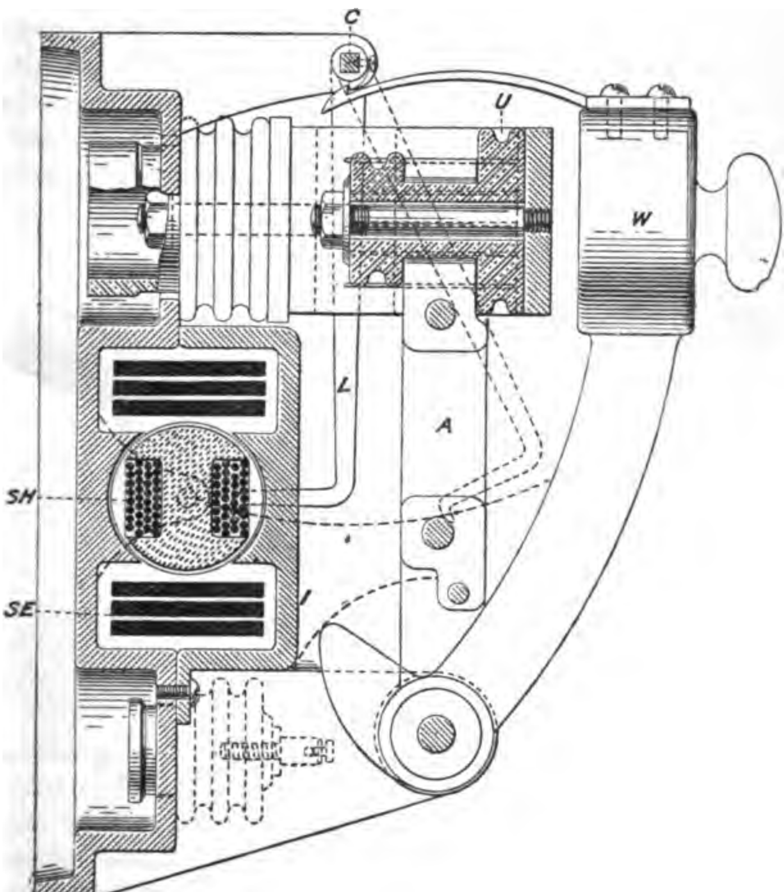

Fig. 2.

mains. The weight, W, is held in a nearly vertical position by the catch, C. Attached to the catch is a lever, L, the free end of which engages in a pin projecting from a metal disc on the end of the armature, S H. The series winding, S E, consists of a few turns of thick copper tape wound directly round the armature. One

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end of this is sweated and riveted directly on to a brass plate screwed and sweated to one of the contacts, and the other end is sweated on to a thimble, T (Fig. 3), which forms one of the series terminals. The other series terminal is screwed and sweated directly on to the second contact. The whole of the series connections and contacts are supported on three corrugated porcelain insulators sulphured into the base. Fig. 3 shows these series connections removed from the rest of the cut-out. This series winding encloses a practically closed double magnetic circuit, consisting of the armature core, a portion of the base, and the cast-iron covers. For alternate-current working these parts are, of course, laminated.

FIG. 3.

An important feature of this cut-out is the releasing catch. This is shown in detail in Fig. 4. The pin, P, is fixed in such a position on the armature disc that an extension of the arc described by the lever, L, will cut the pin P and the centre of the armature disc. The result of this arrangement is that no amount of vibration or pressure applied to the weight W will tend to make the disc rotate in either direction. And, consequently, when the armature is rotated by a return current it releases the weight without having first to lift it, as it would have to do with any other form of catch. We find this an absolutely reliable and extremely sensitive form of release.

When the weight W is released it falls through an angle of about 60 degrees, then with a sharp blow it strikes the arm carrying the contact connecting piece, thus overcoming any sticking of the contacts due to a good fit or to corrosion. Mr. Andrews.

A specimen 500-ampere cut-out of this description is shown on the table. You will see that it is so reliable that, even when there is no forward current on to lock it in position, it may be knocked about with a mallet to show that no amount of vibration

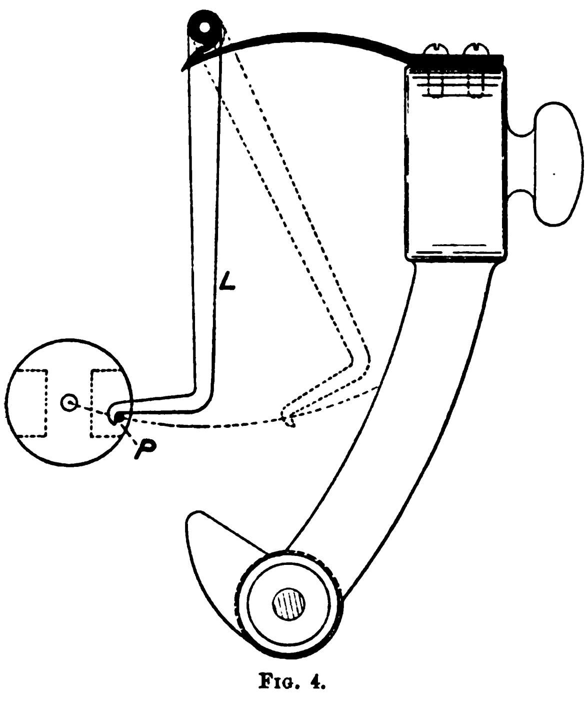


Fig. 4.

will release it, whereas it is so sensitive that the pressure of a feather upon the armature will do so.

The same general arrangement without any winding on the armature makes a very sensitive and reliable excess-current cut-out.

Fig. 5 is a sectional elevation of a similar cut-out modified for use in connection with high-tension currents. In this arrangement the contacts are screwed and sweated into metal pots, and immersed in water. This serves effectually to quench

Mr. Andrews.

any tendency to arcing when large high-tension currents are interrupted. All the high-tension parts in this cut-out are entirely covered with porcelain or other insulating material. The releasing mechanism is practically the same as in the low-tension cut-out.

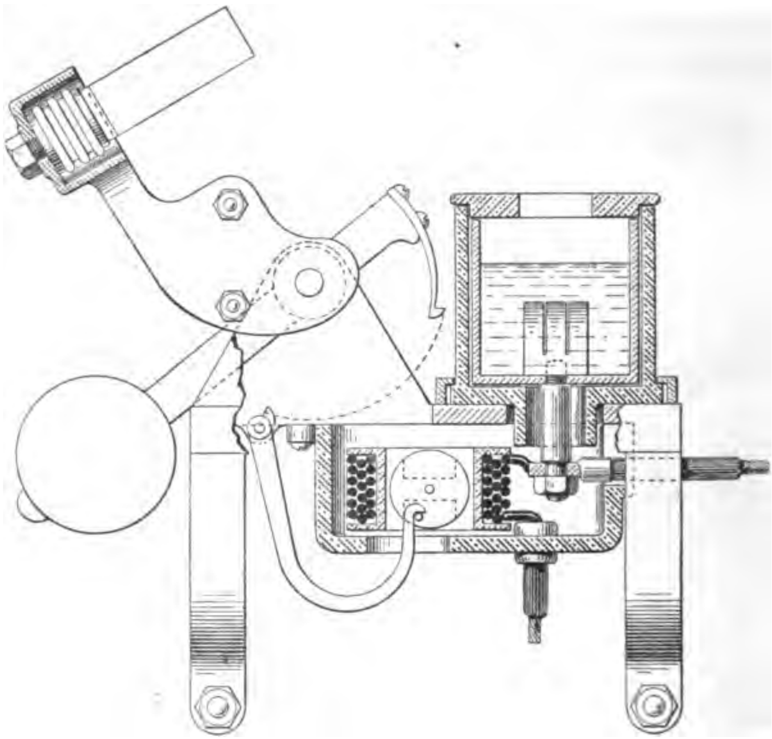
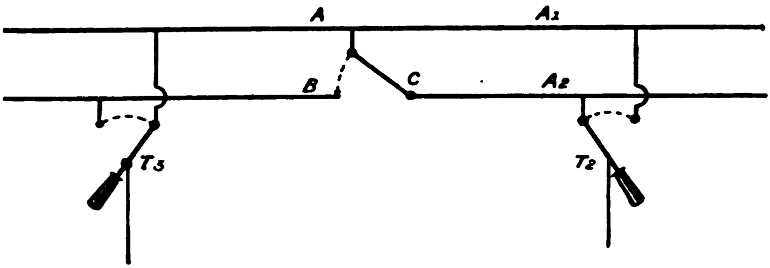


FIG. 5.

Fig. 6 is a diagram of the Hastings switch gear. We have found this arrangement entirely satisfactory in every respect. It has not only enabled us to cope with several breakdowns to machinery without interruptions to the supply, but it has also effected a saving in coal, &c., during the past 18 months of over £400. This has been saved by the arrangement referred to enabling us to work safely without running a spare plant.

All the machines are arranged to feed into a common pair of inner and outer 'bus-bars. The inner 'bus-bar, however, is

divided at A, by a change-over switch, C, into two separate branches. One of these, A_1, is permanently connected to A, but the other branch, A_2, may be connected either to the main 'bus-bar or to a spare 'bus-bar, B. Normally, it is connected to the former. Each



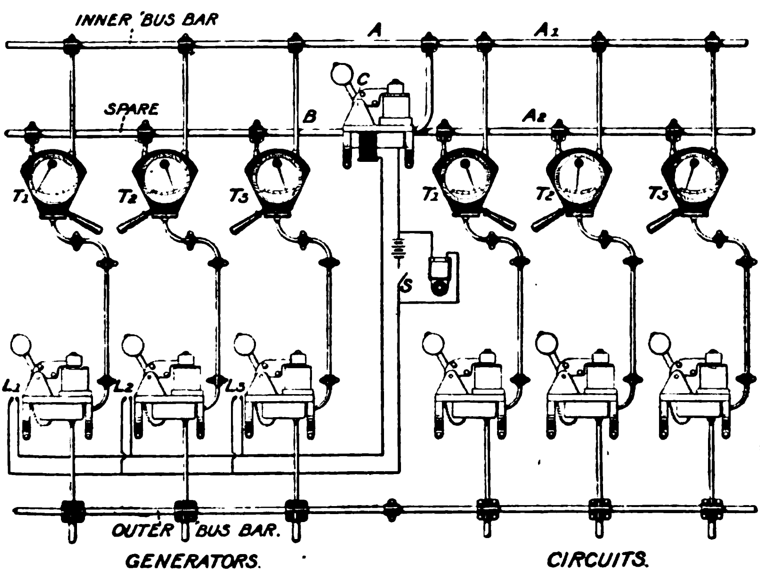
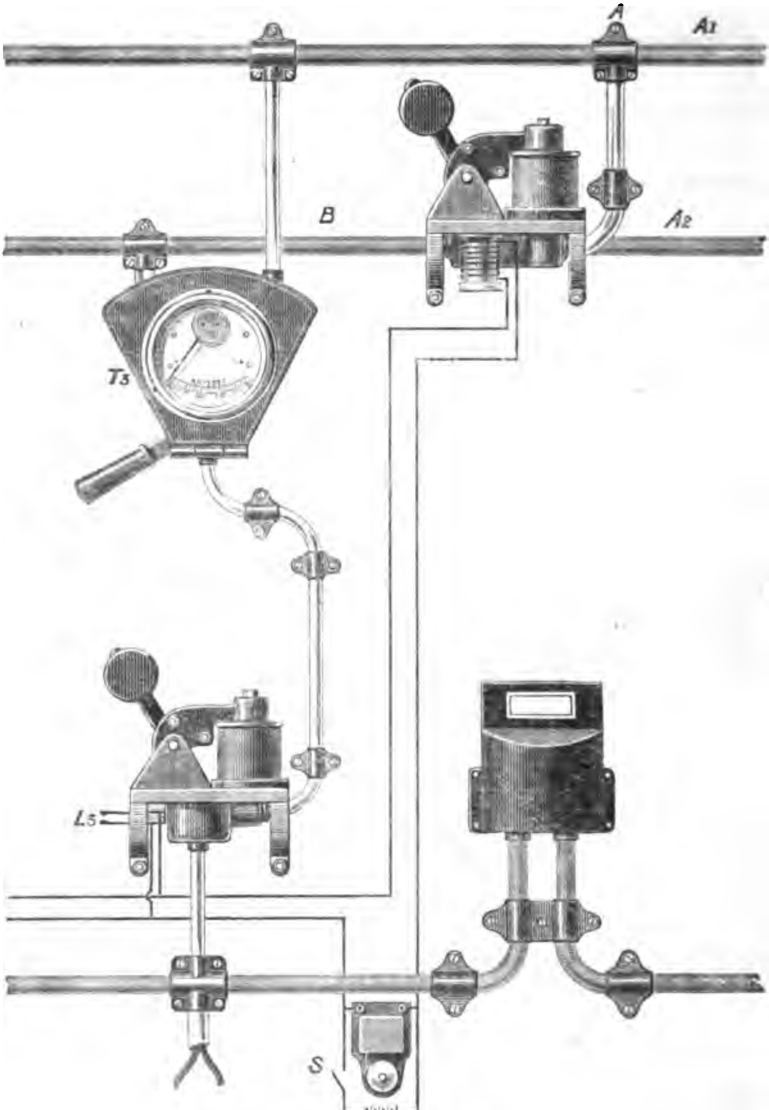


FIG. 6.

machine and circuit is equipped with a two-way switch, T, by means of which any machine or any circuit may be connected either to the inner 'bus-bar or to its auxiliary branch. In the diagram only three circuits and three machines are shown. The maximum output of the machines is 60 amperes, and the total load of the

Mr. Andrews.

three circuits is assumed to be 120 amperes—namely, 60 on No. 1, 40 on No. 2, and 20 on No. 3. By setting the circuit



DETAILS OF ONE SECTION OF FIG. 6.

two-way switches, T_2 and T_3, over to the left, circuits 2 and 3 are connected directly on to the A_1 branch of the inner 'bus-bar;

whereas, T_1 being set over to the right, circuit 1 is connected on to the A_2 branch. The machines Nos. 2 and 3 are connected in parallel by their two-way switches directly on to the inner 'bus-bar, A. And the machine No. 1 is kept turning as a spare, with its two-way switch over to the right, thereby connecting it on to the spare 'bus-bar, B. The change-over switch C is constructed to be released by a solenoid excited off any convenient source, E. Inserted in series with it are two switches, S and L_1, L_2, or L_3. Both the S and one of the L switches must be closed together to excite the solenoid. When S only is closed it completes a circuit through an electric bell, which can be heard anywhere in the station. The driver has instructions that whenever that bell rings he must immediately run the spare plant up to speed. Now, if either of the running plants break down, the switch-board attendant merely has to close switch S, and then as soon as the volts on the spare machine have risen to normal, or before if necessary, he releases the cut-out switch of the faulty machine. The weight of this on falling closes switch L, and so completes the circuit through the solenoid of the releasing change-over switch C. This disconnects the 'bus-bar A_2 with its load of 60 amperes from the inner 'bus-bar A, and transfers it to the spare 'bus-bar B at precisely the same moment as the generator supplying 60 amperes is disconnected from the inner 'bus-bar. Thus the lights on the circuits Nos. 2 and 3 are not affected as they would be if the change-over were not done simultaneously with switching out the faulty machine; and the lights on No. 1 circuit only give a momentary flicker, which, as a rule, is not even noticed by the consumers. Mr. Andrews.

Of course the use of a spare 'bus-bar is not original, but we believe that the simultaneous method of change-over is.

In the discussion on a paper read before the Northern Society of Engineers on switch gear last year, it appeared to be the general opinion of engineers present that all high-tension connections should be absolutely enclosed. But it was objected that it did not appear possible to effect this without having exposed connections at the back of the board, and boards with backs to them increased rather than decreased the risk of accidents.

Mr. Andrews. A suggestion was also made in this same paper that a full-sized diagram of connections painted on the walls above the switch gear would often prove useful, but other engineers thought that the switch gear should be its own diagram. We venture to think that in the switch gear shown in Fig. 6 we have succeeded in complying with both of these specifications. The leads from the machines are carried in porcelain or other insulating pipes directly up to their respective cut-outs, from these to the two-way switches *via* their ammeters, and so on to the 'bus-bars. All the high-tension connections, both in the cut-out switches and the two-way switches, are entirely enclosed; and, as these switches and the conductors are bolted and clipped to the surface of a brick wall, all the connections are diagrammatically shown at a glance.

Some form of excess-current cut-out should certainly be used on the feeders. We prefer magnetic cut-outs to fuses, as we find them more reliable. They can also be used as switches if necessary, which is a distinct advantage. At any rate, whatever form of cut-outs is used, their operation should on no account be permitted to interrupt the supply to any consumers.

It is curious that engineers have not paid more attention to the duplication of electrical mains. It is the custom to spend thousands of pounds on duplicating boilers, engines, dynamos, and other plant which is directly under the engineer's control; but no steps are taken efficiently to duplicate that part of the system over which he has no direct control, and which is always at the mercy of such external forces as gas explosions, burst water mains, fires, pick-holes, &c.

It is true many engineers arrange their mains on some ring system, so that any portion of it may be made dead for repairs, extensions, &c.; and some go even further, and fix fuses at intervals round the ring, so proportioned that a fault will only cut out a certain section of the lights. But that is not sufficient. We ought not to be satisfied until we are able to guarantee an absolutely constant supply to everyone. The problem of how to do this has been troubling us at Hastings for years. But we now

feel satisfied that we have solved it. Our method of doing so is shown in Fig. 7. Mr. Andrews.

Each sub-station or feeding point is supplied from the works, W, by two feeders, either by running to each two distinct mains, each sufficiently heavy to carry without excessive fall of pressure half the load of the sub-station, as shown at A, or by connecting together two sub-stations, each supplied by separate feeders, as at B; or, in the case of low-tension distribution, by running radial feeders from the generating station, and connecting the several feeding points on these to corresponding feeding points on an

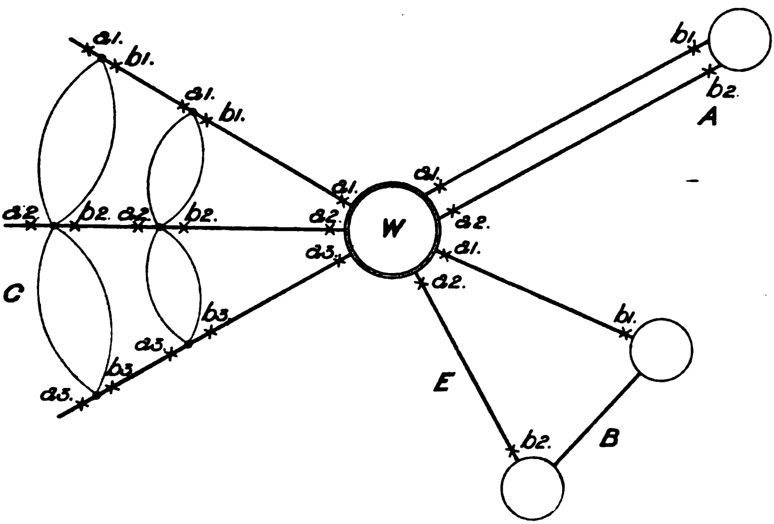


Fig. 7.

adjacent feeder by the distributing mains, as shown at C. If a fault occurs on either of these feeders, the current will be supplied to it both directly from the generating station and also *via* the adjacent feeder and connecting mains. To prevent this fault from short-circuiting the whole of the system, fuses have previously been inserted in the feeders at a_1, a_2, b_1, and b_2. A little consideration will show, however, that this arrangement can never be satisfactory, for it is obvious that either one of these feeders may at any time have to carry as heavy a current as the others; consequently, they must all be equally fused. Now, if a short-circuit occur at, say, E, fuse a_2 will blow. The current will

Mr. Andrews.

then be supplied *via* a_1, b_1, and b_2. Now fuse b_2 should, of course, blow, and so cut out the faulty main, leaving both sub-stations to be supplied *via* feeder 1. But this will not happen, because a_1 and b_1 would have to carry sufficient current to blow b_2, in addition to the useful current taken by the sub-stations. The result will naturally be that a_1 or b_1 will invariably blow before b_2, thus cutting off the lights supplied by both feeders. Now, if fuses b_1 and b_2 are replaced by discriminating cut-outs, no amount of current flowing in its normal direction will cause them to operate, but a comparatively small return current will immediately release them. As the only conditions that can possibly cause the current to flow back from the sub-stations to the generating station is a fault on the feeder between these points, this form of cut-out can be relied upon to operate only when it is required to do so.

It is, of course, very essential that the cut-outs used for this purpose should be made not to operate if either the series or shunt current be interrupted separately or simultaneously, as it would cause a great deal of trouble if the supply from the works were ever interrupted for a few seconds and all the cut-outs on the mains were thereby caused to operate.

Cut-outs that are opened with a spring or springs should also be avoided, as it is impossible to make them sensitive and reliable, owing to the fact that the catch has to be released against the maximum tension of the springs; and, further, these springs must be very stiff, as, in addition to overcoming the friction of the contacts when they are clean, a large margin must be allowed to overcome the increased friction that will certainly be caused by corrosion of the contacts after they have been in, say, a few months. A falling weight seems much better suited for the purpose than a spring, for the pressure on the releasing catch is comparatively small, and the sharp blow upon the contact arm is just what is required to overcome with certainty any tendency to sticking due to corrosion.

Cut-outs should have no screws about them liable to work loose and so release the catch and open the circuit.

For burying under the pavements they should be as compact as possible, as the space is then very limited.

They should also be unaffected by rust, dust, damp, or corrosion, and precaution should be taken to prevent any possibility of their being caused to operate by external vibrations. They should be made to cut out with as small a current as possible, to prevent excessive arcing when the circuit is interrupted. Mr. Andrews.

The cut-out illustrated in Fig. 5 has been designed to comply with these and other requirements.

Another very frequent cause of local interruptions is the failure of primary fuses of transformers. It certainly appears to be advisable to use some form of excess-current cut-out between the primary winding of transformers and the mains supplying them. But the object of this cut-out should be, not to prevent the transformers from being overloaded, but to protect the mains from being short-circuited by a faulty transformer. Where two or three transformers are coupled together no good can come of cutting one of them out of circuit because it is overloaded, for if one is cut out the extra load is thrown upon the others, thus invariably blowing their fuses as well and cutting off the supply to the whole district.

We consider that a transformer fuse should not blow unless the excess current exceed the normal current by about 300 per cent.

Fuses between the secondaries of transformers and secondary 'bus-bars are invariably worse than useless. Take, for instance, the case of three transformers of equal size feeding a common 'bus-bar. If one of these fails, the current will rush back into it from the other two; but, as these have to supply the useful current to the mains, in addition to that required to blow the faulty transformer's fuse, they will blow their own fuses before that of the faulty transformer. Obviously these fuses should be replaced by discriminating cut-outs.

Fig. 8 is a diagram showing the equipment of a sub-station we now have in hand. The two high-tension feeders from the generating station, M_1, M_2, terminate in two return-current cut-outs, R_1 and R_2. Beyond the cut-outs they are connected together by the fuse F. Fuses F_1 and F_2, &c., are

Mr. Andrews.

inserted in series with the primaries of each transformer. Return-current cut-outs, r^1, r^2, r^3, &c., are inserted in series with the secondaries of each transformer. The primary connections of the sub-station are divided into two distinct halves; the inner 'bus-bar of each half is equipped with an earthing fuse, E F. Any man found working on the primary connections of either side without the earth fuse inserted will be instantly dismissed. Either half sub-station can, of course, be made dead by opening the return-current cut-out of the feeder to which it is directly connected, fuse F, and the secondary return-current cut-outs of that side.

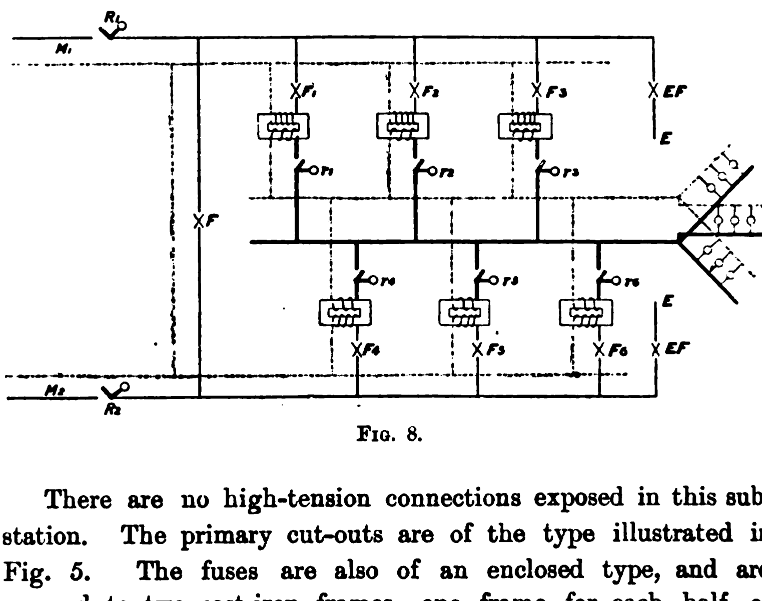


FIG. 8.

There are no high-tension connections exposed in this sub-station. The primary cut-outs are of the type illustrated in Fig. 5. The fuses are also of an enclosed type, and are screwed to two cast-iron frames—one frame for each half of the station. A section of these frames is shown at G, Fig. 9. The 'bus-bar, B, to which the transformer fuses are connected, is supported on insulators inside this frame. These frames are hung on hinges, H, so that they can be lifted to enable the connections to the fuses to be examined periodically. High-tension cables are run down to the transformers in porcelain tubes, P, clipped to the walls. The high-tension apparatus for one half of the station is on the north wall, and that for the other is

on the east wall. The low-tension return-current cut-outs, which also serve as secondary switches, are on the south wall, and the distributing 'bus-bars and instruments are on the west wall. This sub-station is a building 12 feet long by 8 feet wide by 7 feet 6 inches high. It is built above ground in a back garden in the centre of the district it supplies. We pay £10 per annum for the rent of the ground it stands upon. Mr. Andrews.

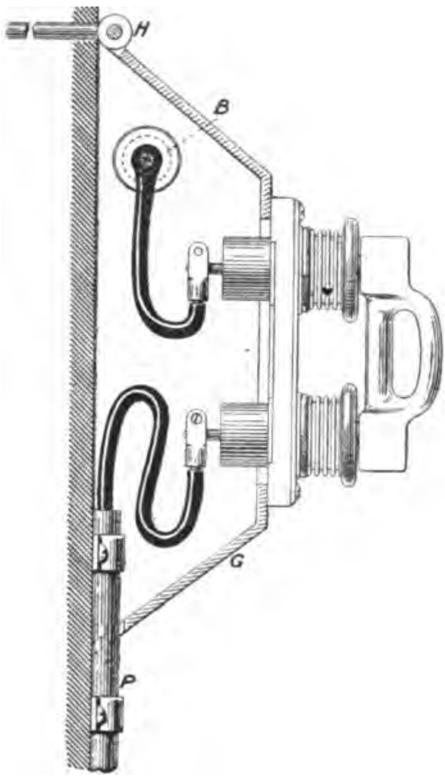


Fig. 9.

Several of our existing sub-stations are placed under the pavement. These have been such a source of trouble to us that we are now abandoning them entirely.

Arrangements are made to cut off all the transformers except one small one during the hours of light load, not only for the purpose of saving the current wasted in exciting them, but also to

Mr. Andrews. allow them to cool down between each heavy shift. We expect by so doing greatly to increase the life of our transformers.

Whether it is advisable to equip the low-tension distributors with cut-outs or not, is a question upon which we should be glad to hear the opinion of other engineers. We are inclined to think that, if a 200-volt short-circuit occurred on a cable not exceeding 1 square inch sectional area, it would in most cases burn itself out before it damaged other parts of the cable. If we could be sure of this, we should endeavour to loop all of our distributors and insert in series with each main a magnetic cut-out adjusted to operate when the current exceeded five times the normal. If the main burnt asunder before the cut-outs operated, the supply would not then be interrupted to any consumers.

Presumably everyone will admit that excess-current cut-outs are necessary on electric light services where they enter consumers' premises, but we think the majority of central station engineers will agree with Mr. Sayers when he says that they should not operate until the normal current has been exceeded by at least 300 per cent. Is it not possible that the number of branch cut-outs at present used to comply with the fire insurance regulations might be reduced? It appears to us to be rather a question whether or no so many of these cut-outs do tend to reduce the risk of fires. Consumers who are repeatedly troubled by these branch fuses melting are apt to discover that a fuse replaced by a stout piece of copper wire gives them far less trouble. Now, if the connection to one of these short-circuited fuses should work loose, it gets hot, the heat is transmitted to the cable, and a smell of burning is the result. Of course, if no branch fuses were used, it would be advisable entirely to enclose the house wiring in some form of fire-proof conduit instead of in wood casing; but we are inclined to think this would be a preferable arrangement both for the prevention of interruptions to the supply, and for the reduction of fire risks.

Mr. Raworth. Mr. Raworth: It is only because I received an invitation from Mr. Andrews to speak in the discussion on this paper that I allow myself to do so, because I am in the invidious position of

having been a bit of an inventor in this direction myself, and I would not on this occasion say anything which could in the slightest degree detract from the honour which is due to Mr. Andrews for the very great amount of painstaking work he has done on this subject, in order to turn any of that honour towards myself, who have done so little. Mr. Andrews hits the right nail on the head in pointing out the futility of using direct fuses upon the connecting links between the dynamos and the omnibus-bars. It was in considering this question in connection with the large undertaking of the City of London Company, some five or six years ago, that I came across the difficulty that one would naturally experience in the case of one machine either breaking down or losing its excitation, and the other dynamo making an ineffectual attempt to burn down the fuse of the disabled machine, because, as Mr. Andrews has very clearly put it, it is almost certain to burn its own down first. It is a curious fact that the first accident the City of London Company ever had was due to this very same cause. There occurred in connection with one of the smaller machines some accident, and the result was that we lost every fuse in the station, and the lights were completely extinguished. It was in trying to find some way out of that palpable difficulty that I made, I believe, the first discriminating fuse—that is to say, a fuse which will act only when the direction of the energy is reversed. It was partly a fuse—and in that fact lay its inefficiency—a fuse of this kind: that there were two paths for the current—one through the fuse, and one through a by-pass. There was a little transformer upon the by-pass, which tended to add volts to the by-pass, so that it sucked the current through the by-pass; but the moment the energy turned round, and attempted to go back upon the machine, it diverted the current through the fuse: thus the fuse would burn down when the energy was going in the wrong direction, and it would not burn down when the energy was going in the right direction. We experimented a long time with these fuses, and at last we got them right, but, as Mr. Andrews says, the trouble was enormous. With an installation such as we had to deal with, the experiments could not be carried

Mr. Raworth.

Mr. Raworth. out for 6d. or 9d. We had to spend a great deal of money on them, and the mere fact that the fuse was a fuse introduced so much time element that there was time enough for the machine to get burned out before it acted. It is just because Mr. Andrews has done away with the fusible metal and has put in a magnetic arrangement—which I know, from having watched his labours, has taken him a great deal of time and trouble to evolve—that he has got it in this practical form in which he has put it before us to-night. You have seen that it cannot be joggled into action; it must be tickled in the right spot. Mr. Andrews has touched in his title a point which I am sure a good many engineers will take up. They will say, "We have no interruptions of supply." I think he has made a mistake in limiting the title of his paper. We are so apt to look at the title and therefrom to sum up the whole gist of the paper. If he had called it, "Apparatus for "Relieving the Mind of the Electrical Engineer from Anxiety," he might have caught more fish in his net, because I will undertake to say that those gentlemen who have kept this remarkable record of never having let their lights out have not done it without a great deal of mental anxiety.

That leads me to another point. Some people may perhaps turn round on Mr. Andrews and say: "If you had alternators with "so much self-induction in the bobbins that you could not force "a destructive current through them in the wrong direction, "then they would not suck so much current out of your machines, "and you might sit down and smoke your pipe while the machine "was quietly fizzling away." That is not the case. You may have heard it said of a machine that it would stand a short-circuit. So it will, if you carefully arrange the short-circuit across the terminals; but if it should occur between one terminal and the first, second, or third bobbin, then you will have fireworks, because the resistance and impedance between those points is nearly negligible; and in practice nobody can ever assume, promise, or guarantee that when you have a short-circuit it will take place between the outside terminals, and that no awkward results will follow. I do not think I need enlarge further upon this subject, except to say that, with regard to the water switch

which Mr. Andrews employs, I have used it under very trying conditions, and I find that you can break 1,000 H.P. at 2,000 volts with that switch without any appreciable spark. What would happen if there were no water in the jar I do not know, because I have never tried it; but I think Mr. Andrews is probably correct in saying that the flash would not get outside the pots. Mr. Haworth.

I made a vow some three or four years ago that, as I had designed more switches than any other living man, my time of switch-designing was over, and I would never design another switch to please anybody. Therefore you will understand that, in speaking approvingly of Mr. Andrews's paper, I have no other object than this—that I wish him every possible success; and I hope the Institution will recognise the enormous amount of trouble and personal care he has given to this subject. He has not merely invented a switch and cut-out, but he has worked out a complete system, which I know, from my personal experience, he would be only too pleased to explain and show in operation, if any of you would favour him with a visit at Hastings.

Mr. R. A. Chattock: I think we must all thank the author very much for his extremely interesting paper. I am sure he has given us a very good idea of what we ought to use in the way of cut-out switch gear. Mr. Chattock.

I have carried out some experiments myself with this kind of switch, with a view to adopting it on the alternators at the City of London Electric Lighting Co.'s Bankside station. These are still unfinished, owing to the winter load interfering with them. I hope, however, to finish them this summer, and am sure that Mr. Andrews's paper will greatly assist me in this.

I feel confident that all station engineers will quite appreciate Mr. Andrews's remarks with regard to the unreliability of fuses arranged as is generally customary with high-tension systems, and also with regard to the absolute necessity for a reliable zero discriminating cut-out for alternating currents; and we must all agree with him that this cut-out switch meets the necessities of the case, both for high-tension alternating- and continuous-current systems.

Personally, I have not had very much to do with continuous

Mr. Chattock. current, but I hope we shall be able to hear the opinions of any gentlemen who have, as to the want of such a cut-out.

With regard to the diagram Fig. 7, if a short-circuit should take place at the point marked B on the inter-connector between the two sub-stations, the cut-outs would not open, as the current would be fed through them both in the direction of ordinary working; the fuses A_1 and A_2 would both probably blow. This could, of course, be overcome by treating each sub-station distinctly, and not inter-connecting. I think that it is the recognised system now in high-tension alternating work only to have the circuits coupled together at the main station 'bus-bar, and not to inter-connect them outside if it can possibly be avoided. All mains should radiate to and from the 'bus-bar in the main station.

Mr. Lawson. Mr. A. J. Lawson: Mr. Andrews, I know, has for many years, been working hard and well in this direction, and I have seen what his switching gear is capable of doing at Hastings. But I do not quite go so far as he does, and say that it is absolutely necessary everywhere. In fact, I think there may be times when in a central station it is not desirable to have to use such very sensitive mechanism as this cut-out when you are paralleling machines. If you are hurrying up to catch a rapidly rising load, it may be that in switching in a machine the cut-out will immediately act, and, the load still increasing, the speed of the engines then at work may fall to such an extent as to render it extremely difficult to synchronise. It is sometimes necessary in such a case to take chances, and to switch in machines when they are not completely in phase. Another point is with regard to inter-connection of mains. If you are to have a system such as is shown in Fig. 7, A, with two cables feeding one station, of course it would be a very reliable system, but it would be somewhat expensive. I prefer, myself, to adopt the system shown in Fig. B. But, as Mr. Chattock pointed out, it may be that both of those cut-outs would act together and cut out both the sub-stations. We, however, arrange it so that, in the case of one fuse on a main feeding one sub-station melting, we can feed round the other way; but, instead of two feeders like that supplying a pair of sub-

stations only, we have perhaps half a dozen sub-stations on each main. Mr. Andrews says in his paper that the use of his switching gear has effected a saving in coal at Hastings during the past 18 months of £400. That is a very big saving in the year by the use of this mechanism. I should like to know whether or no Mr. Andrews has not been able to effect this saving by getting a very much improved load-factor. I have found savings in stations where similar cut-outs had not been in use, of nearly equal amount, with very much increased output, merely due to the increased load-factor. With regard to these cut-outs being more reliable than fuses, that is a point on which I should like to say a word. I have found them sometimes too reliable; they act too quickly. A fuse, on the contrary, has a time constant. These cut-outs, or similar cut-outs, in traction work are so sudden in action that they cut out too quickly, and unless you rig up some bell to warn you, you very often find that someone comes round to your station in a somewhat excited state of mind to demand why you are not giving a supply. Again, with regard to the cut-outs or fuses acting, Mr. Andrews says that the fuse of a defective machine will not blow until the fuses of the other machines at work have blown. He gives us as a reason that, in addition to the current which has to be supplied to that defective machine for the time being, you still have to supply the current to the mains. Well, if you have a short on a machine or mains, what is fed through them is of so very much higher resistance than the fault that the path through them is merely a shunt to the fault; the fuse on the defective machine should act first. Mr. Lawson.

Referring to Mr. Andrews' remarks about sub-stations, I have never yet found any trouble arise in or from a properly constructed underground sub-station.

Mr. F. Charles Raphael: The chief and most interesting part of Mr. Andrews' paper is the description of his latest form of switch gear, and his methods for duplicating mains; but I intend to confine my remarks to one form of interruption to supply, which Mr. Andrews also mentioned, but which he did not dwell upon, namely, the interruptions to individual consumers by their fuses blowing. Mr. Andrews suggested that one should employ Mr. Raphael.

Mr. Raphael. fuses which blow at about 300 per cent. overload, and that that would quite do away with any difficulty of this sort; but I think the fire insurance offices would be quite within their rights if they persisted in objecting to fuses of such high capacity. It must be remembered that there are two sorts of leakage to be dealt with in a house circuit: there is the leakage from wire to wire, and also the leakage to earth from either wire. The leakage from wire to wire can be very simply dealt with by the use of these high-capacity fuses. That sort of leakage is either a short-circuit or it is nothing at all. It is very seldom in the order of the 50 or 100 per cent. excess current for which the fire insurance offices prescribe that fuses should be dimensioned. The leakage to earth, on the other hand, very seldom reaches 200 per cent. overload; but even when it is only a few amperes it may be dangerous as regards fire. Consequently, in my opinion, the way to deal with leakage from house circuits is to endeavour to differentiate these two sorts of leakage—to use our heavy main and branch fuses to cope with the short-circuit leakage, and to endeavour to lead the earth-leakage by another path, so to speak, to collect it and lead it through an automatic device which shall cut the house off as soon as this leakage approaches the value of a few amperes. In this way the consumer would be much safer from interruptions through stale fuses and loose connections, because he would have thick main and branch fuses; but, on the other hand, as regards fire risk, he would also be safer than before, because his house would be disconnected when the earth-leakage only came to a few amperes, and long before it was as high as 50 or 100 per cent. of the normal maximum current. This is not mere hypothetical reasoning. I have thought the thing out and have devised a system which would do this. It is shown diagrammatically in Fig. R. We will take the case of a house wired on the ordinary iron-barrel system, with an iron pipe with continuous joints throughout, so as to offer a path of low resistance from any point of the house circuit to earth. To make this circuit still more complete one could put metallic plates at the back of the fittings and lead the pipe to them; and also in the case of flexible wire

one could use a third wire, still lighter insulated than the other two, connected at one end to the lamp or fitting, and at the other end to the pipe. In that way any current leaking from the flexible wire would be collected by this badly insulated wire and led off to earth. In place of this third wire, one could also use a spiral conductor under the braiding, or else an ordinary conductor, concentric with the axis of the cord; and these two latter methods would, moreover, tend to prevent kinks in the wire. Instead of leading this circuit directly to earth, I propose to lead it through the fuse A (a fuse *with* a time constant), through the brass flap Mr. Raphael.

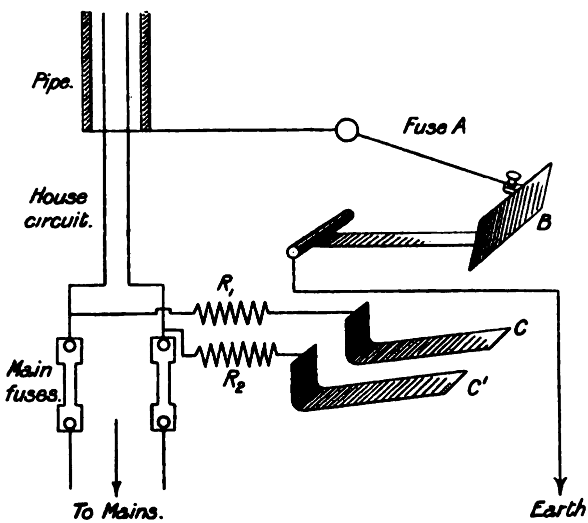


Fig. R

marked B, and to earth. When the current through this fuse is anything appreciable the fuse will melt, and the flap B will fall on the two brass pieces C and C′, and a circuit will be established from the house side of the main fuses through the low resistances R_1 and R_2. These resistances will be given a very low value; they are merely to prevent the central station engineers from having the nuisance of a dead short-circuit on the mains. Consequently, as soon as this current flows, the main fuses will blow and the house will be cut off. In this way the consumer will be protected from

Mr. Raphael. earth-leakage, and at the same time he will have the thick main fuses to which Mr. Andrews attaches a certain importance. It must be remembered that the fuse A normally carries no current at all, but only in the case of leakage; consequently it is not liable to corrode; and, even if the terminals get a little loose and introduce a high resistance, it will not matter, because you want the fuse to blow as soon as there is any current passing through it.

I may say that there is no chance of this system being adopted at all. I have submitted it to several contractors, and they all think it a very good idea indeed; but they point out that this mechanism must necessarily cost a few shillings, that the additional care bestowed on laying down the iron pipes must also cost a few shillings, and in these days of keen competition, &c., &c. The way out of that difficulty would be by using lead instead of iron to surround the wires—either a lead pipe, or else twin lead-covered cables. Then one comes into conflict with the fire offices, who will not allow anything of that sort. I think the fire offices are following a very short-sighted policy by putting so many difficulties in the way of people cheapening house-wiring, or making it less liable to interruptions. They forget that, if only the consumer could get a cheaper wiring, or wiring that is less liable to interruptions, the electric light would be considerably popularised; and a house wired on any system, however badly it is done, is a better risk for the fire offices to take than a gas-lit building. I have put a roughly-made sample of one of these circuit-breakers on the table. The resistances there are 4 ohms each, so that on a 200-volt circuit it would allow 25 amperes to pass from each pole. I may mention that in a big building such devices could, of course, be put on branch fuses also, not necessarily only on the main fuses. It would be preferable, in fact, to put them on the distribution board, so that the whole house would not be cut off if a fault occurred in a branch circuit. The earth contact on the flap B ensures that both fuses will blow, and not merely one, so that the faulty circuit is completely cut off.

Mr. Bathurst. Mr. F. BATHURST: The last speaker rather raised my interest when he suggested his method of preventing interruptions in the

ordinary consumer's premises. We have to thank Mr. Andrews very much for introducing this subject. He has certainly shown station engineers how to keep a continuous supply upon their mains, and how to overcome interruptions that directly concern them. I think wiring contractors should back him up somewhat by trying to devise better mains for the individual consumer, so that the consumer does not trouble the station circuit. If trouble occurs on house wires, let the house be cut out of circuit, for there is no reason why this trouble should be extended to others. Mr. Andrews has shown us how the station engineer can tackle his part, and the last speaker has endeavoured to show us how the contractors should support it. Following the argument of the last speaker, if I understand him correctly, he says we should differentiate between leakage current and short-circuit current, and to do so introduces these fine auxiliary fuses, which he says contractors admit to be a great complication, and therefore probably would not adopt. Being naturally antagonistic to an iron pipe for electrical wiring, I would like to make a suggestion from my point of view. I believe electric wiring should be done in an "electrical" pipe, and that an insulating pipe. An insulating pipe does provide the exact requirements which the last speaker thinks should be provided. Fuses will never differentiate the small leakage currents that can occur in houses—no matter how they occur, whether between wires or to earth. What we have to do is to stop the leakage currents and prevent fires, and provide for the other consideration of "short-circuits" overloading the wires and causing burn-outs. If I may say so, only last week I managed to make at our works, and before the Association of Municipal and County Engineers, some very conclusive experiments showing how easily a plain iron pipe could, in spite of the fuses, be burnt out by a short-circuit, whereas an insulating tube stands very much better. In fact, we have never yet been able to burn one out with the ordinary commercial current. If we have an insulating iron pipe, surely that is the best method of differentiating the leakage currents to earth. We shall keep all current from earth by an armoured, and therefore protected, insulation. We cannot find an insulation which is as hard as

Mr. Bathurst.

Mr. Bathurst. iron, and we have to do the next best thing—namely, take what we have, and preserve it from damage. Having, then, an insulation which will resist leakage, we must face the remaining possibility of overheated wires caused by short-circuits. I think an insulating tube is the best method of subduing the short-circuit "effect." If we adopt this—as Mr. Andrews suggests in his last paragraph—we can, by employing an insulating tube system of wiring, get over the consumer's trouble. I think, incidentally, that our experience up to the present proves that it removes leakage trouble. The fuse does not then become a feature of safety so much as a feature of convenience. It acts in cutting off those wires which have become short-circuited. If contractors could only look at electric wiring from this point of view, we should get some support from the higher authorities, and immediately remove all this trouble from the fire offices. All the fire offices want is a fire-proof system of wiring, and, given it, they should not have any further authority over us electricians, who would then be able to devise our own methods, and go along working out such safe and low-cost wiring systems as should lead to the advancement of the electrical industry.

Mr. Weekes. Mr. R. W. Weekes: When I was at Hastings the year before last, Mr. Andrews allowed me to see the earlier form of the apparatus he has shown to-night. I went through the switchboard connections with him, too, and I happened to take a friend in there with me who had not worked at all with alternating machines. Mr. Andrews allowed him to experiment with the switch in paralleling the alternators. He threw in at the wrong instant three times, and in no case was there any interference observable in the voltage on the 'bus-bars, or any blinking of the lighting. The switch cut-out before the interference between the two machines had risen sufficiently high to upset the resultant voltage. I think Mr. Andrews is rather hard on the other designers of return-current cut-outs,—I do not mean for alternating-current, but for continuous-current working. I remember fixing a small switch at Newcastle in 1888 which would do practically all that Mr. Andrews's cut-out is able to do,

and would cut in as well. It was not worked out in mechanical detail like his is, but it was simply an oscillating lever with mercury contacts. The engine in this case worked both a lift and a dynamo, running all day, charging the accumulators up for the night work; but every time the lift was put on it took the full power of the engine, and slowed it up. The switch then prevented the accumulators working back and driving the dynamo as a motor. I may add that Messrs. J. H. Holmes & Co. made the switch, and have used the type regularly since. Mr. Weekes.

Mr. E. KILBURN SCOTT: There is a reference in the paper (page 490) with regard to the supposed delicateness of magnetic cut-outs for continuous-current stations. Now magnetic cut-out switches have been made for many years by the companies at Wolverhampton, and they are working exceedingly well at St. Pancras, Sydenham, Oxford, Stafford, Burnley, Walsall, Chester, Liverpool, Hartlepool, Nottingham, &c., &c. There is, therefore, no occasion to say that "the majority of those now in "use require too careful and delicate treatment to be popular." I know them, from experience, to be quite workable apparatus, and most reliable over extended periods of time. Mr. Scott.

The CHAIRMAN: Before I call on Mr. Andrews to reply to the various speakers, I propose to make a few remarks myself. As the last speaker remarked, it is perfectly true that magnetic cut-outs have been known for a great many years, and even magnetic cut-outs which would discriminate, for continuous currents, between a current in one direction and a current in another. They have been continually used on switch-boards for the charging of accumulators. Anyone who has tried to step from a fuse that will discriminate for the purpose of accumulator charging, to a fuse that will discriminate for the purpose of alternating-current work, will know that there is a long step to be taken, and that it is not everyone who can design a switch suitable for that latter purpose. At the time when this matter was troubling the City of London Electrical Company in its earlier development, when it had but its old temporary station, I remember very well discussing this problem with the engineers of the Brush Company, and I think I made the remark that there was one instrument Prof. Thompson.

Prof. Thompson.

known which did discriminate, and that was a wattmeter. If you have a wattmeter suitable for alternating currents, when the currents in the fine-wire and thick-wire circuits are in phase with one another, the index hand goes over to one side. If they are in opposite phases, the hand goes over to the other side. So it is clear that if you simply take a wattmeter you can adapt it as a discriminating mechanism which would cut off the current from any dynamo or circuit when that current had reversed in phase. It strikes me that, after all, that is what Mr. Andrews has done. What is his fundamental mechanism but a special kind of wattmeter? He has a thick- and a thin-wire circuit set at right angles to one another, but provided with an iron core to increase the mechanical effect. It tends to go round in one direction when the currents in the two parts are in phase with one another, and tends to turn in the other direction when they are in opposite phases. It is true he does not set it to work as a relay with electric contacts, but he sets it to work as a mechanical relay, letting off with a trigger a loaded lever which gives a big blow to open the circuit. In working out this device—which I can quite well see was not thought out in a day, or a week, or a month—he has certainly conferred a benefit on that part of the electrical industry which is concerned with alternate-current working. Some reference was made—I think, in the first instance, by Mr. Lawson—to the necessity of allowing for a margin of extra current flowing to or from an alternator at the moment you are putting two or more machines in parallel. This operation of paralleling is rather a terrible one for those who have only tried it on a few occasions in a station. Anyone who knows what paralleling is with fairly large machines, and how time may be wasted by an experienced hand in attempting to throw machines into parallel, will recognise the great utility of a device of this kind, which protects the circuit on the machine against the evil consequences which may happen from a miscalculation on the part of the switch-man who is attempting to parallel. I think the last word has probably not been said upon this question of paralleling large machines. I hold that a machine ought to be so well built that it will not come to pieces or injure itself either by

being short-circuited, or being put into circuit and thrown into parallel with machines with which it is not in phase. Each machine ought to be built so strongly as to be able to pull up its own engine. I think either of those qualifications is a much more important one than the qualification which some people are so fond of having—that the machine ought to have so much self-induction that it will choke the current off if anything happens. I do not agree with that view of things at all. As to the reason why in the City of London station certain machines alluded to by one of the recent speakers are not now worked with Mr. Raworth's form of automatic cut-out, I believe the answer is that there are other good and sufficient causes, which are certainly not to the discredit of any machines which were, or are, in that station. When I said that the last word did not seem to me to have been said on the question of paralleling, I wanted you to infer also that there is possibly another solution of that difficulty. I think that the difficulties of synchronising may be obviated by the use of non-synchronising alternators. So far as I know, there is not a single example in this country yet. No contractor or station engineer has yet ventured on the experiment of trying so simple and obvious a thing, namely, using alternators which do not require to be synchronised, but merely require to be run up a little faster when it is desired that they should feed the circuit. They may be running idle simply because they are running a little slow. When you want to throw them in as generators, all that is requisite is to speed them up a little, when at once they give out currents which are in phase with the voltage of the circuit of the other machines. My last remark is this (and I believe that Mr. Andrews will thoroughly endorse it), that a fuse is a very excellent thing—to do without.

Prof. Thompson.

Mr. LEONARD ANDREWS, in reply, said: I thank you for the kind way in which you have received my paper, and for your almost too lenient criticism. Professor Thompson's and Mr. Raworth's complimentary remarks have been particularly encouraging. I must say, however, that, had these gentlemen attacked my suggestions with a view to showing that they were not

Mr. Andrews.

Mr. Andrews.

practical, I should not have been so much at loss for a reply as I am at present. Mr. Raworth has reminded us of the discriminating cut-out he invented five or six years ago. I am proud to say that that cut-out was the mother of the entire system I have had the honour of describing to you this evening. It was Mr. Raworth who first drew my attention to the utter uselessness of ordinary fuses for preventing a faulty generator from damaging its neighbours.

Mr. Chattock has drawn attention to an omission in the diagram Fig. 7. As he points out, a short-circuit on the auxiliary feeder between the sub-station shown at B would blow the main fuses at the works a_1 and a_2, and so cut off the supply to this district. This might be prevented by the use of excess-current cut-outs at each end of this auxiliary feeder, adjusted to operate at half the current required to blow a_1 and a_2. These cut-outs should have been shown on the diagram. Mr. Chattock thought two mains running from one station was a better arrangement than two stations. Mr. Lawson thinks the latter is better, because of the capital outlay. In the station we are now putting down we are running two mains to the one station, and it is my present intention to carry on these two mains to other stations throughout a district of the town, keeping the two mains to each station, and dividing each station into two distinct halves, as shown in Fig. 8.

Mr. Lawson suggests that the discriminating cut-outs on the generators might operate when you want to synchronise in a hurry. We have found these cut-outs a great advantage for synchronising in a hurry, because, if a man has to get a machine in parallel with others in a hurry, he is bound to get a little bit flurried. The fact that the cut-outs are there ready to operate, if necessary, counteracts this nervous tendency. I venture to think that we could show Mr. Lawson much more rapid paralleling at Hastings than he has ever seen in any other station.

Mr. Lawson thinks that the saving in coal may have been effected by an improved load-factor or an increased load. Of course an improved load-factor will tend to reduce the cost of coal per unit, but I do not think it will reduce the total coal bill. In our case, we actually sold last year about 40,000 more units, and

reduced the coal bill by over £200, in spite of selling so many more units. The reason we have been able to effect this saving is perfectly obvious. Before we had the special arrangement of duplicate 'bus-bars shown in Fig. 6, we always used to run three machines for any load over 75 amperes, because, if we had only two machines in, and one broke down, it was rather rough on the remaining machine to expect it to do more than 50 per cent. overload. Now, with this arrangement, we only run two machines for any load under 120 amperes. Anybody who knows anything about central station running will know that you can soon effect an enormous saving by avoiding the necessity of running a spare plant up to speed for about five hours a night. Mr. Andrews.

Mr. Lawson also suggests that, if magnetic cut-outs are used instead of fuses, they might act too quickly. Now magnetic cut-outs can always be depended upon to act promptly when any predetermined current for which they are adjusted is reached, whereas with fuses you never know where you are. We have sometimes had fuses go on less than half their normal load, and at other times they have failed to blow with an excess current of 500 per cent. I think all alternate-current station engineers will agree with me that you can never tell what fuses are going to do. How can a safety device act too quickly to remove a fault which is damaging the rest of the system? Take the case of a machine which fails, and so short-circuits the 'bus-bars. You want to cut it out as quickly as possible, before it robs too much current from the other machines. For feeders, when they are not duplicated, it may be advisable to use safety devices that do not operate immediately, as a main may sometimes burn asunder without blowing its fuses; but if feeders were duplicated as shown in Fig. 7, it would surely be advisable to cut out a faulty section immediately the fault developed. Mr. Lawson thinks that a fuse on a failing generator may sometimes act as it is required to because the generator may so effectually short-circuit the 'bus-bars as to prevent any current going to the mains. My experience of short-circuits on generators is small, but those that have come under my notice have been caused by the insulation of a coil breaking down and starting an arc to earth *via* the pole-pieces. The resistance of this arc was

Mr. Andrews.

usually sufficiently high to prevent the fault from robbing the whole of the current from the mains. Thus the fuse of the faulty machine would merely have to carry the current supplied to the fault, whereas the fuses of the healthy generators would have to carry this current plus the current that would undoubtedly still be taken by the mains.

I am afraid the ingenious safety fuse described by Mr. Raphael would tend to increase rather than decrease the number of interruptions; it might reduce the risk of fire, though I think Mr. Bathurst's suggestion of an insulated pipe would be more efficient in preventing both casualties. If, as Mr. Bathurst said, he can guarantee his conduit to be absolutely arc-proof, then surely there is no need for so many branch fuses where this is used.

I think it was Mr. Weekes who said I was hard on other magnetic cut-outs. I certainly did not intend to be. When I said that magnetic cut-outs as previously made required too delicate treatment to be popular, I was simply quoting an argument that several engineers who have tried these other cut-outs have used when I have advocated their use in preference to fuses. Cut-outs for small currents in which mercury may safely be used have been in satisfactory use for years, but I was referring to cut-outs for large currents rather than to these.

Mr. Kilburn Scott says there are several perfect continuous-current discriminating cut-outs on the market. If this is so, why do continuous-current engineers use so-called zero cut-outs, which admittedly cause them a lot of trouble by cutting their generators out of circuit when a short-circuit occurs on a main outside the works? If these are perfect discriminating cut-outs, I hope those I have described are more than perfect.

Professor Thompson practically condenses the entire gist of my paper in his concluding remark that "fuses are very good "things to do without."

The CHAIRMAN: We have had a very interesting paper from Mr. Andrews, and it has led to a very interesting discussion. We cannot do less than thank Mr. Andrews, but I think your applause has shown that I need hardly put a formal vote of thanks to the meeting.

I have to announce that the scrutineeers report the following candidates to have been duly elected :—

Member:

Robert Thorburn Turnbull.

Associates:

David Armitage.
Harold Reade Braid.
Walter Donovan.
Thomas Hesketh.
Edward Benjamin Hibberdine.

Students:

John Robert Craig.
Kenneth T. Mackinlay.
Alexander Pope.
Bertrice Leonard Roberts.

The meeting then adjourned.

NOTICE.

1. The Institution's Library is open to members of all Scientific Bodies, and (on application to the Secretary) to the Public generally.

2. The Library is open (except from the 14th August to the 16th September) daily between the hours of 11.0 a.m. and 8.0 p.m., except on Thursdays, and on Saturdays, when it closes at 2.0 p.m.

// JOURNAL

OF THE

Institution of Electrical Engineers.

Founded 1871. *Incorporated* 1883.

VOL. XXVII. 1898. No. 136.

'he Three Hundred and Seventeenth Ordinary General Meeting of the Institution was held at the Society of Arts, Adelphi, on Thursday evening, May 12th, 1898—Mr. J. W. SWAN, F.R.S., President, in the Chair.

The minutes of the Ordinary General Meeting held on May h, 1898, were read and approved.

The names of new candidates for election into the Institution re announced and ordered to be suspended.

The following transfers were announced as having been proved by the Council:—

From the class of Students to that of Associates—

Ernest Bernstein.	H. S. King.
Henry T. Constable.	Herbert H. Loraine Prendergast.
H. J. Gridley.	Harry George Whiting.

Mr. R. P. Lovell and Mr. O. Burne were appointed scrutineers of the ballot for new members.

The PRESIDENT: I have now the pleasure to announce that

the Council have elected Mr. Henry Wilde, F.R.S., an Honorary Member of this Institution.

The name and fame of Henry Wilde are written indelibly on the pages of engineering history, and this Institution is honoured in being able to add his distinguished name to those already on the roll of its Honorary Members.

A MAGNETIC BALANCE FOR WORKSHOP TESTS OF PERMEABILITY.

By Professor J. A. EWING, F.R.S., Member.

Prof. Ewing. The author believes that the want is felt of a workshop instrument for making tests, in an easy and rapid fashion, of the magnetic permeability of cast and forged metal for dynamo magnets.

His own permeability bridge,* introduced two years ago, and now somewhat extensively used, allows the B–H curve for a given bar to be determined with very much less trouble than is needed to carry out ballistic tests. For the accurate comparison of one bar with another, throughout a wide range of magnetising forces, the permeability bridge is entirely suitable, and it furnishes as simple a means of performing that operation as can well be had. The author uses it systematically in his own testing, and is thoroughly satisfied with it as a means of determining the B–H curve. But the complete B–H curve is really more than the dynamo builder or the steel founder generally wants to know. For his purpose it would often suffice to find the induction produced by some one (fairly high) value of the magnetising force. That information is a sufficient index of the character of the specimen to allow judgment to be passed on its suitability for use in the field magnets of a dynamo.

These considerations have led the author to develop another testing instrument, which, while it tells less about the specimen than can be learnt by means of the permeability bridge, gives the

* Described in the author's paper on "The Magnetic Testing of Iron and Steel," *Min. Proc. Inst. C.E.*, May, 1896. See also *The Electrician*, May 8th, 1896.

most useful information in a still more easy way. To use it requires no knowledge of electrical testing, and the results need no working out. The value of the magnetic induction, in the usual units, corresponding to a single stated magnetising force, is directly read off on a divided scale. Prof. Ewing.

The instrument is a magnetic balance of the traction type, making use of the principle already applied in magnetic testing in apparatus designed by Professor S. P. Thompson, Mr. Gisbert Kapp, and Professor H. Du Bois. In most apparatus of this kind the specimen has taken the form of a turned bar with a faced end on which the pull due to magnetisation was exerted. In the author's balance this facing of the end is not required, the magnetic pull being exerted between the side of the turned bar and a magnet pole which it touches, and from which it is pulled away. The specimen is a turned rod $\frac{1}{4}$ of an inch in diameter and 4 inches long. It lies across the two poles of a U-shaped electro-magnet, which is excited by a constant current of such strength as to produce a magnetising force in the rod of about 20 C.G.S. units. In one of the poles there is a V-notch for the bar to rest in, and the other pole has a slightly convex surface, being curved to form a portion of a cylinder with its axis perpendicular to the direction of the length of the rod. The side of the rod touches this pole at one point only, and the tractive force at this point of contact is the force which is measured. A lever or weigh-beam is applied to pull the rod away from this pole, while the other end of the rod remains in the V-notch in the other pole, forming what may be called a magnetic hinge. The tractive force is measured by means of a weight which slides along the graduated weigh-beam.

When the rod is put in place, the current is reversed once or twice, to wipe out any residual effects of previous magnetisation. The weight is then moved along the beam until the beam just drops each time it is raised, so as to bring the side of the rod into contact with the pole.

The rod requires no preparation beyond turning it to the proper diameter. Its cylindrically turned side touches the convex pole-face in a perfectly definite manner, and the rod may be taken

Prof. Ewing. out and put back without altering the character of the contact. The lever is arranged in such a way that the rod always touches the same point of the pole-face.

The value of the magnetising force to be brought to bear on the rods under test was fixed at about 20 C.G.S. units for the following reasons:—

At forces much weaker than this the B–H curves of different specimens often cross; in other words, the order of merit often changes when the force is varied. But the author's experience in testing dynamo steel leads him to the conclusion that with forces of 20 units and over there is no serious change in the order of merit of various specimens. If a piece is good when $H = 20$, it remains good under stronger forces; if it is only fair when $H = 20$, it remains only fair; and a specimen that has relatively low permeability under this force does not take a materially better place when the force is increased. On the other hand, any considerably stronger force would be less convenient for testing, especially because the difference between good and bad specimens would become less well marked, and the sensitiveness of the test would consequently be reduced. The author has selected 20 as a force which on the one hand is sufficiently low to make the distinction wide between bad and good specimens, and on the other hand is sufficiently high to make the order of merit substantially the same as is maintained under stronger forces.

From the measured induction at $H = 20$ the probable induction at higher forces can be inferred with some confidence. By examination of the results of tests of a very large number of samples of dynamo steel, including the published tests of Mr. Parshall,* as well as his own tests, the author has prepared the following table, to show the probable approximate values of B at forces of 25, 30, 40, and 50 C.G.S. units, when the value of B at a force of 20 is known. The values of B found for $H = 20$ range, in dynamo steel, from 16,000 in the very best specimens down to 12,000 in specimens of decidedly low permeability. About 15,000

* *Min. Proc. Inst. C.E.*, May, 1896.

is representative of good dynamo-steel castings, and anything below 14,000 may be pronounced poor. Prof. Ewing.

Table I.—PROBABLE VALUES OF MAGNETIC INDUCTION, B, FOR VARIOUS AMOUNTS OF MAGNETISING FORCE, H.

Magnetising Force, H.	Magnetic Induction, B.				
20	12,000	13,000	14,000	15,000	16,000
25	12,700	13,700	14,600	15,500	16,350
30	13,300	14,200	15,100	15,900	16,600
40	14,200	15,000	15,700	16,400	17,000
50	14,900	15,600	16,300	16,900	17,400

The range of the new magnetic balance extends (for H = 20) from 12,000 up to something over 16,000. It will test at the top of its range the very best samples that are found, and at the bottom of the range it will test steel of poorer quality than would be accepted for use in dynamo magnets.

The scale is a linear one, in which equal divisions correspond to equal differences in B, for a constant value of H. It is graduated to give by direct reading the values of B for H = 20. This uniform graduation is arrived at in consequence of the fact that with different specimens the magnetising force is not quite constant, although the current in the electro-magnet is constant. A specimen of high permeability increases the induction in the magnetic circuit, and consequently causes a larger share of the magneto-motive force to be used in that portion of the circuit which lies outside of the specimen itself. Hence the induction in the specimen is less high than its greater permeability would imply; in other words, the better specimen is exposed to a somewhat less magnetising force than the worse specimen is exposed to. The tractive force increases more rapidly than in simple proportion to the actual induction; but matters are so arranged that the lessening of the induction which comes about in the way just stated compensates for this, and the observed

Prof. Ewing. differences of tractive force, as measured throughout the range of the scale, stand in simple proportion to the differences in the values of B which the various specimens would exhibit if the force H were constant. In other words, a scale of equal parts on the weigh-beam corresponds to equal differences of B under a constant magnetising force, and the weigh-beam is accordingly lettered to read B directly in equal divisions. The readings give B for H = 20, although, in consequence of the action just explained, the actual magnetising force is barely 20 for rods of very good quality, and somewhat exceeds 20 for rods of lesser permeability. The scale is adjusted by the maker by selecting values of the sliding weight and of a fixed weight on the weigh-beam which will bring the readings into agreement with the known values of B in certain standard rods.

A single standard rod is supplied with each instrument, and the observer adjusts his current until the tractive force on that rod is such that the sliding weight stands at the place on the beam corresponding to the known value of B which a force of 20 C.G.S. units produces in that standard. The standard rod consequently serves instead of an ampere-gauge, and no other current-measurer is required. A rheostat is provided in the instrument for regulating the current, and a single small storage cell forms the necessary battery. The observer puts in the standard rod, and turns the rheostat until he finds that the weigh-beam just drops each time it is lifted, while the sliding weight indicates the known value of B. He then puts in the rod which is to be tested, and finds the position which the sliding weight has to take for it, no change being made in the current. The constancy of the current is checked at the end of the tests by again putting in the standard rod.

The complete instrument is shown in the figure (Fig. 1). The weigh-beam lifts the rod by means of a V-shaped stirrup close to the pole-piece, from which it is to be pulled away. When the rod is pulled away the beam comes immediately against a stop which limits the motion. A hinged piece is provided under the far end of the weigh-beam, to hold it up while a rod is being taken out or put in. The weigh-beam can readily be lifted out of the way

when it is desired to clean the pole-faces, and care has to be taken Prof. Ewing. to keep them, as well as the side of the rod where it touches them, free of dust and rust.

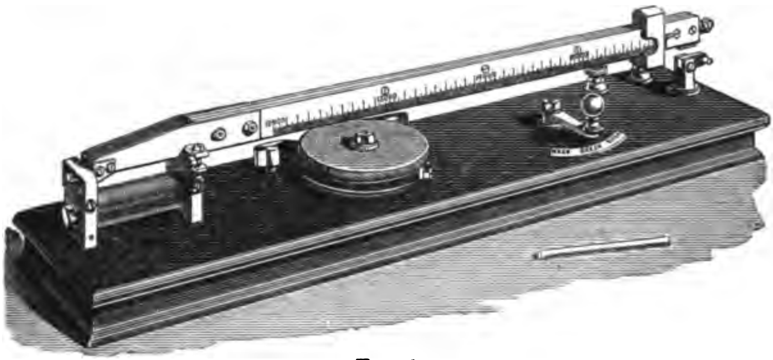


FIG. 1

In the following table a comparison is made, for a number of rods of different qualities, of the values of B known to be produced by a magnetising force of 20 units with the values as measured by this magnetic balance. The known values of B were determined by means of the permeability bridge, by comparing each rod with a standard whose B–H curve had been found in the first instance by ballistic tests. The range covered by these examples is as wide as is likely to be met with in the practical testing of dynamo steel.

Table II.—CALIBRATION OF THE BALANCE.

Induction, B, at H = 20, determined from Independent Measurements.		Induction, B, read from Balance.
13,300	...	13,280
13,400	...	13,400
14,340	...	14,360
14,350	...	14,290
14,600	...	14,470
14,900	...	14,960
15,100	...	15,060
15,080	...	15,150
15,570	...	15,650
15,800	...	15,720
16,000	...	16,000

Prof. Ewing.

These tests relate to different specimens, all tested with one constant current in the magnet of the balance. The agreement between the scale readings and the known values of B is satisfactory. Fig. 2 exhibits the same tests graphically, the readings

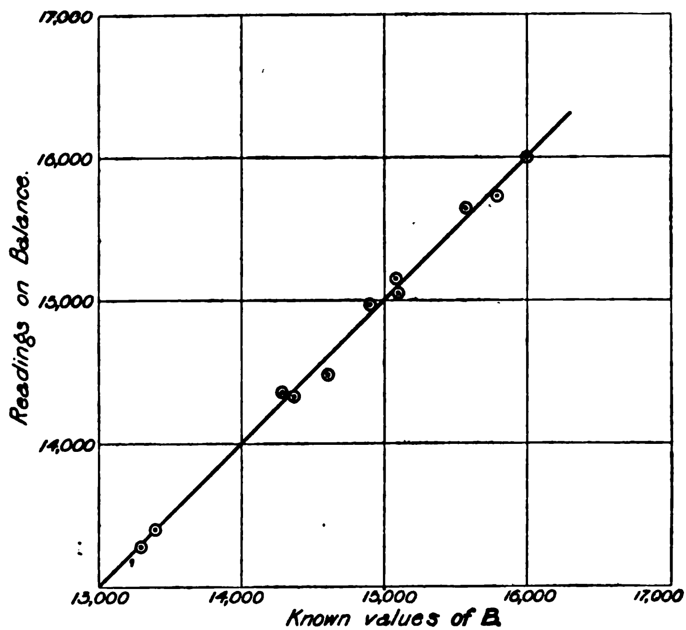


FIG 2.

of the balance being plotted against the known values of B for H = 20. They show that within this range of B the values of the induction (under constant H) are fairly represented by the readings on the uniformly divided scale of the balance. Such irregularities as occur lie equally, so far as can be judged, on both sides of the straight line. The readings of the balance may be accepted as giving values of B for H = 20 at least as accurately as these are required in the uses which the balance is meant to serve.

Prof. Ayrton.

Professor AYRTON: We must congratulate the author, as we have had to do on several former occasions, for bringing before us ingenious instruments for magnetic measurement. One is sur-

prised to see that it is possible to get so long and open a scale on the balance arm for the comparatively small range of inductions that are obtainable in iron with a magnetising force of 20 C.G.S. units. Prof. Ayrton.

A priori one would have expected that the scale would have been much more crowded together, and therefore the instrument would have been less sensitive and, of course, less valuable.

I should like to ask one or two questions. What is the actual force that is dealt with when the sliding weight is in some particular position—the end or the middle, or whatever it may be—what, in fact, approximately is the actual force of detachment that is employed? Secondly, how is the magneto-motive force for an average specimen divided up? What fraction is used in the electro-magnet, roughly, and what fraction is used in the specimen? Lastly—what, of course, would strike everybody—how far is there a risk of error being introduced from the possibility of there being dust or dirt on the surface of the specimen? No doubt, with curved surfaces in contact with one another, the pressure is pretty considerable, and therefore the reluctance of the contact is probably small. But I would like to know what it is, because then one could get an idea of the sort of error that might be expected. A simple plan might be if the author would tell us himself, from his own experience, how far it is necessary very carefully to clean the specimen that has to be tested—how far does a slight trace of oil or of dirt interfere with the accuracy of the apparatus, which, from the experiments that the author has shown us to-night, seems to be very great? I am sure I am only echoing the opinions and wishes of all who are present in offering to the author our sincere congratulations.

Professor PERRY: Before making any remarks myself, I should have preferred to hear what Dr. Thompson has to say. For myself, I must confess I was satisfied when Professor Ewing said he was satisfied with the instrument. He has tested it, he has tried it, he has had an enormous experience in measuring induction; and when he tells us that the errors are less than 1 per cent., I, for one, feel quite satisfied. I should hardly have expected such accuracy as has been arrived at. Prof. Perry.

Prof. Thompson

Professor SILVANUS THOMPSON: Let me join Professor Ayrton in congratulating the author on this exceedingly elegant instrument which he has produced. But I would congratulate Professor Ewing for a second reason. It is now nearly 10 years ago that I described a much cruder instrument, operating on the same plan, for measuring flux-densities and permeabilities by traction; and I am glad to see that Professor Ewing is converted to that mode of measuring these magnetic quantities, in preference to the induction methods that require a ballistic galvanometer. I hoped to be able to produce an instrument which would be suitable for use in the workshop rather than in the laboratory. Professor Ewing has now gone a great deal further in the same direction, and produced a much better and more refined instrument. Now, perhaps, I may be allowed to return to him another compliment, which he was good enough to pay me some time ago, namely, that of criticism. His objection, I think, to my form of permeameter was that I took a rod of iron and put it down the inside of the magnetising coil, letting it touch the block of iron at the bottom, using, of course, a proper magnetic return circuit. The magnetisation was measured by determining the force which was required to detach the end of the rod from the anvil, as we may call it. The criticism which was passed upon that was that I ought to have cut the rod in two, right in the middle of the coil, and have produced the detachment in the very middle of the coil. I had tried both ways before I adopted the simpler, which I found quite good enough. Now Professor Ewing has gone one better, and not only does not make his magnetic contact in the inside of the coil exactly halfway along it, where he told me it ought to be made, nor even at the end of the coil, but right outside on the top of the projecting pole-piece. One other point of criticism, namely, with reference to the mode of contact. I employed simply a sufficiently well-surfaced, flat end, coming into contact with a flat anvil. It was suggested at the time that the state of the surface of the contact might make some difference. But the reluctance of the bad joint was found so exceedingly small compared with the reluctance of the bar used—which was nearly a

Prof. Thompson.

foot long, and thin—that it made no perceptible difference. Professor Ewing has again gone one better there, and has reduced his contact to a mere contact of one rounded surface upon another one. But the point which arrested my attention, and upon which I am going to put a question to Professor Ewing, is this: Having such an exceedingly small area of contact, is there not some error introduced by the gathering of magnetic flux through that very small contact with the lower piece of iron? The iron near the contact must be a good deal more highly saturated than at other parts where the cross section of it is greater. Of course that may not introduce any appreciable error; and any error which might be introduced would be of the same character as the other error which he referred to, and which he most ingeniously applied to straighten out his scale. He has taken the value of the field, H, as 20 as a standard, choosing that number—20—no doubt for very good reasons. The instrument does not pretend to give you a number of points all along the magnetic curve. It gives you values of the flux-density at a definite value of the field. But I would remark that we want to know very frequently about specimens, particularly of cast steel for dynamo building, the values of the flux-density in other fields than those of value 20. If I am not very much mistaken, in nearly all the large multipolar machines which are now being built for traction work, the flux-density is lower than any within the range given by that instrument—lower than 13,000 per square centimetre—and the fields employed are lower than 20. They are, in fact, about 10, or something of that kind; so that they are outside the range of the instrument. *Per contra*, in the teeth, particularly of core discs (of course this does not pretend to be an instrument for measuring the permeability of sheet iron for core discs), the flux-density is run up to 20,000, or even higher, requiring fields far beyond the range of this instrument at the other end of the scale. Those are limitations, perhaps, to the degree to which the instrument is useful. Doubtless a similar instrument equally nice might be made for measuring with a lower field than 20. I have only one other point by way of small detail of criticism of Professor Ewing's paper. I wish I could induce him to use

Prof. Thompson. clarendon type, or block letters, for **B** and **H**, since everybody else does so.

Prof. Perry. Professor J. PERRY: I beg to say that I protest against this general use of clarendon type for induction and magnetic force. I do not know who introduced it; he evidently had the correct notion that clarendon type was good for *vectors*. But in B–H curves and tables it is the mere scalar part of B and H which is referred to, and I protest against the use of clarendon type as a finiking "putting on of side," which is worse than that of the illiterate man who quotes phrases from the Latin grammar.

Mr. Mordey. Mr. W. M. MORDEY: I wish to congratulate the author on this addition to the practical magnetic testing instruments with which he is supplementing his valuable theoretical work on the magnetism of iron; but I am sure he will not long be satisfied with an instrument—however good for a particular purpose—which reads a value only at one point of a variable curve. I would ask Professor Ewing whether in using the instrument before us it is necessary to be particular as to the time the tests take—whether, if one is very slow about it, or very quick, there is any difference in the result. I wish to draw his attention to the problem of testing iron in bulk. It is very convenient to be able to test a bar simply, and the permeameter we have before us, as it is Professor Ewing's work, is sure to be good. But we want to be able to take a forging or casting and test it before any work is done on it. Not only is it difficult or inconvenient to take a test bar from such a mass, but the bar, when obtained, does not necessarily represent the quality of the mass. Either it is different in composition, or it is affected in its physical condition. I would therefore ask Professor Ewing to increase our indebtedness to him by giving us an instrument which will enable us to make tests on large blocks of iron. It is not unnecessary. It is supposed that iron and steel supplied for magnets are always very good; but those of us who have had experience on a pretty extensive scale, know that sometimes it varies very much. Let me give you an example. Two machines were made exactly alike—very large, heavy machines. The steel castings were supplied at the same time, by the same firm; and

the machines were identical in every external respect. The exciting watts differed 70 per cent.; that is to say, the ampere-turns differed about 13 per cent. When one has to work to a close specification as to excitation, such a difference is very serious indeed, is a great inconvenience, and may be enough to lead to the rejection of a machine which has cost a good deal of money. In saying that we want an instrument to test iron before we do any tooling work on it, I would say that we want to be able to order iron of a certain magnetic quality, and to see that we get it. We want to order, not by some loose general description, such as soft Swedish charcoal iron, or mild steel. We do not mind much about the mechanical qualities, but we do want certain definite magnetic qualities. The only way to get that is to have a definite specification of quality, and to be able to test that quality. If I am not going beyond the scope of the paper, I should like to give an illustration of the effect of purchasing iron in terms of magnetic quality, instead of in terms of a mere description of what it is supposed to be in character. When the making of transformers commenced on a considerable scale, the Swedish iron was very good; but when the demand for transformers increased, the iron all over the world got worse. I suppose the demand for good soft Swedish charcoal iron, or other good magnetic iron, was too much for the supply at the time. The quality deteriorated, and as a result transformers of a given size would vary very much in their iron losses. Variations from 1 to 1·4 and 1·5 were quite common, and even 1·8 was not uncommon. We had some very uncomfortable results. The Brush Company accepted orders for transformers on a certain specification of loss, which would have been quite easily complied with but for the change in the quality of the iron, and the company was penalised under a specification drawn by a member of this Institution—and a very good specification too—which demanded that something should be supplied that the user wanted. From a cause over which the manufacturers had no control they were penalised very heavily. I then advised that, instead of ordering simply charcoal iron, or any other quality of iron, we should order a material of a certain definite magnetic quality as to losses, specified in watts

Mr. Mordey.

Mr. Mordey. per lb. at a certain magnetisation and a certain number of periods per second. The iron manufacturers objected that they did not know what B meant: they knew nothing about periods nor watts; and that everything had been done that could be done to provide good iron. They did not like the specification, but eventually they accepted it. All deliveries were tested, and a penalty was imposed or a bonus added according to quality below or above the specification. The result was that in the course of a few months the quality crept up from the very bad and irregular iron that was being supplied to the whole trade, to iron that did not vary more than 5 per cent. from the specification which was fixed at that time, where it has remained ever since. You may ask how the iron was improved. It was improved by the highly scientific method of giving the makers an actual interest in improving the iron, and by paying for what we wanted, and by constant testing to see that we got it. I think if Professor Ewing would get out an instrument to help us to specify iron for magnets, and to test it, with as much ease as we test transformer iron by a wattmeter, he would be doing a very great service to electrical engineers.

Mr. Swinburne. Mr. J. SWINBURNE: I should like to point out that I think the popular method of testing induction in iron by the pull is really due to Bosanquet, though I do not know that he thought of it in a way that was of any use. He was working at verifying the relation of the pull to the induction—a matter that could not need verification, following as it does from definitions. But he was, if I remember right, led to a wrong conclusion by the distribution of the induction being non-uniform, so that the pull was too great for the total induction. The distribution of induction at the contact must interfere with the accuracy of Professor Ewing's 1898 instrument, as, though the best part of the excitation may be spent on the body of the bar, unless the curve of a sample is the same, or a multiple of the curve of the specimen, the reading must be more or less inaccurate. Professor Ewing may have got good results by trying samples which had curves corresponding with the standard, but unless there is this correspondence there must be inaccuracy.

Professor AYRTON: It seems to me, if I may say so, Mr. Swinburne, that that is the whole interest of the apparatus,—that while *a priori* rather large errors might have been anticipated, the numbers given by Professor Ewing in his paper appear to show that such is not the case. Prof. Ayrton.

He actually gives values, not merely to one in 100, as stated by Professor Perry, but to one in 1,000; therefore, within the range of induction that the instrument is intended to measure, this crowding together of the lines of force at the contact does not seem, for some reason or other, to have produced any error. I think that is, I will not say the main, but at any rate the great interest of the apparatus, in an accuracy being obtainable with its use which, according to the author, is much greater than one would have expected *a priori*.

Mr. S. EVERSHED: I did not intend to speak on this paper; but I think the natural criticism from the dynamo manufacturers' point of view has been very well put by Mr. Mordey, and I will not add anything to what he has said. Mr. Evershed.

The PRESIDENT: I congratulate Professor Ewing on the production of an admirable workshop instrument. Now that dynamo construction has become a common variety of mechanical engineering, it will no doubt be found widely useful as a means of easy discrimination between magnetically good and bad iron. The President.

The members have seen how easily Professor Ewing manipulated his balance. Yesterday I had the opportunity myself of proving the ease and precision with which the apparatus can be used. It seems to me that Professor Ewing has most happily combined extreme simplicity in the method of using with practically accurate results.

Mr. R. A. HADFIELD [*communicated*]: A handy instrument of the kind described by Professor Ewing should be of considerable service to manufacturers, who have in many cases not sufficient time to take long and exhaustive tests. With the Hadfield high permeability material we are getting very special results, so I suppose the magnetic balance in question will take up to 18,000 B, for in tests which have been obtained by Professor Ewing we ran up to this; and similar results have also been obtained by Dr. Mr. Hadfield.

Mr. Hadfield. Hicks, F.R.S., of the University College, Sheffield. It may be interesting to electrical engineers to see the advance metallurgists are making.

Whilst not quite germane to the question, it would be useful if Professor Ewing could also prepare an apparatus for determining electrical resistance, as the measurement of this quality is also being found of considerable importance to metallurgists.

Mr. Hartnell. Mr. W. HARTNELL [*communicated*]: Professor Ewing's permeability bridge was an excellent and much-needed instrument, but the magnetic balance appears to be of still more general utility. I can but admire the simple manner in which the test pieces automatically adjust their contact surfaces, free from practical error; so that the results are as accurate as they are easy to ascertain.

Hitherto the makers of magnet iron appear satisfied to have had their material tested for its magnetic qualities at long intervals, and meantime they seem ready stoutly to maintain that the magnetic quality of all they supply is equally good, so long as their chemical analysis shows nearly the same composition. The experience of dynamo makers shows that this is far from being the case. Although a fair average quality may be maintained by the more experienced magnet-iron manufacturer, occasional specimens are above the average; and not a few embarrassingly below. I could give many examples.

The magnetic quality is usually not discovered until the dynamo is tested. An inferior quality may require readjustment of the windings, or alteration of expensive driving gear, or even rejection of the dynamo for the purpose intended.

If, however, the magnet manufacturer accompanied every delivery of material with a certificate of its magnetic quality, it would save the dynamo manufacturer from much anxiety, and often from much unnecessary expense.

The advent of Professor Ewing's magnetic balance should render such a certificate easy to obtain.

A striking instance of the necessity for magnetic tests by the magnet iron makers is impressed upon me by the fact that the worst material for magnets I have received for some years was

recently delivered by a firm who are eminent by their published researches and chemical analyses. Their test curve (by Professor Ewing) is one of the best I have seen. Mr. Hartnell.

Hence, in my opinion, dynamo makers and designers should insist upon magnetic certificates with the magnet iron delivered, in the same way that inspectors and boiler makers insist upon "test certificates" of strength, extension, and elasticity of the steel they purpose using.

Professor Ewing's magnetic balance seems to give all that is required for ordinary purposes. The inexpensiveness and simplicity of the tests made by its means renders the demand for magnetic certificates not unreasonable.

May I be allowed to express my sense of obligation to Professor Ewing for providing this convenient and useful instrument?

Mr. C. C. HAWKINS [*communicated*]: On referring to various B–H curves of dynamo materials, the question arises how far the determination of B for a single value of H really meets the requirements of the dynamo manufacturer, especially in the case of steel castings. In the *Electrician* of September 17th, 1897, six curves for steel castings were published, all taken by Professor Ewing; four of these practically coincide in the value of B = 15,000 for H = 20, but when H = 50 the values of B range from 16,500 to 17,200. This may not appear a very wide difference, yet it implies that when B = 16,500, which is not an abnormally high value for the induction in cast-steel dynamo magnets, H varies from 50 down to as low as 32—a very large percentage difference. The designer can then only fall back on Professor Ewing's table of probabilities, which gives the intermediate value of H = 41; yet this would introduce a considerable amount of uncertainty in the total ampere-turns of a short-air-space dynamo. Would it not be possible for Professor Ewing to arrange his permeameter so as to give at least two determinations of B, one for a value of H considerably greater than 20—say 40? Or, if only one value is retained, ought not this value to be higher than H = 20? Mr. Hawkins.

Mr. R. JENKINS [*communicated*]: There is no doubt whatever Mr. Jenkins.

Mr. Jenkins. that the efforts of Professor Ewing in the way of producing instruments for testing iron for electrical purposes, these instruments being easily worked by non-professional people, are a great help to the trade.

In contracts which I undertake I always specify Professor Ewing's instruments to be used, because by their use manufacturers can ascertain a general average of the quality of the material being supplied. The qualities of a material for a given purpose are easily arrived at; and the works I represent are reinforced now by these instruments, which they find of the greatest possible use in the fixing up of contracts where electrical tests are concerned.

Mr. Scott. Mr. E. KILBURN SCOTT [*communicated*]: With reference to the point raised by Mr. Mordey in the discussion on Professor Ewing's paper, on the importance of knowing the magnetic qualities of soft magnet steel *in bulk:* It would appear that the variation in magnetic quality has much improved of late, owing to the steel makers having come to the conclusion that it pays them to make special blows of steel, instead of using any mixture they may have in hand at the time. The South Staffordshire Steel and Ingot Company, for example, who turn out very large quantities of soft magnet steel, appear to have now found out exactly what is wanted, and, by taking a little trouble with the mixture and with the chemical analysis, the quality is practically constant for each "magnet steel blow," which they make at frequent intervals. The following is the average analysis :—

Carbon	...	..	0·075	per cent.
Manganese	...	...	0·380	„
Sulphur	...	...	0·075	„
Phosphorus	...	...	0·060	„
Silicon	...	...	trace.	

At the same time it would undoubtedly be a great advantage for electrical firms to be able to make their own tests of the magnetic quality of castings and ingots *in bulk;* and, in the absence of any such method, the writer thinks that perhaps the following suggestion may be interesting :—An electro-magnet similar to those which are now taking the place of crane

hooks, but with tapered and rounded poles, is suspended over an ordinary weigh-table. When the ingot or casting comes forward to be "weighed in" by the stores department, two places are fettled up, at say, about a foot apart, for the electro-magnet poles to rest against. After the casting has been weighed in the usual way, the magnet is brought against it, and a definite known current is switched on. The weights are then run off the scale-beam in the weigh-office, until the casting drops away from the poles of the electro-magnet. Mr. Scott.

It seems to the writer that this is roughly analagous to the traction method of testing as used in Professor Ewing's instrument, and that the difference between the actual weight and the weight at which contact is broken is roughly a measure of the magnetic quality of the steel lying adjacent to the pole-pieces of the electro-magnet.

Professor EWING, in reply, said: Let me in general terms thank you, Sir, and the various speakers, for the very cordial reception you and they have been kind enough to give to this instrument. Professor Ayrton has commented upon the comparatively large range of scale. Even with the slider as near as it can go to the fulcrum, there is still a large force due to the preponderance of weight on the part of the beam itself. The sliding weight merely serves to measure the comparatively small differences between the smallest and largest amounts of attraction. The slider has its weight adjusted by taking two specimens whose quality is known, and making the weight of the slider such as will give the desired range of movement between the two standard specimens. Then the beam has its weight adjusted so that one of the standards comes to the desired point of the scale. Prof. Ewing

It is, of course, quite true that there is a great crowding of the lines of induction at the place where contact occurs, and no doubt a large part of the magneto-motive force is used up in overcoming what we may call the resistance of the joints at the points of contact. For that reason, I do not think the instrument would probably serve very well if one attempted to apply it to the measurement of permeability under low values of the magnetising force, because in that case we should have this complication—that

Prof. Ewing. the main part of the specimen would have a low force applied to it, whilst the joint, which is a large part of the whole resistance, would have a high force applied to it, and a high induction through it. But as things are we have a force which is high within the specimen itself, as well as at the joints. What we have is a force 20 over the greater part of the length of the specimen, and then a force which is no doubt very much more than 20 over a small part, namely, the part in the immediate neighbourhood of the point of contact. The induction which the specimen takes up depends on both of these constituents of the magnetic circuit; but if the bar has a good permeability with reference to one of the forces, it will also have a good permeability with reference to the other, since both forces are high. I confess I was just as much surprised as other speakers have been to-night when I found it was possible to get a linear scale which would be a reasonably correct representation of the induction due to a standard magnetising force. What I at first expected to do when I designed the instrument was only to get a means of comparing one specimen with several others. I intended at the time to supply with the instrument several specimens of known quality, and so, by comparing any sample with these, to get a good general idea of its relative position. I did not look for anything so simple as a linear scale of B, and it was more or less by a happy accident, after several trials, that this linear scale of B was arrived at.

Another matter referred to was the influence of dirt or dust at the possible contact. It is necessary that the specimen should be free from rust and oil where it touches the poles. But there is no difficulty in wiping it sufficiently clean for the purpose. Further, the instrument has been constructed in such a way that the lever can be lifted bodily off, and then one can very easily rub up the pole and keep the surface of contact bright.

An interesting point which the President has raised is whether the quality of the polish of the turned surface makes any important difference in the results. I have tested several specimens with the surface in different states. In some cases the tool mark of the last cut has been left on the specimen, and in

other cases the surface has been polished, and I have not been able to detect any serious difference between the two. Prof. Ewing.

Professor Thompson speaks of my conversion to traction methods. It is true I was unprepared to believe that traction methods could be so successfully employed as I now think they can be, for purposes of comparison. But I would still deprecate the use of traction methods as a means of determining B in an absolute measure without any reference to ballistic tests. In most of the permeameters which use traction methods, the attempt has been made to apply a direct measurement of traction to evaluate B, on the basis of the relationship which Joule first showed to exist between the traction and B; that is to say, the stress at the magnetic joint has been measured absolutely, and from that B has been calculated. I do not think that traction methods are satisfactory in that application. All that I do here is to use the traction method as the means of comparing one bar with another, that other bar having had its value of B determined previously by ballistic or other methods. The distinction is an important one, and it is only to that extent that I admit a conversion to the use of traction methods.

It is, of course, quite true, as Dr. Thompson points out, that this instrument does not give points at the B–H curve—or, rather, it only gives one point at the B–H curve; it only gives the value of B for H = 20. If the B–H curve is wanted, it is very conveniently and accurately found by using the permeability bridge. I should be sorry to think this instrument was going to put the permeability bridge out of use. If the relation between B and H throughout the curve is wanted for any purpose—as, for instance, in the case Dr. Thompson mentions, where the induction lies below what this instrument measures—the permeability bridge is distinctly the instrument to be employed.

Mr. Mordey has raised a very important point in speaking of the desirability of being able to obtain tests of magnetic material in bulk. It is an unsatisfactory thing that the tests which so far we have been able to apply, have been only to small specimens. The further question is always present, How far does the specimen really represent the large casting or the large forging which

Prof. Ewing. is supplied to the dynamo maker? It is an open question, and I fancy that dynamo makers could point to instances indicating that specimens do not always fairly represent the quality of castings or forgings. Mr. Hartnell's experience is particularly interesting in this connection. Blow-holes in a casting may make the result given by it poor when compared with the result that has been given, quite fairly, by a specimen cast from the same melting of steel. The problem Mr. Mordey suggests of measuring the magnetic quality of steel in bulk, is one I have often considered, but so far without seeing the way to any solution.

Mr. Mordey also touched on a subject which, though perhaps not quite relevant to the paper, is one which it would be of great interest to have a discussion upon, namely, the hysteresis losses in sheet iron. This instrument is not designed to tell us anything about sheet iron. It is only intended to deal with metal which can be turned into cylindrical rods. The hysteresis loss in sheet iron is a subject to which Mr. Mordey himself has contributed a very important item of knowledge in his experiments on the effects of prolonged heating. If it had come within the scope of the paper, I should have been glad to say something about recent work done in my own laboratory on this subject by Mr. Roget, who read a paper about it before the Royal Society to-day.

May I quote, before sitting down, a sentence from a letter just received from Berlin from Mr. Gisbert Kapp, whose experience in magnetic testing and in dynamo design gives his opinion particular weight? He says: "I think your balance is a most "useful invention, and I should not be surprised to see it largely "taken up over here."

The instrument now on the table will remain for some time on view with Messrs. Elliott Bros., who have undertaken the sale of the new magnetic balance.

The PRESIDENT: I ask you to express your indebtedness to Professor Ewing by a vote of thanks.

Carried by acclamation.

The PRESIDENT: I hope we may look forward to a further communication from Professor Ewing with regard to the hysteresis losses.

The following paper was then read :—

THE REGISTRATION OF SMALL CURRENTS USED FOR ELECTRIC LIGHTING OR OTHER PURPOSES.

By ALFRED H. GIBBINGS, Member.

Mr. Gibbings.

The subject which I have the honour of bringing before your attention this evening is one which is becoming of considerable importance to the suppliers of electrical energy from central stations. This arises chiefly from the fact that several of the electricity supply concerns have recently increased the pressure at which they supply current to the consumer, or, in other words, they have raised the voltage on the distributing mains. One of the most important reasons, however, which has brought about this change—viz., the economy effected in distributing mains through the halving of the current hitherto required—is in itself responsible for the imperative necessity of greater accuracy in the registration of the smaller current consumed. I refer, of course, to the change from a supply pressure of 100 volts to that of 200 volts, and to the proportionately less current required for lamps in connection therewith.

In looking through the list of consumers of any undertaking for the public supply of electricity, it will be found, if the business has been carried on for more than two years, that the maximum demand for current by the great majority is considerably below 10 amperes. If, however, we include all those consumers who would come within a 20-ampere maximum demand limit, there will remain but a very small percentage requiring more than that amount. If these facts be true for supply at 100 volts, it is evident that at 200 volts pressure the 10-ampere limit will practically include all consumers.

The question, however, with which we are more concerned is the registration of the minimum current which the consumer uses when he has only one or two lights in circuit, and also that this registration should be accurately proportionate throughout the capacity and range of the instrument. There is, moreover, the consumer whose demand does not exceed 3 amperes—say 10 16-C.P. 60-watt

Mr Gibbings.

lamps—and who probably does not, as a rule, have more than half that number in use at one time. The revenue derived from such a consumer is small, and the net profit does not permit of providing him free of charge with an expensive meter. The consumer on his part either strongly protests against, or is unable to afford, a heavy meter rent. In a recent discussion on meters, at the last annual convention of the Municipal Electrical Association, the following remarks were made by Mr. Evershed:—"As re-"garded energy meters as against quantity meters, the quantity "meter was cheaper to make, and, therefore, for small consumers "it seemed to him to be greatly desired. He agreed that the "small consumer must have a meter to cost less than £5, but "where it was to come from he did not know. There would "always be a large demand for meters for big consumers, and "meters which must have a very long range, and it appeared to "him to be folly on the part of engineers to expect a meter of "that kind to be made at a low cost. They could not have them "cheap and with a long range. But the small consumer did not "want a long range. If the price was to come down to the "lowest level good work would have to go, and he was afraid they "would find themselves very much mistaken if they thought that "good meters would become very much cheaper than they are "now. It was true the workmen were getting more skilled, but "the rate at which wages were rising entirely wiped out the "advantage that was gained by that fact, and even the labour-"saving appliances were hardly sufficient to atone for the rise of "wages. He had no hope of the price coming down to 50s. or "40s., as he had heard suggested." We are, therefore, confronted with these two desiderata, viz.:—

1. A meter that will register accurately at all atmospheric temperatures and at all loads, from $\frac{1}{10}$th ampere to its range limit.

2. A considerable reduction in the capital cost of the meter.

Such a meter I propose to bring before your notice to-night. I shall endeavour to show, by reference to certain tests, &c., which I have carried out, that this meter fulfils the conditions I have just mentioned. In order to make the description as clear as

possible, and to do justice to the consideration of every point, it will be best to treat each feature under a distinct and separate heading. I will, therefore, deal with the subject in the following order:— Mr. Gibbings.

1. Principle of action and registration.
2. Construction and probable cost.
3. Tests.
4. Summary.

I.—Principle of Action and Registration.

The principle which has been applied to the registration of the electric current in this meter is that of electrolysis, or the decomposition of a liquid. This effect of the electric current has not only been known from almost the inception of the science, but it has been applied to the purposes of registration in many cases, and forms the subject of many patents, as I will presently describe. The method of registration is that of the difference of level of the electrolyte due to electrolysis and observed by a graduated reading of the tube containing the liquid. My object this evening will be to show that the application and method of registration, however, which has been adopted in this meter is novel, and that as an ampere-hour meter it has advantages over those in which other principles are utilised. Let me say at once (though, indeed, this is scarcely necessary) that it is only applicable to the registration of continuous currents; but even as such, and notwithstanding its confined scope, it is still an advance upon present methods. We have at the present time meters which employ those effects of the electric current which render them equally available for use with either continuous or alternating currents; and we have, moreover, various forms of wattmeters. All these perform their functions sufficiently accurately to be articles of commercial value. With these aspects of the case, therefore, the present paper has nothing whatever to do, and hence the following remarks should not be criticised from such standpoints. In making a comparison between this meter and other types of ampere-hour meters, as far as the restricted points of consideration admit, I will, at the

Mr. Gibbings. outset, state concisely the characteristics which are claimed for it, viz. :—

1. Starting with an infinitely small current, and stopping immediately current is switched off.
2. Accurate at all temperatures and at all loads.
3. No periodical testing required for starting current.
4. Independent of direction of current.
5. No mechanism.
6. No permanent magnets.
7. No shunt currents.
8. Cheap in initial cost.
9. Cheap in maintenance.
10. No special adjustment for different lamp voltages, except on calibration of scale.
11. Accuracy unaffected by temporary excess current.
12. Unaffected by local short-circuits.
13. Unaffected by outside influence.
14. Not susceptible within wide limits to vibration, temperature, or barometric changes.

In support of some of the foregoing features, evidence will be forthcoming in the portions of this paper devoted to "Construction" and "Tests" respectively. For the moment I will deal with those which require little or no proof in support of the claim, and which are numbered 1 to 7 inclusive in the preceding list. The first four claims are indisputable, as they arise out of the fundamental laws of electrolysis. The one fact alone that no periodical testing is required for starting current, is of immense importance in itself, as the necessity of the inspection which exists at present forms a considerable item in the costs of the meter department. Further, the result of this inspection is often enough the removal and recalibration of the meter, and on this matter I shall have more to say when I deal with the question generally. With regard to items 5, 6, and 7, it is apparent at a glance that the removal of all mechanism, and the absence of permanent magnets and shunt currents, render the meter free from many errors which arise from those causes in most of the existing types.

Before proceeding with the second division of my paper, I propose to give herewith a list of all those electrolytic meters which have been invented since the beginning of the year 1883. Many of these applications of electrolysis are characterised by the unique and ingenious nature of their construction and their registering devices, as will be seen from the extracts which are given below. Mr. Gibbings.

(Butler)—Acidulated water is electrolysed, and the pressure of the gas generated is caused to actuate the counting mechanism. Arrangements are also made for periodically exploding the gases.

(Boucher)—Acidulated water is electrolysed, and the gases given off are caused to pass up into an inverted funnel which is placed under the water in a tank. This funnel is connected to a lever which swings loosely upon a pivot. When the funnel becomes filled with gas, the lightness of the gases causes it to tip up so that the lower end of the funnel comes above water, and allows the gases to escape, and simultaneously the movement of the lever attached to the funnel actuates the counting mechanism, either mechanically or by the agency of an electro-magnet.

(Wright)—An electro-magnet is wound with a very fine coil, which is connected with a shunt across one parallel or set of parallels, and an electrolytic cell is placed in the circuit, the plates of which are removable and can be weighed, so as to obtain a measure of the energy absorbed by the circuit. Used with high-tension incandescent lighting when the lamps are arranged in multiple series.

(Greenhalgh)—Two electrodes are placed in an electrolytic cell, of which one is fixed and the other movable. The weight of the metal deposited upon this latter electrode is caused to actuate the registering mechanism. This is effected by the agency of a ratchet-wheel and paul, which in its turn controls a train of wheels.

Mr. Gibbings.

(Shippey)—Acidulated water is electrolysed, and the pressure of the gas generated is caused to move an indicator. It is mentioned that a pencil may be used to trace a line on a paper mounted upon a drum, and thus obtain a register. It is also stated that the gas generated may be measured by passing through a gas meter.

(J. Swinburne)—Relates to arrangements of meters for direct or alternating currents. The meter consists of a voltameter or copper deposition apparatus, the current through which traverses a variable resistance, which is controlled by the core of a solenoid, or by the expansion of a wire heated by the current passing through it. Or the meter may be operated by the secondary circuit of a small transformer which traverses the voltameter, and is made and broken by a suitable contact-breaker.

(Fairfax and Wetter)—Refers to electrolytic meters used for measuring alternating currents. Metal is precipitated from one or both of the electrodes, and any convenient method of indicating the loss of weight in the electrodes may be employed, such as attaching a delicate spring balance to the plates, or connecting the plates to a system of counterweighted levers.

(Lowrie & Hall)—Also refers to alternating-current meters. This specification deals chiefly with means of differentiating the wave of potential of an alternating current, so that the quantity of current flowing in opposite directions is unequal, and to utilise this difference to measure the amount of the total current flowing. The specification states that an electrolytic cell may in some cases be employed in order that the gain or waste in weight of an electrode may measure the amount of current.

(Sellow & Jackson)—A liquid, such as acidulated water, is electrolysed within a U-shaped tube which is closed at both ends. At one end of the tube is fitted an arrangement for causing the pressure of gas to actuate

any suitable registering device, and at the other an arrangement for periodically exploding the gases which are generated. It is mentioned that the pressure of gas may be used to give motion to float or piston, or, by closing electric circuits, to actuate electro-magnetic devices. Mr. Gibbings.

(W. Emmott)—The gas or gases liberated by means of electrolysing a fluid are caused to turn a sort of water-wheel which actuates the registering mechanism.

(C. S. Forbes)—One or more secondary batteries are so arranged that either the whole or a part of the main current passes through them, a proportional part of the current being stored by these batteries.

(C. P. Elieson)—Water or other liquid is electrolysed, and the amount of current which has passed is ascertained by metering the gas generated.

(Dr. Smelles)—This meter deposits metal upon the electrodes alternately, and is fitted with a commutator arrangement which changes the relative position of the anode and kathode. One of the electrodes is movable, and the changes of weight of this electrode actuates the registering mechanism.

(H. W. Miller)—Between the electrodes (in an electrolytic bath) is placed a balanced metallic cylindrical wheel. When the current passes it deposits metal upon one side of the cylinder and dissolves it from the other side, thus causing the cylinder to slowly revolve by upsetting its equilibrium. The motion of the cylinder is used to actuate the registering dials.

(Grassot, Paris)—The tip of the lower end of a straight vertical wire, placed within a tube, dips into an electrolytic bath and forms one of the electrodes (the anode). The above-mentioned tip is provided with an insulating support to rest on in the electrolyte, the lower end of the wire being slowly dissolved by electrolytic action. The wire descends by gravity, and this motion is caused to register on dials.

Mr. Gibbings.

(A. E. Waterhouse, U.S.A.)—This meter electrolyses fluids, and the gas decomposed is caused to actuate registering apparatus. Arrangements are included for automatically discharging the gas after a certain amount has been collected.

(McKenna, U.S.A.)—This meter consists of an electrolytic cell containing a mercury salt, from which mercury is caused to be deposited upon an electrode composed of carbon (the other electrode consisting of mercury), and in the form of a round rod placed vertically, and terminating in a point at its lower end. Beneath this carbon electrode is placed a graduated glass tube into which the mercury drops, the amount of mercury in the tube showing the quantity of current which has passed.

(Alders & Hottgen, London)—This meter is almost precisely similar to McKenna's, but instead of an electrode of carbon for the mercury to deposit upon, a platinum electrode is substituted, conically shaped, and with the pointed end downwards over the indicating tube.

(Naber, Amsterdam)—Water is electrolysed, and the gas collected and measured. A thermometer is mentioned, also table, as being used in reading the meter (probably for correcting temperature errors).

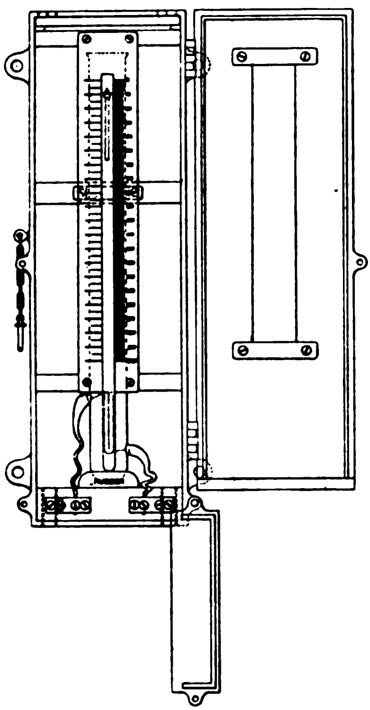
The foregoing details show very clearly the importance in which the electrolytic action of the current has been held as a principle of registration. The application, however, has been entirely confined either to electro-deposition of metal, or to the measurement of the gases given off in the process of decomposition of the liquid. In the first case so many difficulties arise that only one type has been adapted commercially as an ampere-hour meter for electric lighting purposes, and that with very indifferent success. In the second instance even greater disadvantages have to be considered owing to the large errors introduced due to variations of temperature and complicated and 'icate mechanism.

II.—Construction and Probable Cost.

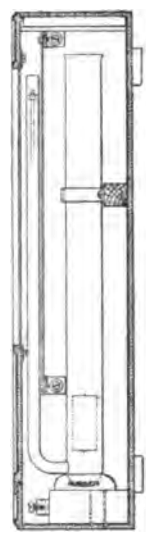
Mr. Gibbings.

Construction.—The general construction and arrangement of this meter are shown in section in Fig. 1, and from photographs in Fig. 2.

There is also on the table before you a skeleton meter consisting only of the electrolytic portion, which I have arranged so as to demonstrate, as far as practicable at the present time, one or two of the principal features of the instrument which I have already enumerated.



Front Elevation.



Section.

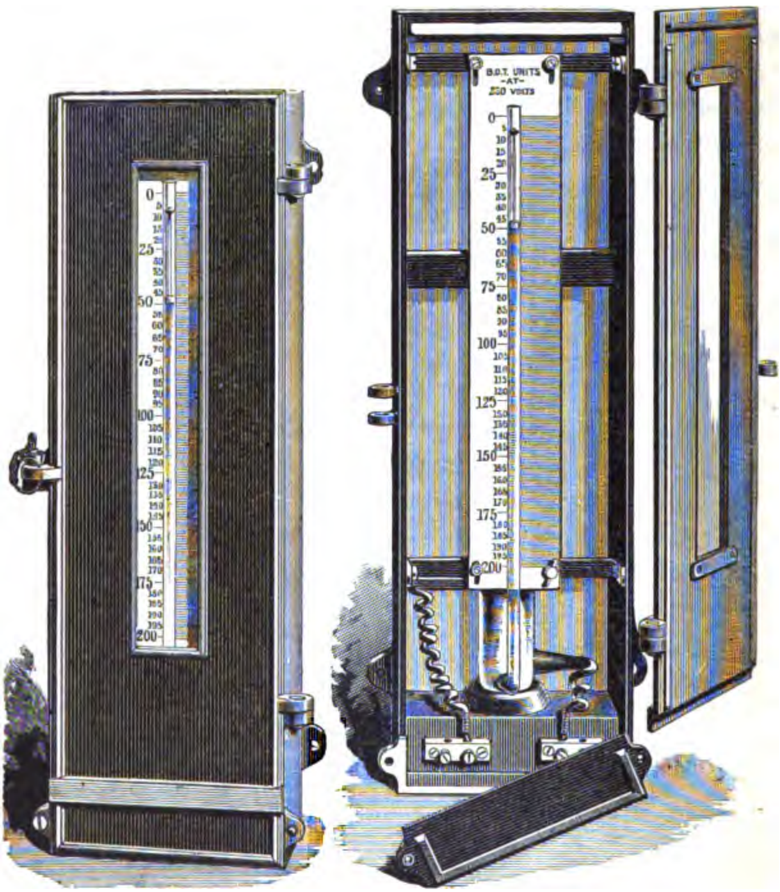
Fig. 1.

There are also a few samples of the commercial article before you, which have been calibrated in Board of Trade units, at 115 and 230 volts respectively, and which I will now briefly describe.

The electrolytic apparatus consists of the usual platinum electrodes mounted in a glass tube of true bore throughout the

Mr. Gibbings.

range of the scale. The lower end of the tube is sealed, the top remaining open for the purposes of refilling with water and the escape of the gases. On the top of the liquid is poured a thin film of oil to prevent atmospheric evaporation.



Closed. Open.

FIG. 2.

The liquid is composed chiefly of water, rendered non-freezing to within 24° Fahr. below the ordinary freezing point of water by the addition of sulphuric acid, upon which, however, the electric current has no appreciable effect.

The entire electrolytic apparatus, which is self-contained, is

mounted in a cast-iron case, as shown in Figs. 1 and 2. The terminals of the electrolytic apparatus are permanently connected to two other main terminals, which are mounted on a porcelain or ebonite block at the base of the meter. Mr. Gibbings.

The cast-iron case is fitted with a large hinged door and a terminal door, which are provided with the necessary sealing arrangements. In the large door a slot is arranged into which glass is fitted, and through which the scale may be read.

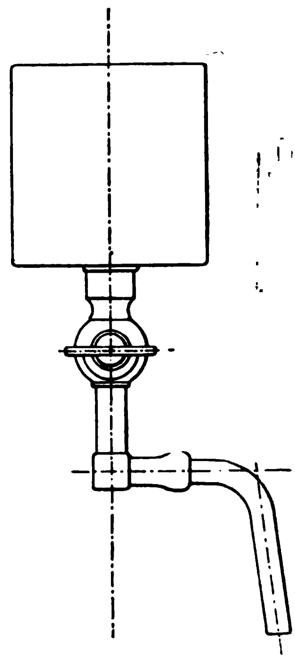


FIG. 3.

The scale, which is placed on each side of the tube, is made of enamelled sheet metal or other suitable metal. It can be adjusted up or down, to the extent of three-eighths of an inch, by means of an adjusting nut and thread at the lower end, for the purpose of obtaining an accurate zero every time the tube is refilled with the electrolyte; thus avoiding the otherwise necessity of extreme care in the act of refilling, and the use of a syringe if filled slightly too high.

The refilling apparatus, as shown in Fig. 3, consists of a long

Mr. Gibbings. funnel which is provided with a cock in the stem, so that the supply of the electrolyte may be instantly stopped when it rises to the zero on the scale.

As, however, in the action of the meter only water is decomposed, thereby leaving the specific gravity of the electrolyte much greater at the bottom of the scale than when the tube is full, it is clear that in periodically filling the meter water only need be used.

The entire meter is fixed on the wall or on a meter bracket in the usual way.

Probable Cost.—It will easily be seen from the foregoing details that the meter is inexpensive in construction, the chief item being the platinum and the glass tube. I may state at once that the meter can be supplied at 50s. up to 10 amperes capacity. This fact should be reassuring to Mr. Evershed, whose remarks on this point at the Municipal Electrical Association meeting I quoted in the previous part of this paper, and who doubtless at the time accurately represented the opinions of most electrical engineers.

III.—Tests.

Calibration.—In commencing the test of a meter with new electrolyte through which no current has been previously passed, a peculiar action occurs. The liquid first of all becomes slightly milky, and for the first three or four minutes the voltage across the terminals rises about one-fifth of a volt, and then gradually falls again to the normal in from 30 to 60 minutes. This does not occur subsequently with the same electrolyte. The zero of the scale should be obtained after the electrotye has been placed in the meter for a few hours in an average temperature. Although the entire scale is made adjustable to zero without affecting the calibration, readings must be taken only after the current has ceased for fully an hour, for the following reason:—On starting the meter throughout its range of capacity, the electrolyte expands, and consequently rises in the tube to an extent corresponding to the size of meter, being about 2 mm. for a 5-ampere capacity. Hence, if a reading is taken

with current passing, an error is introduced, but the electrolyte returns to its correct level in about an hour after current has been switched off. On a range of 250 units at 200 volts calibration the error would be 0·5 per cent. on the calibration. Mr. Gibbings.

The only calibration of this meter which is necessary is at the full load of its capacity, and this may be effected in the usual way by connecting up a number in series. The calibration should extend to half the range of the meter, and the scales are then marked off mathematically accurate for each meter respectively. The scales are thus not interchangeable.

Accuracy.—Tests have been taken throughout the ranges of several meters, at varying loads, at the respective temperatures of 32° Fahr. and 100° Fahr. The greatest variation from perfect accuracy which occurred was about 2 per cent. between these two temperatures, after allowing the liquid to settle to its normal temperature before and after readings were taken.

Temperature.—The temperature of the electrolyte rises with the increase of current from minimum load to its range limit. This is shown as a curve in Fig. 4. As the conductivity of liquids increases with the rise of temperature, this point is an advantage, in contradistinction to its usual effect in other types of meters.

Overloading and Excess Current.—This is one of the most prolific causes of trouble with meters of the ampere-hour type. With regard to excess currents, with which we may include the local short-circuit which blows a main or subsidiary fuse, their effect upon ampere-hour meters which employ permanent magnets is of such a nature as to render them quite unreliable, and their recalibration a matter of necessity. In one type the effect of overloading leaves the meter permanently high in calibration; while in another type the effect is the very reverse of this. These inaccuracies are quite unable to be discovered readily, and are frequently not discovered at all until the consumer complains of an abnormal increase upon his usual consumption. The error will, of course, vary in extent, but I have found several to be from 17 per cent. to 25 per cent. During the year 1897 the Bradford Corporation have removed 28 meters which were found to be out of order from the above causes.

Mr. Gibbings.

Watt Loss.—Referring again to meters of the motor or pendulum types, we find that the watt loss ranges from 2 to 12 watts. In this respect they show a slight advantage over the electrolytic meter, in which, at all loads, over 2 volts are lost through

Rise in Temperature in relation to Current.

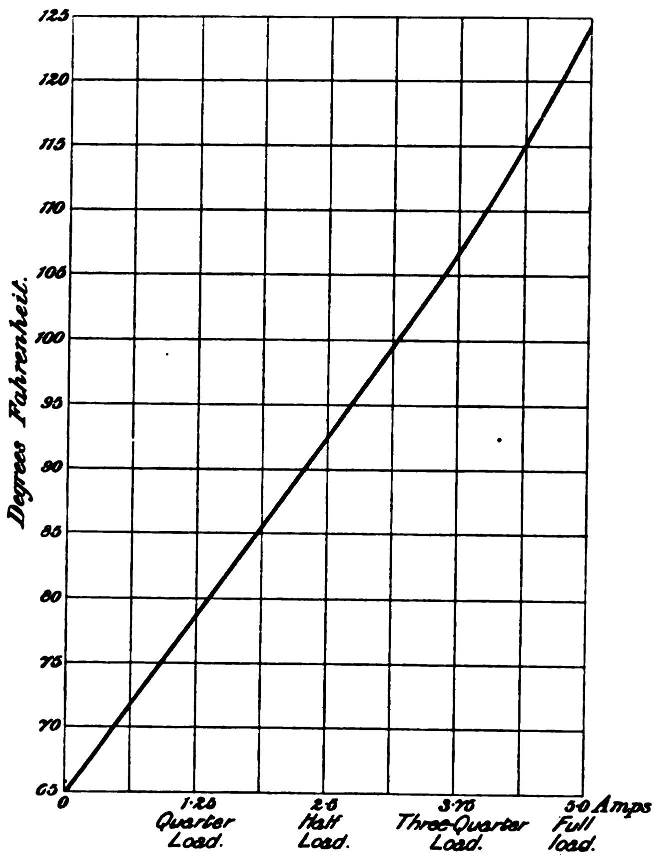


Fig. 4.

the action of the meter. Against this one defect, however, must be set several compensating advantages, such as greater accuracy at light loads, and no shunt coil losses, which, in some cases, is a never-ending loss. The following curves of fall of potential across

meter terminals have been taken when the electrolyte has been at zero on the scale and at end of range, showing that the meter has a maximum loss at the zero end of the scale:— Mr. Gibbings.

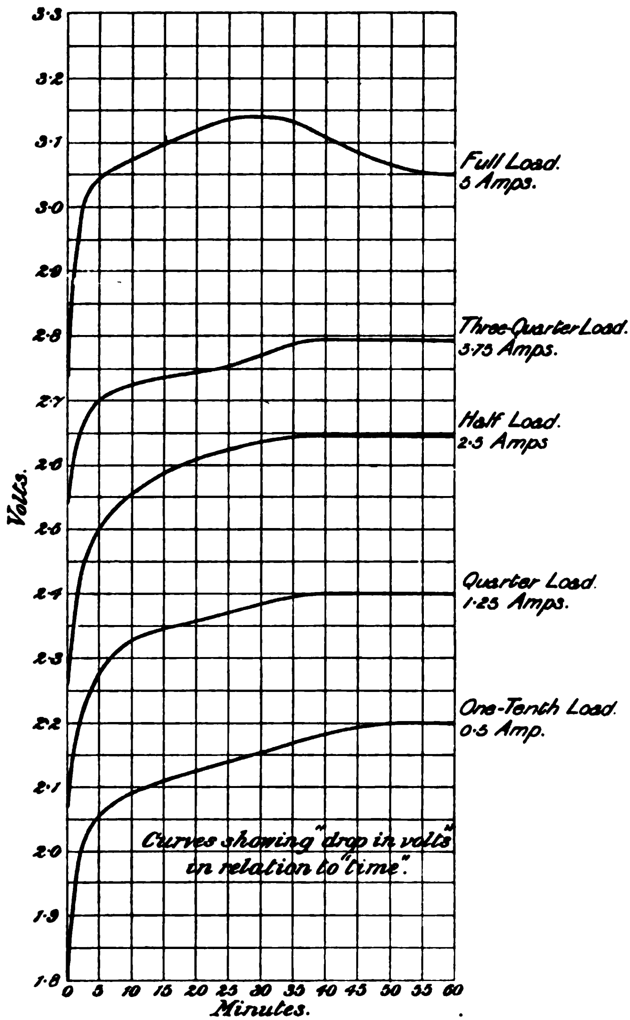


FIG. 5.—At zero of scale.

Mr. Gibbings.

Drop in Volts in relation to Time, when Meter has registered 200 *B.T.U.*

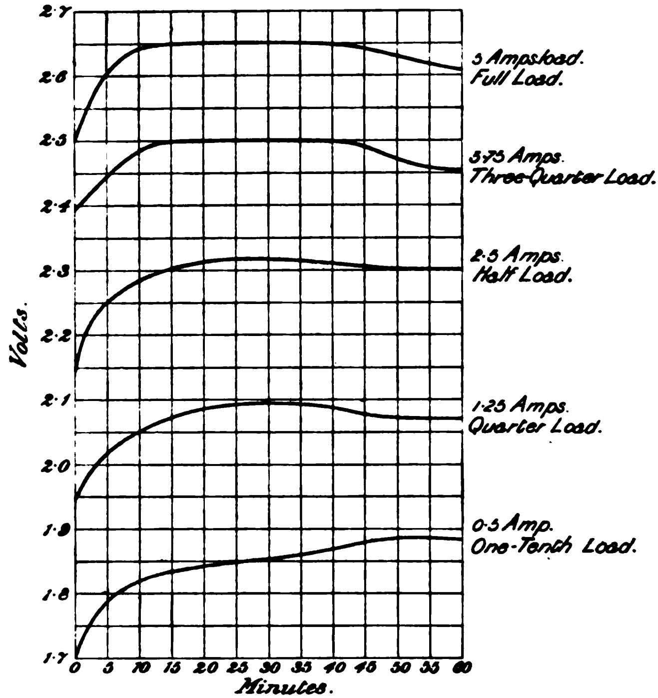


FIG. 6.—At end of range.

SUMMARY.

I have just described in the foregoing tests the range of accuracy of the meter as far as the registration, efficiency, and general reliability are concerned. You will have seen, however, that the results which have been obtained are not only satisfactory in themselves, but that they also have a most important bearing upon the whole question of meter troubles, and that in this respect these results cannot be too highly appreciated. I shall proceed to consider them in detail in the light of the influence which they have on the costs of the meter department, and will briefly deal with these under three heads, viz.:—

1. Calibration.
2. Periodical Inspection.
3. Repairs and Maintenance.

Mr. Gibbings

Calibration.—By calibration I mean the actual first test to which a meter is subjected as it is received from the maker's hands, and by means of which its accuracy of registration is ascertained. The mean percentage error which I believe is usually allowed is 3 per cent., and those which do not come within that limit are rejected. Also, with most of the existing types, calibrations have to be made at one-tenth, one-quarter, one-half, and full load. The question, then, becomes, How much does a meter actually cost before it is ready for the consumer? Meters which are returned to the makers after a first test involve a second, and frequently a third, test, so enormously adding to the cost. I give herewith a list of meters ordered by the Bradford Corporation in 1897, with the percentage rejected:—

1897. Months.	Number of Meters Tested.		Number of Meters sent back to Makers		Number of Meters Accepted.		Percentage Rejected.	
	Watt.	Amp.-Hour.	Watt.	Amp.-Hour.	Watt.	Amp.-Hour.	Watt.	Amp.-Hour.
January ...	...	31	...	10	...	21	...	32·3
February	12	6	4	2	8	4	33·3	33·3
March ..	...	38	...	13	...	25	...	34·2
April ...	...	34	...	14	...	20	...	41·2
May ..	18	20	...	8	18	12	...	40·0
June ...	...	24	...	13	...	11	...	54·2
July ...	..	44	...	25	...	19	...	56·8
August ...	..	42	...	25	...	17	...	59·5
September	5	68	...	44	5	24	...	64·7
October ...	...	60	...	20	...	40	...	33·3
November...	...	58	...	27	...	31	...	46·6
December...	...	36	...	18	...	18	...	50·0
Total ...	35	461	4	219	31	242	11·4	47·5

	£	s.	d.
Invoice cost of above accepted meters ...	1,412	11	0
Cost of testing all meters	60	0	0
Total cost of meters	£1,472	11	0

Mr. Gibbings.

Hence, average cost of accepted meters, £5 3s. 5d. each.

If we compare this figure with the cost of making one calibration of scale at full load only, and add to that the cost of the electrolytic meter—viz., £2 10s.—a more accurate and fairer comparison will be made with present methods and meters.

Periodical Inspection.—The argument which I have put forward above is equally applicable to the cost of periodical inspection, though in a slightly different sense. In provincial towns the meters are usually inspected once a month, and where any pretence whatever is made to really try the meters for starting current and to keep up their efficiency, the work absorbs the constant services of a certain number of meter inspectors. But it is evident that inspection once in three months, instead of once a month, would entail only one-third of the cost. And it must further be borne in mind that no tests for starting current are necessary, and that the operation of refilling with water is a matter which takes but a few moments.

Repairs and Maintenance.—In the course of periodical inspection throughout the year, many meters are found to be defective, and these have to be removed and others substituted. Here again the cost of re-testing and re-calibration is incurred, as well as the cost of carriage to and from the works of the maker. The actual cost depends, of course, upon the number of the meters in circuit, but the *average* cost forms no inconsiderable item in the costs of the department. In this respect also the electrolytic meter compares most favourably. Even when repairs are necessary—such, for instance, as the replacement of a broken tube—it can readily be accomplished *in situ*, and without even removing the meter case from the wall.

Evershed.

Mr. EVERSHED: Mr. Gibbings refers to some remarks which I made at Manchester last year, in the course of which I expressed a doubt as to the possibility of making a meter which could be sold at from 40s. to 50s. I think it can hardly be doubted that the meter Mr. Gibbings has described to-night could be made for 50s., and the only doubt in my mind is how long it is going to last. There have been many electrolytic meters introduced, and they have

Mr. Evershed

one and all failed, from causes which are perhaps difficult to trace; but, as a rule, an electrolytic meter usually ends, after one or two years' use, in general disintegration of the electrodes. I do not know how long Mr. Gibbings has used his meter, but that is the kind of trouble which one would expect. Another criticism which I would venture to make is with regard to the evaporation of the water. This is a question entirely of experience, and no doubt Mr. Gibbings can tell us exactly what happens. Some years ago I put oil on the surface of the acid in some accumulators, with the idea that I should get rid of both the evaporation and the spray. It certainly did not get rid of the spray. I do not remember whether the experiment was done carefully enough to know whether it entirely got rid of evaporation. Of course, if oil does so, that removes one difficulty. Mr. Gibbings has already removed the difficulty with regard to the spray—perhaps a more serious difficulty—and has done so ingeniously by means of the little curved tube. On reading the paper for the first time, it occurred to me—and I daresay to nearly everyone who read it—that the most serious drawback to the meter was the large drop in volts at full load, amounting to about 3 volts. But it should be noticed that the consumer has nothing to do with the 2 volts drop due to the back E.M.F. of the plates, which is practically constant; all it means is that, instead of using a 200-volt lamp, he must use 198-volt lamps, and practically he will have no trouble on that score. We have, then, only to consider the additional drop between no load and full load, and this amounts to a little over 1 volt. On a 200-volt circuit it is ½ per cent., and I do not think we need quarrel with that. I think we must all agree that Mr. Gibbings has given us a very simple means of registering small currents. The absence of any mechanism for recording is a great thing in its favour. Its efficiency as a meter is also high; and here I should like just to make one or two remarks as to the efficiency of electric meters generally, because it has recently occurred to me that one of the difficulties in making electric meters cheaply is that it is almost impossible to make them really efficient—that is to say, to make the ratio of useful

Mr. Evershed.

work done to energy wasted as high as possible. When you consider a motor meter (and nearly all good electric meters are of the motor type), you find that nearly all the energy spent in the meter is wasted in heating the conductors, and only a very small proportion goes in turning round the motor and driving the wheels of the counting mechanism. I had the curiosity to measure the other day the amount of electric energy wasted in my own meter, and the proportion that was converted into mechanical energy. I found that only 1-5,000th of the total power absorbed in the meter was spent in turning round the meter itself—it is a motor meter—the rest was all wasted in heating the conductors. That is by no means an exceptional amount. In the Hookham and Thomson meters the proportion is very much the same. So that you have in a motor meter an absurdly inefficient mechanism for doing what you want, namely, turning round the wheels of the counting mechanism. Unless some means is found of getting over that difficulty, I really do not see how meters are going to be made any cheaper. You can only make them more efficient by putting more material into the conductors, into the coils, and you can only put more material into the coils at greater cost. Mr. Gibbings, of course, entirely gets over that difficulty by abandoning the motor principle. The great disparity between the cost of electric meters and gas meters has frequently been given as a reason for anticipating a great fall in the cost of electric meters. An analogous case was noticed by Dr. Thompson, I remember, years ago, in commenting upon the cost of ammeters and voltmeters. He said he could obtain a very reasonably accurate thermometer for 1s. 6d., and how was it he could not get an ammeter for something approaching that?

Dr. Silvanus Thompson: Half a crown.

Mr. Evershed: I pointed out at the time that the materials of the ammeter cost more than 1s. 6d., for the simple reason that, in order to reduce the C^2 R losses in the instrument to a reasonable amount, you must have a certain weight of copper in the coils. Much the same cause affects the cost of electric meters. On the other hand, you can very easily make a gas meter a very

reasonably efficient motor. When I say "reasonably efficient," I do not mean that you can get 90 per cent. efficiency out of it; but you can get 30 or 40 per cent. efficiency out of it, for the simple reason that what you can call the $C^2 R$ losses in a gas meter—that is to say, the loss from friction of the gas—can be reduced almost without limit by making all the conducting pipes or channels in the meter very large. By making those channels large you do not necessarily spend very much more money than if you made them small. I have measured the total power taken in driving an ordinary 30-light dry meter, and it amounts to 0·133 of a watt at full load. Mr. Evershed.

I do not think there is a single electric motor meter in the market that spends less than 4 or 5 watts at full load. Mr. Gibbings's spends 15 watts, or something of that order; of that, about 10 watts is spent in decomposing the water, and 5 watts is wasted in heating the meter. In the Hookham meter about 5 watts would be spent; in the Thomson something like 12 watts, I believe. So that we have this rather startling fact—that for the last 20 or 25 years gas-meter manufacturers have been making far more efficient meters than we electrical people have ever been able to turn out. My own impression is that we shall always have that difficulty: we shall always have to spend a fair amount of power in the meter, and therefore we shall always have to spend far more money on an electric meter than is spent on a gas meter.

I should like, before sitting down, to refer to one other point. When you look at the average electric meter, you find it is practically impossible to shut it up inside its case and send it out of the manufacturer's works and put it to work in a house without any examination or adjustment of its mechanism. When electric meters reach the supply company they have to be overhauled; and, according to Mr. Gibbings—and he speaks with great authority on this point—about half the meters delivered have to be sent back to the manufacturers to be overhauled. I need hardly say that gas meters could not be manufactured at the prices they are if anything like 50 per cent. of them were returned to the makers for further adjustment. I do not think we can be considered to be really making

Mr. Evershed. any progress in electric meters until we can turn out instruments which can be sent away sealed so that you cannot get at the inside, but so that you can see what is necessary in order to make the necessary tests for accuracy. The meter then will be in a practically air-tight case, and you will not be troubled with dirt. It must be capable of being turned upside down. (I do not know how Mr. Gibbings would like his meter to be tipped upside down.) It must stand a railway journey. There, again, is a little point to which I call Mr. Gibbings's attention. I do not know whether his meter will stand a railway journey. He has a rubber cushion for his glass to sit on, so that I presume he has had something of that sort in view. These really are essential points for all electric meters. They must ultimately be made like gas meters; they must be capable of working for years without adjustment, or even examination of the mechanism. It is not a question of months; they must go on for years. I took a gas meter to pieces the other day which was made in 1872, and soldered up at that date. Its works were just about as good as they were in 1872. [Professor PERRY: As bad!] Yes, I know it is not saying very much, because the mechanism inside is simply shocking. The workmanship is atrocious, but it is sufficient for the purpose, and that is the sort of thing we want for electric meters.

Professor PERRY: I should like to ask Mr. Evershed a question, namely, whether he has examined the domestic meters, and whether he can give us any information as to what the average percentage of error in an ordinary domestic gas meter is; is it 15 per cent., or less?

Mr. EVERSHED: I am afraid I cannot pose as an authority on gas meters. Of course I share the average prejudice against gas meters, but my impression is that that prejudice is somewhat old-fashioned and groundless. I do really believe that the average gas meter reads right within 3 or 4 per cent., and it goes on reading right for years. The fact is that the mechanical forces at work in it are so large that friction and things of that sort do not very much matter. The only thing that can happen to a gas meter is that it may get stuck, and then you do not get all the gas you want. You may also obtain

gas by leakage through a defective gas meter without any record on the dials; but I do not think you can get a dry gas meter to record without having the equivalent volume of gas passed through it. Mr. Evershed.

Mr. J. SWINBURNE: When I came into this room I met Mr. Evershed, and he said, "I was going to speak, but now I see you "are here I suppose you will tell us all about meters." I said, "Why should I?" Mr. Evershed said, "Because you have tried to "make every kind of meter, and none of them would work!" There is a little bit of truth in that, because I have tried to make meters; but it is not quite true, because I did make one meter which worked magnificently. I was going to bring it out, but, unfortunately, I found a man had patented it six years before. I only introduce that as a preface to the remark I was going to make, and that is about such inventions as this of Mr. Bastian's. One's first inclination on seeing a thing of that sort is to say, "Oh! "that was done years ago; somebody else has done it." I do not believe that it has been done before. Although Mr. Gibbings has produced a list of patents, I do not think that they are very relevant. There is a general feeling—I think especially in the electrical business—that a crude idea is an invention; but I think we ought to give far more credit to people, like Mr. Bastian, who complete an invention and work it out and bring it into its commercial form. To put the matter shortly, I will summarise it by saying that any fool can make an invention, it takes a clever man to work out the details, and it takes a genius to sell it. Mr. Swinburne.

Mr. A. WRIGHT: I must thank Mr. Gibbings for bringing out something that will help most of us central station men in getting a greater field for the sale of our products. I think the instrument he has developed will help us very materially. I cannot agree with Mr. Evershed about the efficiency of meters being very relevant to our business. I do not think we need consider such things. All we want to know is whether it will give us much trouble,—whether it will bring us any revenue, or cost us very much to buy. Whether a meter consumes 1-5,000th part of its consumption in moving it round, and the rest in friction, does not seem to me to have any bearing on the subject. Mr. Wright.

Mr. Wright. The great thing is, will it register lamps being put on in the daytime, or very small candle-power lamps? and does it cost very much to keep in order? I notice Mr. Evershed said that the question of the first 2 volts necessarily lost in electrolysis is immaterial. To me that is the only objection I can raise to the meter. It means practically that 1 per cent. of the total station output will be wasted in making gas on 230 supply circuits. I cannot help thinking that is a possible objection, as it amounts to a rather large sum per annum. I think the other loss in voltage, which varies with the current, is as nothing compared with those 2 volts which Mr. Evershed puts on one side as insignificant. Notwithstanding this, I think the meter will help us materially in getting in the smaller classes of consumers, where the cost of a motor meter might be a very serious obstacle. I hope Mr. Gibbings will develop the prepayment attachment to it, as this ought to be very easy in such a mechanicless meter.

Mr. Hirst. Mr. H. Hirst: With regard to Mr. Swinburne's definition, I class myself essentially amongst that third class of people who have to do with meters, namely, the geniuses. I have had a great deal of experience with central station engineers; and, while I do not presume to be able to criticise an engineer of Mr. Gibbings's experience, I would like to ask him whether he has considered those questions which we who have to sell the meters to the central stations are constantly being asked. What is the cost of maintenance? Does it not appear that the refilling of the meter at irregular times adds to the cost of the maintenance? Is it not a fact that there being oil at the top of the water the meter may want cleaning pretty frequently? Then is it not a fact that as the water is being consumed and has to be refilled an entire loss of the record of the previous readings is caused? and is not that a serious drawback? Probably Mr. Gibbings has considered all these points; but I should like very much to know what he has to say, because there is no doubt that every meter maker feels the want of a very cheap small meter for small currents, alongside those used for the larger ones. As Mr. Evershed mentioned the question

of the comparison of gas meters and electric light meters, I would like to state that we are also confronted with the fact that the accuracy which the Board of Trade requires for electric meters is very much greater than that for gas meters. If that accuracy were not insisted upon, I think cheaper meters could be got out. With regard to the fact that he does not know a meter which takes less than 10 or 12 or 15 watts, I should think Mr. Evershed has in his mind only motor meters. If people would look in the direction of those which register by the influence, and not by the direct working, of the current, I should say a meter can be produced, and I know there are some in the market—I do not wish to mention names—which work with at least 1 watt. Mr. Hirst.

The PRESIDENT: Mr. Gibbings has brought before us an exceedingly simple meter—a meter which seems to meet an urgent want, namely, that of taking account of the small quantities of electricity which are sometimes used in electric lighting, and which most mechanical meters fail to record. Very few mechanical meters of any considerable range would start with a current of 1-10th of an ampere, and it is desirable that currents even as small as 1-10th of an ampere should be duly recorded. Considering the principle involved in this meter, there can be no question that it would take due account of even the very smallest current, without failure. The President.

It has, too, the merit claimed for it of extreme simplicity, and corresponding cheapness; also, I should think it has the merit of being approximately exact in its record. I see no reason to doubt that, with attention to the filling up of the tube with water at proper times—that is to say, frequently—it should not act very well for such small currents as Mr. Gibbings has exclusively in view in connection with the use of this meter. I think, however, that, where the entire current passes through the meter, a 20-ampere meter on this principle is probably outside the practical limit, and that a maximum of 5 amperes is more likely to be the practical limit of its range. In the case supposed by Mr. Gibbings where the installation is for ten 60-watt lamps on a 200-volt circuit, and the maximum current is therefore about 3 amperes, nearly 10 ounces of water would be

The President. decomposed in 300 hours, provided the full current was used all the time. But it might happen in some exceptional case that the ampere-hours in 100 days might be double this amount: then the quantity of water dissipated would be 20 ounces. Therefore the regular and frequent replacement of the water lost by electrolysis is a necessary condition of its successful use.

The lowering of the voltage occasioned by a meter of this kind is, I fear, rather a serious drawback, not so much on account of the actual loss of energy it involves, as the inconvenience of having other meters for measuring larger currents on the same circuit, which do not cause the same drop of potential, from 2 to 3 volts, through counter E.M.F. It seems to necessitate the use of two different classes of lamps on the same circuit, the class used in connection with the electrolytic meter requiring a potential difference of 2 or 3 volts lower than lamps used with mechanical meters. I should like to ask Mr. Gibbings what experience he has had in the use of this meter. Over how long a time has it been in actual use, and on what size and kind of installation? Also, I should like to know the result where it has been used. I think this meter well deserves a trial, and I hope it may come out of the practical ordeal as well as it has come out of the theoretical. I congratulate Mr. Gibbings on having brought before us a means of smoothing away one of the minor difficulties in connection with central station electric lighting. I would also ask whether, if the meter has been in any case used for a considerable time, any disintegration of the cathode plate has been noticed. From previous experience of the long use of thin plates of platinum for the evolution of hydrogen in electrolysis, one would anticipate a tendency to the disintegration of the cathode.

Mr. Boot. Mr. H. L. P. Boot [*communicated*]: I am glad to see that the question of electricity supply meters, in some form or other, is receiving the attention of this Institution. Only those who are intimately associated with electricity supply know of the difficulties experienced in obtaining reliable and accurate meters. It is one of the most unpleasant duties the central station engineer has to perform—*i.e.*, listening to the complaints of

consumers with respect to the gymnastics performed by their meters—and, unfortunately, the meter's reliability cannot be sworn to in the manner one would like; and the author is to be congratulated for bringing up this subject for debate; but I regret that he does not open a pathway for alternate-current engineers, many of whom have changed over to the higher pressure on the distributors, and for that reason there is just as much need of an accurate meter by them for very small currents. I do not agree with the author with respect to the selling price of the meters, as I do not consider the prices at present at all prohibitive, provided *that* were all they cost. I think it would be cheaper in the end to obtain an accurate meter, rather than a low-cost one. Most of the meters in use at present inherit the fault that they may be accurate for six months, but after that necessitate considerable expense in removing and re-calibrating; and it is the additional expense after having purchased the meter that I have to complain of. We want a meter that, when once installed, will not want changing or re-calibrating for at least three years; in fact, according to the system of many Corporations and companies, the meter is supposed to last 25 years at least, if one is to judge by the amount of their *sinking* fund payments. The principal objection to the meter Mr. Gibbings proposes to use in his paper, appears to be the drop in pressure. Mr. Boot.

It is interesting to see the results of the Bradford Corporation with respect to the percentage of meters rejected, and it is surprising that this percentage is so heavy. It certainly shows plenty of room for improvement by the manufacturers before they are sent away.

Mr. W. H. EVERETT [*communicated*]: It is refreshing to hear of a meter which dispenses with the conventional method of registration by hands on dials, and reads directly on a straight scale without the use of any mechanism. Some experience with electrolytic meters prejudiced me against them, but Mr. Gibbings's instrument seems much more promising; though long use is necessary before it, or any other meter, can be fully approved. The principle of working is such that the meter ought to remain practically constant in its indications for a long time, which is Mr. Everett.

Mr. Everett. the chief desideratum in a meter. To exemplify the behaviour in this respect of one of the best modern types of ampere-hour meter, I may quote the results of tests of over 200 instruments, taken after they had been in use for about a year. Before going out they were all correct within 3 per cent., the average error being perhaps $\pm$ 1½ per cent. After the year's use this had increased to $\pm$ 4·6 per cent.; however, the + and − errors about counterbalanced, so that the whole average was only + 0·04 per cent. The proportion over 5 per cent. high was 13·6 per cent.; and 13·1 per cent. were over 5 per cent. low. Very few of these much exceeded 5 per cent. in error. Each meter was tested with approximately the current for which it had been used. As regards meters of the same type direct from the makers, in a batch of 80 about 38 per cent. were decidedly unsatisfactory—for one reason or another—in their test, and an additional 21 per cent. were only tolerable.

Another advantage Mr. Gibbings's meter possesses is that it is more easily read, and therefore less liable to error in reading, than instruments in which successive dials are used, especially as dial hands are often slightly misplaced. A slight objection to the meter is that it cannot be tested in a few minutes, as others can, since it has no quickly moving part. However, such tests, in any case, can only be regarded as provisional, since errors may arise in the mechanism transmitting the motion to the hands.

By not using a shunt, such as is adopted in the Edison meter and some of other types, Mr. Gibbings loses 2½ or 3 volts, as well as the security against interruption given by a shunt, but gains a magnified reading and greater reliability. The larger part of the voltage loss is independent of the resistance, and therefore cannot be obviated.

The error produced in some types of meter by excessive currents—as referred to in the paper—I have often noticed. Of course certain meters, such as that of Elihu Thomson, are free from it.

The cost quoted for modern meters is certainly representative, yet there is one good and well-known type which can, I believe, be obtained for a much lower figure in the 5-ampere size. Even

this, however, is considerably higher than that mentioned in connection with the meter under consideration. Mr. Everett.

Mr. H. F. HUNT [*communicated*]: I hope this exceedingly interesting paper which has been brought before us to-night will, for one thing, infuse into some of our members a little interest in the meter question—a subject which has hitherto received but the scantiest attention at the hands of this Institution. Mr. Hunt.

Mr. Gibbings gives us a concise summary, in chronological order, of all the British patents granted since 1883 for electrolytic meters. It is a pity he did not complete the list by including earlier workers, such as Sprague and Edison, the last of whom had brought his electrolytic meter into commercial form prior to 1883.

With regard to the novelty of Mr. Gibbings's invention, I would like to refer to two previous patents which bear some similarity to the one before us. In Lane Fox's patent for electric lighting, &c. (No. 3988, of 1878), there is a meter described which consists of an electrolytic cell having a rather long neck at the top; and it is claimed in this specification that "the quantity of "water decomposed will be approximately proportional to the "quantity of electricity which has passed through the apparatus." Lane Fox's invention, differs, however, from the one before us in two respects—first, in being designed to carry only a definite fraction of the total current; and, secondly, in possessing a coil with "make" and "break" contact connected in parallel with the bottle, the object of such coil being to increase the decomposition by reason of the "extra," or induced, currents. In 1882, however, a disclaimer was granted for that part of the specification relating to the above-described measuring apparatus, on account of its validity being considered doubtful. The second anticipation—if such it can be called—is Mr. Green's United States patent (No. 337679) of 1886. Here we have an electrolytic meter consisting of a vertical tube with one electrode fused through the bottom; the other electrode is suspended through the upper and open end, and is made adjustable up and down. A scale is etched on the glass to show how much water has been used up.

On page 559 of his paper, Mr. Gibbings says, "The calibration

Mr. Hunt. "should extend to half the range of the meter." In the meter on the table, this means that current equivalent to 125 units at 230 volts is required; and, as the maximum current of this particular instrument is 5 amperes, and its maximum registration thus 1·15 units per hour, we see that the calibration occupies about 109 hours—a rather inconveniently long time.

Some of the speakers have referred to the excessive drop in volts; but we must remember that Mr. Gibbings has designed his instrument specifically for voltages over 200, so that his "drop" amounts to but little over 1 per cent. Taking the Thomson meter on a 100-volt circuit as a comparison, we find the full-load drop forms very nearly as high a percentage, and yet no one complains about it.

The other points to which I intended to refer have been so clearly put by Mr. Evershed that any further comment on my part would be superfluous.

Mr. O'Gorman. Mr. MERVYN O'GORMAN [*communicated*]: Some of the reasons which may have deterred others from adopting Mr. Gibbings's meter principle are of interest. The difficulties have probably been overcome, but the means adopted and the tests spoken of do not clearly show this.

After an hour of full load (10 amperes) rather more than 3·3 cubic centimetres of water will be decomposed, giving rise to 6,600 cubic centimetres of explosive gas. This volume largely exceeds the contents of the cast-iron box, which, by diffusion, will become completely filled with it. This would appear to be a source of danger if a flame be employed when examining the instrument. If a consumer be present at the reading and opening of the meter case, he will notice many globules of water on the sides of the larger containing jar. The globules will be large, because of the manner in which they will have been formed. The oily inside surface of the jar will receive the spray of the bursting bubbles, and detached drops, which would have run down the sides of a clean vessel, will be built up to the maximum size allowed by the surface tension of the liquid. The consumer may then insist upon the meter being "well shaken "before taken"—an undesirable proceeding, since the accuracy

of a reading depends on the meter not being moved since the zero was taken, and on its being plumb. Inaccuracy also arises from the scale of the instrument being screwed to the iron box, while the indicator tube is seated on a soft rubber cushion: as the tube empties itself of 400 grammes (say a pound) of water, the resiliency of the rubber will assert itself and diminish the reading. The corrosion of the rubber cushion was foreseen, but scarcely guaranteed against, by the special provision of a funnel and tap for refilling the jar with acid without spillage or causing overflow of the oil. Mr. O'Gorman.

There is a risk that, on filling, the incoming fluid will tend to force down the surface oil into contact with the platinum plates, thus insulating them, and increasing the resistance of the instrument. The resistance may also be increased by the impact of the liquid displacing the platinum plates. Doubtless this error is not serious.

There is a temperature error of a peculiar kind. Two hundred and fifty kilowatt-hours on 200-volt mains represent in the meter some 400 grammes of water, the gases from which will occupy about 2,000 times the liquid volume, or 800,000 cubic centimetres. This gas will become saturated in bubbling through the liquid, and will carry off an amount of water vapour depending on the climatic conditions at the time of the energy consumption. In summer, at an average temperature of 25° C., and 760 millimetres, 800,000 cubic centimetres will carry off 18 grammes of water vapour—that is, 18 cubic centimetres. This will be increased when the water gets hot, at times of heavy load and in the winter.

Irregularities will arise unless precautions are taken that the oil shall not freeze, any more than the dielectric does, at 24° Fahr. below the temperature of melting ice; likewise, it must not oxidise and solidify into a grease under the action of pure oxygen and hydrogen peroxide.

From a different standpoint, two criticisms suggest themselves—

(1) When once the water chamber has been filled no record of all previous readings remains to the user, as in the counting mechanisms of the ordinary type.

Mr. O'Gorman.

(2) Since electrical energy during heavy-load time is worth 7d. a unit, as against 1d. during light load, it is seven times more important to read accurately the full-load current. Until, therefore, such accuracy is abundantly guaranteed, it would be questionable economy to adopt the electrolytic system, however admirable the method may be for cumulatively measuring the smallest demand.

Mr Gibbings.

Mr. A. H. GIBBINGS, in reply, said: The discussion has turned upon just those points which I desired. As a matter of fact, I have had an experience of this meter for only six months; and I must also disown the qualifications which Mr. Swinburne has set out in regard to this meter, by saying that I disclaim being either the inventor or the seller, or the one who worked out the details. I have brought this paper before you to-night with the hope that other experience could be given by the members upon possible defects which the meter might have. In reply to Mr. Evershed, and with regard to putting oil on accumulators to prevent evaporation, there is in that case a very much greater surface than in a smaller meter like this. The efficiency of the meter is also an important point. In putting forward the advantages of the instrument, I have not wished to cover up its disadvantages in any way. That is one of the points upon which I wish to gain the opinion of the members of this Institution, while at the same time putting before them something which I thought might be of considerable interest to those engaged in central station work. The meter is undoubtedly a delicate one, and will probably not stand railway journeys so well as some other types of meters, but it certainly can be packed so as to prevent any serious damage to it. With reference, again, to the question of efficiency, which has been raised by two other speakers, I may say that I think that the fact of the small candle-power lamps being registered, and there being practically no loss with them at all at the time they are on, even if there is only one lamp in use, makes up very largely for the drop of volts across the terminals, which is possibly a greater loss than in some of the motor meters. In reply to a question of the President, I have

not had this meter in use for a sufficient length of time to notice any deleterious effect upon the electrodes, and I have not seen the disintegration of which he spoke. We have about a dozen of these meters now on consumers' circuits at Bradford, and they are still going on very well. They require refilling once in four or five months, according to the proportionate average demand to the capacity of the meter. I think most of the ground has been covered in the discussion, so without further remark I invite you to make an inspection of the meter, which I have here in operation. I beg to thank you for the kind attention you have given to the paper. Mr. Gibbings.

The PRESIDENT: I will now ask you to pass a vote of thanks to Mr. Gibbings for his paper.

The resolution was carried by acclamation.

The PRESIDENT: I hope Mr. Gibbings will communicate in some way the result of the working of the meter over a longer time.

Mr. GIBBINGS: With much pleasure; and possibly others who will have then had more experience will also do so.

The PRESIDENT: I have to announce that the scrutineers report the following candidates to have been duly elected:—

Associates:

Michael J. Buckley.	J. F. Lamb.
Ralph Henry Covernton.	Alfred Miskin.
Howard Kyan de Lacy.	Léon Mirabel.
Harold Steuart Gladstone.	Moritz G. S. Swallow.

P. J. S. Tiddeman.

Students:

G. F. R. Jacomb-Hood.	Theophilus Kerr-Jones.

The Three Hundred and Eighteenth Ordinary General Meeting of the Institution was held at the Society of Arts, John Street, Adelphi, on Thursday evening, May 26th, 1898—Mr. J. W. SWAN, F.R.S., President, in the Chair.

The minutes of the Ordinary General Meeting held on May 12th, 1898, were read and approved.

The names of new candidates for election into the Institution were announced, and, this being the last meeting before the recess, it was, on the motion of the PRESIDENT, agreed unanimously that the candidates should be balloted for that evening.

The following transfers were announced as having been approved by the Council:—

From the class of Associates to that of Members—

Alfred Whalley.

From the class of Students to that of Associates—

Reginald Charles Clinker.
Edward Dixon.
Selwyn Seafield Grant.
Arnold Grant Livesay.
Arthur Holroyd Sears
Herbert Turnbull.

Mr. J. Brooking and Mr. W. Bunn were appointed scrutineers of the ballot for new members.

A donation to the Library was announced as having been received since the last meeting from Mr. W. Perren Maycock, to whom the thanks of the meeting were duly accorded.

A letter from Mr. H. Wilde was read, thanking the members for his election as an Honorary Member.

The following paper was then read :—

THE DESIGN OF ELECTRIC RAILWAY MOTORS FOR RAPID ACCELERATION.

By Professor CHARLES A. CARUS-WILSON, Member.

The torque on the shaft of a motor may be expressed by the equation, Prof. Carus-Wilson.

$$\text{torque} = 1{\cdot}41\, p\, A\, C\, N\, 10^{-8} \text{ inch-pounds} \quad \ldots \quad (1)$$

where N is the number of C.G.S. lines per pole, A is the number of surface conductors, C is the total current passing into the motor, in amperes, and p is a numerical constant depending upon the way in which the armature is wound. This equation may be written,

$$\text{torque} = 1{\cdot}41\, C\, M \quad \ldots \quad \ldots \quad (2)$$

where M is given by

$$M = p\, A\, N\, 10^{-8} \quad \ldots \quad \ldots \quad (3)$$

We shall call M the *induction factor* of the motor. Since the tension, e, induced at n revolutions per second is given by

$$e = p\, A\, N\, n\, 10^{-8} \text{ volts} \quad \ldots \quad \ldots \quad (4)$$

the induction factor may be found by dividing the induced tension in volts by the speed in revolutions per second, and the induced tension is given by the product of the induction factor and the speed.

The constant p may be defined as the ratio of the number of surface conductors in series between the main terminals to the number of surface conductors lying between two adjacent neutral points, and is unity for a bipolar machine, whether drum-wound or ring-wound.

When a motor is running at n revolutions per second, and taking a current of C amperes, we have the following expression for the speed :—

$$n = \frac{E - C R}{M} \quad \ldots \quad \ldots \quad \ldots \quad (5)$$

where E is the terminal tension in volts, and R is the resistance of the motor in ohms measured between the same points as the

Prof. Carus-Wilson.

tension. Hence, in the case of a railway motor, the speed in feet per second is given by

$$s = 0{\cdot}262 \frac{d}{\mathrm{M}\, v} (\mathrm{E} - \mathrm{C\,R})\ldots \qquad \ldots \qquad (6)$$

where v is the ratio of the speed of the motor to that of the main axle—afterwards called the velocity ratio—and d is the diameter of the driving wheel in inches.

If an experiment be made in which the speed, the tension of the line, the current, and the resistance are observed, we can find from equation 5 the value of the induction factor for different currents, and thus obtain what we shall call the *induction curve.* Such a curve is given in Fig. 1, for the "G.E. 800" railway motor made by the General Electric Company.

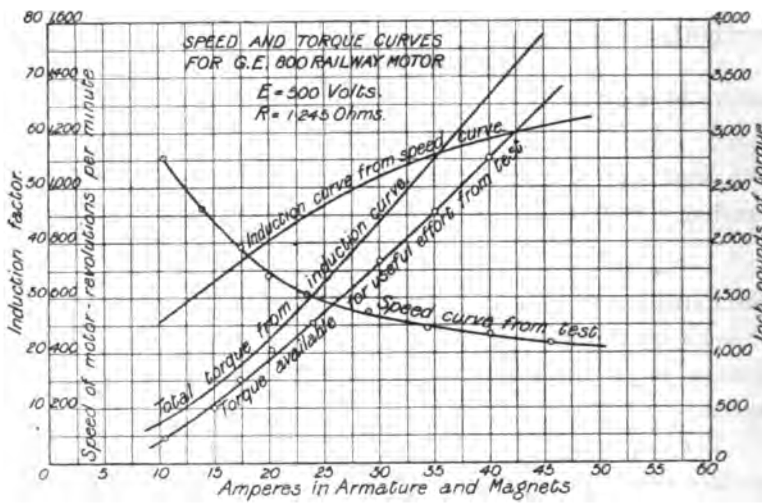


FIG. 1.

From the induction curve we can deduce the curve of total torque for different currents. This curve will lie above that obtained by measuring the torque at the rim of the brake-wheel, the difference for any current representing the torque expended in overcoming friction of gearing, hysteresis, &c. The ratio of the two ordinates for any current gives the mechanical efficiency for that current.

If the current passing through the motor at any instant

is greater than that required to overcome the frictional and other resistance to motion, the motor will accelerate, and the acceleration in feet per second per second will be given by Prof. Carus-Wilson.

$$a = 405 \times 10^{-4} \frac{M\,v}{d} \frac{C_a}{W} \quad \dots \quad \dots \quad (7)$$

where C_a is the current in amperes available for acceleration, and W is the whole weight that has to be accelerated, in tons of 2,240 pounds.

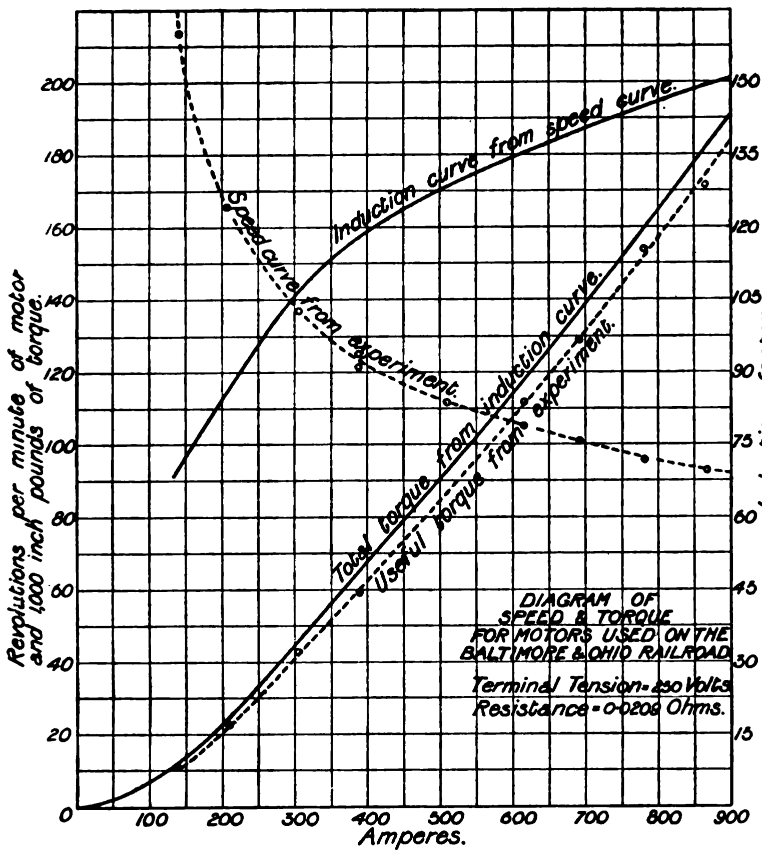


FIG. 2.

As an illustration, we may take the motors used on the Baltimore and Ohio Railroad. The conditions are as follows:—A train weighing 780 tons has to start from rest on a grade of

Prof. Carus-Wilson. 0·8 per cent. The train is drawn by a locomotive equipped with four motors permanently connected in series. The driving wheels, which are gearless, have a diameter of 62 inches. The maximum current from the line is limited to 1,800 amperes, and the mean value of M while the motors are starting may be taken as 155.

The tractive effort per motor required for the grade is 3,490 lbs., and for friction, allowing 9 lbs. per ton, 1,755 lbs., making altogether 5,245 lbs. If we allow 95 per cent. mechanical efficiency, we find from equation 2 that the current required to overcome friction must be equal to 780 amperes, leaving 1,020 amperes available for acceleration. Under these circumstances the train will start up from rest with an acceleration of 0·53 f.p.s. per second. The induction curve of these motors is given in Fig. 2, and the current-curve observed in starting is given in Fig. 3.

If a pulley of d centimetres diameter is placed on the shaft of a motor of induction factor M, carrying a current of C amperes, the tangential force at the rim of the pulley is given by

$$T = \frac{1}{\pi d} \, M \, C \, 10^7 \text{ dynes} \quad \dots \quad \dots \quad (8)$$

If $d = \frac{1}{\pi} \, 10^7$ centimetres, this may be written,

$$T = M C \text{ dynes} \dots \quad \dots \quad \dots \quad (9)$$

The force of a motor may thus be defined as a force of M C dynes at the rim of a pulley 10^7 centimetres in circumference. We shall call M C the *force factor* of the motor. Thus, in the preceding example, each of the four motors must have a force factor of 279 kilodynes in order to start up with an acceleration of 0·53 f.p.s. per second.

The rating of a motor in horse-power gives us no indication of its ability to accelerate, though this may be the most important function it is called upon to perform. Thus, in the last example, the horse-power of the motors at the moment of starting is nothing. In the problem that we now propose to discuss we shall find it convenient to be able to define the action of a motor in terms of a force unit instead of a power unit, and for this purpose

we shall make use of the force factor. We may note in passing that the power in kilowatts at any moment is given by multiplying the force factor in kilodynes by the number of revolutions per second. Prof. Carus-Wilson.

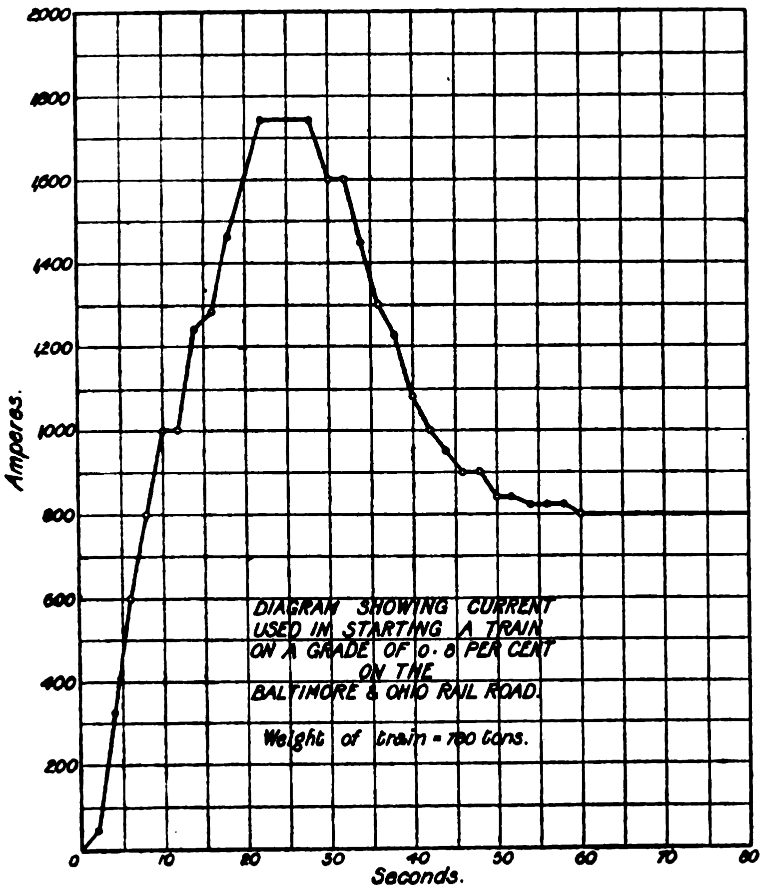


Fig. 3.

When a given distance has to be covered, we may divide the whole period of motion into two parts—that of acceleration, and that of uniform speed. For the present we shall assume that, if there are two or more motors in the locomotive, they are

Prof. Carus-Wilson connected in parallel, and that they speed up with uniform acceleration until full speed is reached.

From equations 6 and 7 it appears that, if everything else remains unchanged, the acceleration increases directly, and the final speed inversely, as $\frac{M\ v}{d}$. For example, if we keep M and v the same, we can increase the acceleration by putting on a smaller wheel, but we shall thereby reduce the final speed. The accelerating period will then be small, and most of the distance will be covered at full speed.

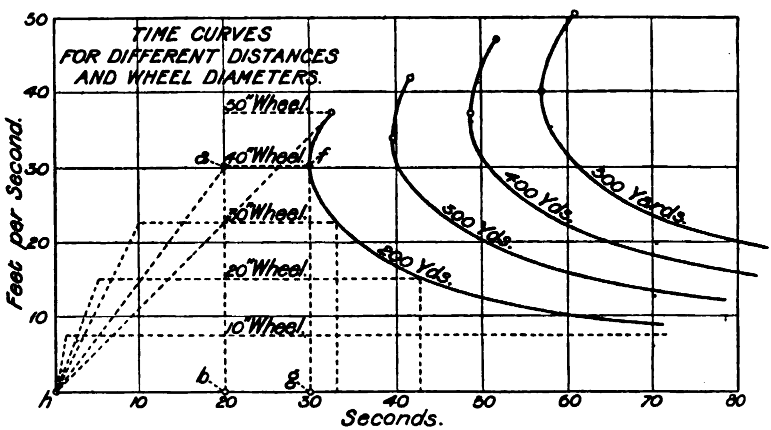


FIG. 4.

On the other hand, if we increase the diameter of the driving wheel, we shall get a small acceleration but a high final speed; most of the distance will then be covered during the process of accelerating, and full speed may not be reached before the given distance has been traversed. Similarly, if we vary the velocity ratio, keeping M and d the same, we shall get the reverse of these results. Or, if we keep v and d fixed and vary M, we shall get the same results as if we varied the velocity ratio.

In Fig. 4 the horizontal axis represents seconds, and the vertical axis speed in feet per second. Let us suppose that the conditions are such that with a driving wheel 40 inches in

diameter an acceleration of 1·5 f.p.s. per second is obtained, and that the final speed is 30 feet per second. A distance of 200 yards will then be covered in 30 seconds, 20 seconds being occupied in accelerating, during which time 100 yards is covered, the remaining 100 yards being covered in 10 seconds at full speed. Prof. Carus-Wilson.

If now we replace the 40-inch wheel by one whose diameter is 30 inches, we increase the acceleration to 2 f.p.s. per second, but reduce the final speed to 22·5 f.p.s., so that it takes 33 seconds to travel 200 yards. If we put on a 50-inch wheel, the acceleration is decreased to 1·2 f.p.s. per second, and full speed is only just reached when the 200 yards has been covered, the time being nearly 33 seconds.

If a line, such as $\{a\,f\}$ in the figure, is drawn to a point at which the given distance is covered, the points thus found by using wheels of different diameters will lie on a curve. We shall call this the *time curve*. In the figure, dotted lines such as $\{h\,a\}$ represent the accelerating period, and dotted lines such as $\{a\,f\}$ the period during which the motors are running at full speed. The area $h\,a\,f\,g$, then, represents the whole distance covered in the time $\{h\,g\}$.

Time curves have been drawn for distances of 200, 300, 400, and 500 yards. An increase in the value of M or of v gives the same result as a decrease in the value of d.

It is evident that there is a certain value of $\frac{M\,v}{d}$ for which the time occupied in covering any given distance is a minimum. This value we shall now proceed to find.

We know from equation 7 that the acceleration varies inversely as $\frac{d}{M\,v}$. We may express this fact as follows:—

$$\frac{\{a\,b\}}{\{h\,b\}} = k_1\,\frac{1}{\beta} \quad \ldots \quad \ldots \quad \ldots \quad (10)$$

where k_1 is a constant, and $\beta = \frac{d}{M\,v}$.

From equation 6 we have

$$\{a\,b\} = k_2\,\beta \quad \ldots \quad \ldots \quad \ldots \quad (11)$$

where k_2 is a constant.

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If D is the whole distance in feet that has to be covered, we have

$$D = \tfrac{1}{2} \frac{k_2^2}{k_1} \beta^3 + k_2 \beta \times \{b\, g\} \quad \dots \quad \dots \quad (12)$$

hence, by substitution, we get

$$t = \frac{D}{k_2 \beta} + \tfrac{1}{2} \frac{k_2}{k_1} \beta^2 \quad \dots \quad \dots \quad (13)$$

where t is the time occupied. To find what value of β makes the time a minimum, differentiate and equate to nothing, and we have $\beta^3 = \frac{k_1}{k_2^2}$ D, or $\{b\, g\} = \tfrac{1}{2} \{b\, h\}$. The given distance then is covered in the shortest time, when the equipment is such that the distance travelled during the process of acceleration is equal to that travelled at full speed, the time of accelerating being two-thirds of the whole time.

Substituting for k_1 and k_2 their values as given by equations 7 and 6, we get

$$\left\{\frac{d}{M\, v}\right\}^3 = 0{\cdot}59 \ \frac{D}{W} \frac{C_a}{(E - c_f R)^2} \quad \dots \quad (14)$$

It appears, then, that, when a train of weight W tons has to be started from rest and moved through a distance of D feet, the tension of the line being E volts, the accelerating current c_a amperes, and the internal drop when running at full speed c_f R volts, the time occupied is least when the ratio $\frac{d}{M\, v}$ is that given by equation 14; and that, if this value of $\frac{d}{M\, v}$ is adopted, half the distance will be covered in the process of accelerating.

Since the equation 14 gives the value of $\frac{d}{M\, v}$ for covering any distance in the least time for a given accelerating current, it follows that, when the time as well as the distance is given, the accelerating current will be least when half the distance is covered during acceleration.

For, if any other ratio of $\frac{d}{M\, v}$ is adopted than that which covers half the distance during acceleration, the time will be prolonged, and consequently a greater accelerating current required.

We have, then, two conditions to fulfil. First, half the distance must be covered at full speed in one-third the time. If we are at liberty, as we generally are, to adjust the value of the resistance so that the drop at full speed is independent of M, v, and d, we then have Prof. Carus-Wilson.

$$\frac{M\,v}{d} = 0{\cdot}1747\ \frac{t\,e}{D} \qquad \ldots \qquad \ldots \qquad (15)$$

where e is the induced tension at full speed, or the tension of the line minus the heat drop.

It thus appears that the ratio $\frac{M\,v}{d}$, which governs the design of the whole equipment, is given by the consideration that half the distance must be covered at full speed in one-third of the time.

The accelerating current can now be found from equation 7. We know that half the distance has to be covered in two-thirds of the time: this gives us the acceleration. We know also the value of $\frac{M\,v}{d}$, and of W: hence we deduce,

$$C_a = 55{\cdot}5\ \frac{D\,W}{t^2}\ \frac{d}{M\,v} \qquad \ldots \qquad \ldots \qquad (16)$$

or we may write at once,

$$C_a = 318\ \frac{D^2\,W}{e\,t^3} \qquad \ldots \qquad \ldots \qquad (17)$$

If we know the retarding forces at full speed we can find the current, since $\frac{M\,v}{d}$ is fixed, and hence we can obtain the resistance of the motor.

For example, suppose that we have to design an equipment by which a tram-car weighing 10 tons can be started from rest and moved through 500 feet in 30 seconds. We may suppose, further, that two motors are to be used, connected in parallel throughout; that the tension of the line is 500 volts, and the drop at full speed 9 volts.

From equation 15 we obtain the value of $\frac{M\,v}{d}$, and find it to be 5·15. We may assume for the present that v is limited to 4·78, and that d is 33 inches; hence M = 35·5. The maximum speed is 25 feet per second, or 17 miles per hour. If the

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frictional and other forces retarding the motion are equal to a torque of 3,580 inch-pounds on each axle, the current at full speed will be 15 amperes, and the resistance of each motor 0·6 ohm.

From equation 16 we find the accelerating current to be 30 amperes; so that the total current at starting is 45 amperes, assuming that the induction factor remains constant throughout. These results are shown in Fig. 5. Horizontal ordinates represent time in seconds, and vertical ordinates speed in feet per second, and also amperes.

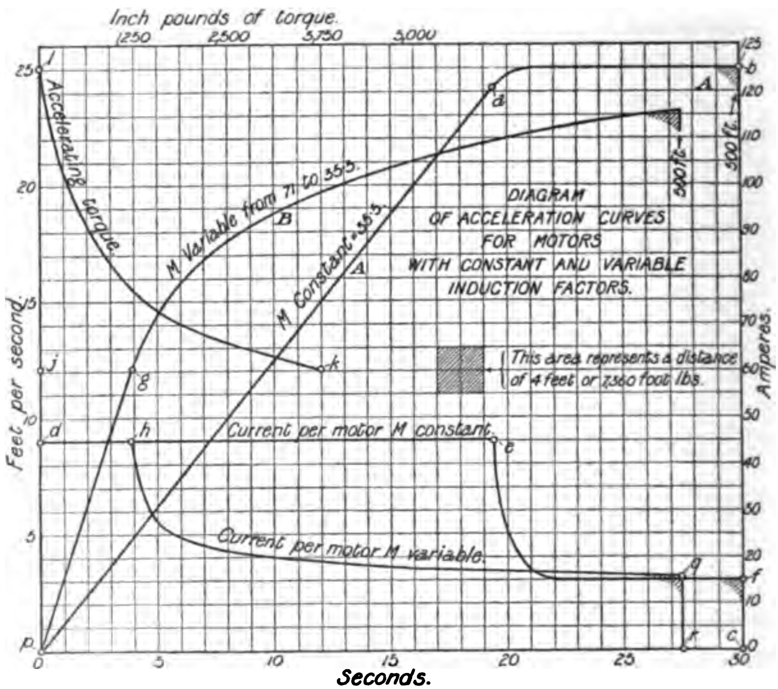

FIG. 5.

The acceleration is 1·25 f.p.s. per second, and can be kept constant until the starting rheostat is all out. The speed at which this takes place can be found from equation 6, by inserting the known value of $\frac{M\,v}{d}$, and putting E = 500, R = 0·6, C = 45. We find that the speed is 24·2 f.p.s., or 97 per cent. of the final

speed. The error involved in assuming that the acceleration is constant up to full speed does not amount to one foot of distance. From the figure we see that half the distance is covered in 20 seconds during the process of accelerating, and the remaining 250 feet is covered at full speed in 10 seconds. The whole area of the curve *p a b c* represents 500 feet. Prof. Carus-Wilson.

The maximum current, 45 amperes, is constant up to the point *a*, when the starting rheostat is all out. This is shown by the current-curve. At the point *e* on this curve, corresponding to the point *a* on the acceleration curve, the current will rapidly diminish; the form of the curve has been calculated and plotted in the figure.

We must now consider the influence of series winding on the curves of current and acceleration. In Fig. 6, let values of the current be measured horizontally, and values of the induction factor be measured vertically. Take *a h* equal to 15 amperes, and set up *h b* equal to 35·5 on the vertical scale. Then *b* is a point on the induction curve of the motor. For, whatever are the values of M for large currents, the value of M for 15 amperes must be 35·5 in order that the motors may run at the required rate at full speed.

Take *a g* equal to the maximum current, 45 amperes. Produce *a b* to cut a vertical line through *g* in *c*. The greatest possible induction factor the motors can have at 45 amperes is given by *g c*, equal to 106 on the M scale. For the induction curve of a series-wound motor cannot be convex to the axis of current, though it may be a straight line passing throughout the origin if no part of the iron in the magnetic circuit is magnetised over the bend of the magnetisation curve. We have shown in this case that the induction curve must pass through the point *b*; hence the greatest possible value of M for these motors is found by making the induction curve a straight line passing through *b*, giving us a maximum induction factor of 106.

Our calculations hitherto have shown us that the motors must have an induction factor of 35·5 at 15 amperes, and that the maximum current at starting must be 45 amperes. We have not, however, determined the value of the induction factor at 45

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amperes. All we know is that if M is constant, and equal to 35·5 for all currents, we shall cover the given distance in the given time.

It is clear that there are an infinite number of possible induction curves, all passing through the point b, having different

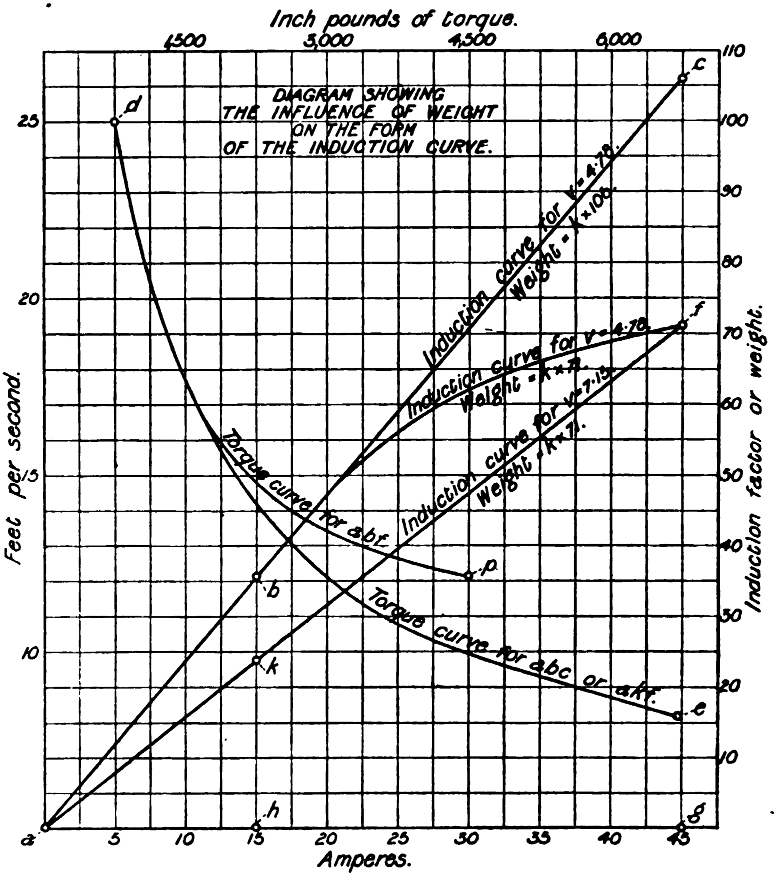

FIG. 6.

values of M for 45 amperes, all less than 106. Any one of these curves would comply with the specification as to time and distance, but we shall see that none of them would be so good from the point of view of economy as the line *a b c*.

When the maximum current to be carried by a motor is fixed,

the weight increases nearly in proportion to the induction factor for that current. We shall assume that for any current the weight is given by k times the induction factor for the current, where k is some constant. Hence, of all induction curves that might be chosen, that given by a b c will involve the greatest weight. Prof. Carus Wilson.

Let us suppose that the practical considerations of space and cost limit the weight of the motors in this case, so that the maximum value of M for 45 amperes is 71—twice that for 15 amperes. The induction curve must then pass through the points a b f; let the curve a b f in the figure be the curve chosen.

From the induction curve we can construct a curve giving the total torque available for all purposes. In the figure this is drawn at d p, horizontal ordinates giving torque in inch-pounds on each motor axle, and vertical ordinates speed in feet per second. By deducting from the horizontal ordinates of this curve the torque required to overcome the retarding forces we obtain a curve of torque available for acceleration. This curve is reproduced at l k in Fig. 5; it cuts the speed axis at 25 feet per second.

We can now construct the acceleration curve for the series-wound motors. The maximum total torque is 4,500 inch-pounds; deducting 750 inch-pounds for the retarding forces, assumed to remain constant at all speeds, we get an initial acceleration of 3·12 f.p.s. per second—more than twice that obtained when M was constant. The speed of the car when the rheostat is all out is 12·1 f.p.s.; this speed is reached in about 4 seconds, and is shown by the point g. From p to g the acceleration is constant. The form of the acceleration curve beyond this point has been found by graphic construction, and continued up to the point at which the area, as obtained with a planimeter, is equal to a distance of 500 feet; this is at 27·5 seconds from the moment of starting.

The current-curve has also been drawn. The maximum current is passing for 4 seconds, after which time the current decreases; the value at any time being obtained from the acceleration curve by using equation 6. An examination of the curves in Fig. 5 shows that the effect of increasing the induction factor by series winding has been to decrease slightly the time required to cover

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the given distance, the saving of time in this case being 2·5 seconds.

If we compare the acceleration curves for the constant and variable induction factors, we shall see that the series-wound motor gains in distance up to the point at which the curves cross one another, and after this point loses in distance. If the distance gained is equal to that lost, there will be no difference in the time required to cover a given distance. This may often happen. The form of the acceleration curve depends upon that of the curve of

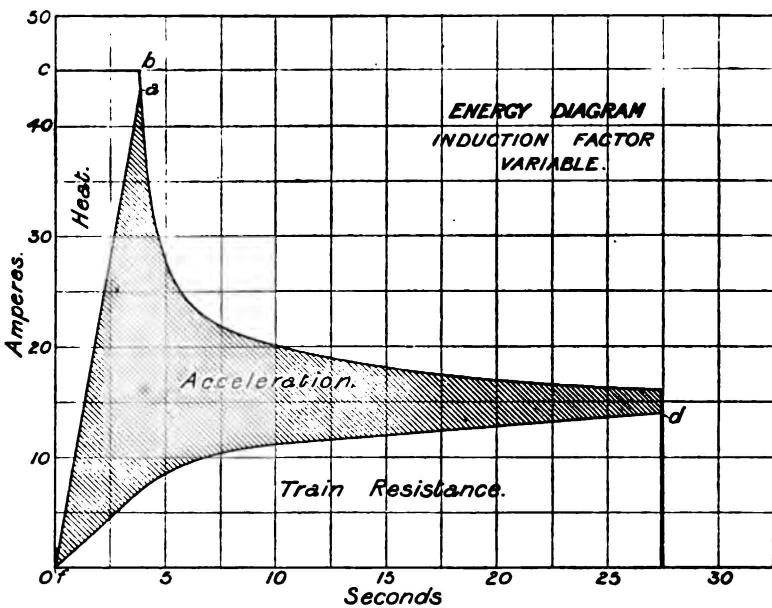

Fig. 7.

accelerating torque. If this is nearly straight between k and l, the acceleration curve will rise up steeply, and the gain in time may be considerable. If, on the other hand, the torque curve is very much bent, the acceleration curve will bend over rapidly, and the series-wound motor will take a longer time to cover the given distance than one with constant induction factor.

The form of the torque curve depends on that of the induction curve. Hence, the straighter we can make the induction curve,

the shorter will be the time required to cover the given distance. A ratio of maximum to minimum induction factor of two to one is very commonly obtained, and in such a case the series-wound motor may show a gain of 5 to 10 per cent. in the time occupied. We have here, then, a reason why the induction curve should be as straight as possible. Prof. Carus-Wilson.

The energy expended in covering the given distance is shown in each case by the area of the current-curve. A glance at the diagram is sufficient to show how great a saving is effected by the use of the series winding.

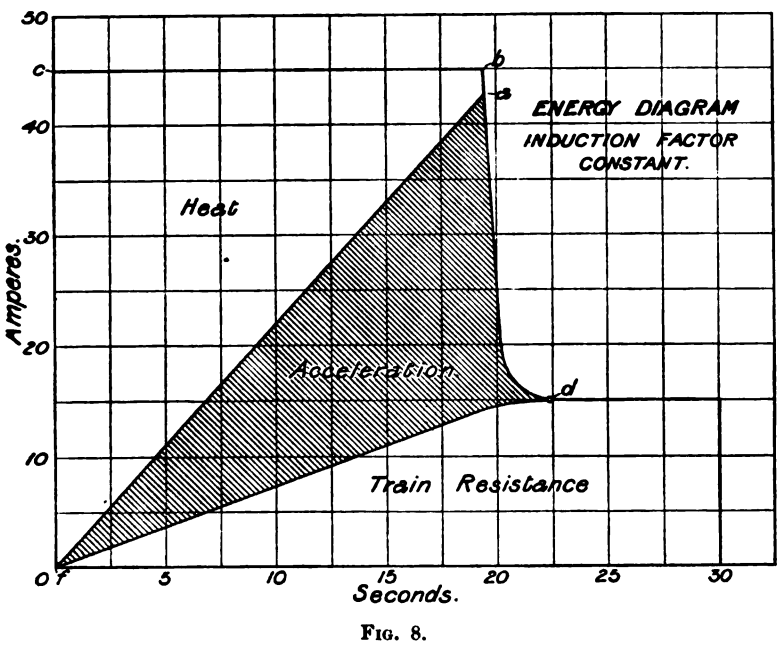

FIG. 8.

The two current-curves have been reproduced in Figs. 7 and 8. If we multiply the vertical current ordinates by the tension of the line, we may take these to represent watts instead of amperes. At the point f the whole of the energy is being expended in heat. The heat loss at any point may be calculated by finding the speed and the resistance in the circuit, and then multiplying this by the square of the corresponding current. If

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the heat watts is divided by the tension of the line, we obtain the part of the total current that represents the loss due to heat.

When the current representing the heat loss has been deducted from the total current at any instant, the remainder represents the expenditure of energy in producing acceleration and overcoming train resistance. The proportion of these two can be obtained from the curve of total torque, since that tells us how much is being used for accelerating, and how much for overcoming train resistance at any speed.

The curves *o a* and *o d* in Figs 7 and 8 have been constructed in this way, thus dividing the whole area into three portions, representing respectively the energy used in heating, in accelerating, and in overcoming train resistance.

In comparing the two diagrams we see that the areas giving the energy used in overcoming friction must be the same; for the distance is equal, and so is the frictional resistance to motion. In this case the energy thus expended is, by calculation, 109 thousand foot-pounds.

Since the final speeds in the two cases are respectively 25 and 23·2 feet per second, the kinetic energy for the motors with constant and variable induction factor will bear to one another the ratio of the squares of these numbers. The values are, by calculation, 109 and 88·7 thousand foot-pounds. There is thus a small gain in favour of the series-wound motors, owing to the fact that the final speed is less than with the motors with constant induction factor.

It is, however, when we come to consider the areas representing the heat loss that we see wherein lies the great advantage of the series winding. The energy expended in heating with the motors having constant M is more than five times that expended with the series-wound motors, the actual values being 32·2 and 169 thousand foot-pounds respectively.

Examination of the diagram shows that the area giving the heat loss is very nearly one-half of the area of the current-curve up to the point at which the starting rheostat is all out. Now the effect of the series winding is to reduce the time during which the starting rheostat is in the circuit. And this reduction is

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brought about in two ways. First, the speed at the point when the rheostat is all out is reduced in direct proportion as M is increased. Second, the increase in the initial acceleration sets back this point still further. Thus, in Fig. 5, the point *a* gives the moment when the rheostat is all out with constant M. The speed is 24·2 f.p.s. If M at the start is doubled, owing to the use of series winding, the speed is reduced to 12·1 f.p.s., and the point *g* then still further set back, so that the time is reduced from 20 seconds to 4 seconds.

Since the speed when the starting rheostat is all out varies nearly inversely as M, and the initial acceleration varies nearly directly as M, the area giving the heat loss varies nearly inversely as the square of the induction factor at the moment of starting.

By increasing the induction factor indefinitely we could reduce the heat loss to that due to the resistance of the motor only; in other words, we could do without the starting rheostat altogether. The reason why we are unable to do this is because the maximum possible value of M is determined by the form of the induction curve. Thus, we have seen in Fig. 6 that in this case the greatest possible value of M is 106. If the weight involved in using this value of M were not an objection, we could reduce the heat loss to 13 thousand foot-pounds. Such a value for M would, however, be inadmissible, on account of the cost of construction and the space taken up, and we have to be content with a loss two or three times this amount.

A reference to Fig. 5 shows that the points, such as *a* and *g*, where the rheostat is all out lie on a curve passing through the origin. This curve is nearly a parabola, whose horizontal ordinate varies inversely as M^2. It is thus evident that, the more the heat loss is reduced, the greater will be the increase in M required to effect any further reduction; so that there is a point at which it is not worth while increasing the weight of the motor, the saving effected not being large enough to compensate for the disadvantages of the heavier motor.

The following table shows the expenditure of energy in foot-pounds in the two cases:—

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		Constant Induction Factor.		Variable Induction Factor.
For acceleration	...	109×10^3	...	$88{\cdot}7 \times 10^3$
For train resistance	...	109×10^3	...	$109{\cdot}0 \times 10^3$
For C^2 R loss ...	...	169×10^3	...	$32{\cdot}2 \times 10^3$
		387×10^3		$229{\cdot}9 \times 10^3$

The expression "train resistance" means here all forces opposing the motion, including those due to the friction of the gearing and the torque lost in the motor itself.

Referring once more to Fig. 6, we have seen that the induction curve of the motors must pass through the point b, and that if the maximum value of M is limited to 71 the induction curve must be bent so as to pass through the point f.

If now the velocity ratio employed can be increased, in the ratio of 71 to 106, or—what would come to the same thing—if the diameter of the driving wheel can be decreased in the same ratio, the induction factor at 15 amperes must be reduced to 23·8, so that the final speed may remain unaltered. Let $h\,k$ equal 23·8 on the M scale. It follows that a straight line through a and k will cut the vertical line through g at f, where $g\,f$ is equal to 71 on the M scale.

We have thus made our induction curve pass through the point of maximum M for 45 amperes, and $a\,k\,h$ is the best induction curve from the point of view of economy. We have done this by simply increasing the velocity ratio and altering the inclination of the induction curve to the axis of the current. This inclination will depend upon the permeance of the air gap if the iron circuit is unaltered. Hence, by rightly proportioning the gap and the velocity ratio, we can obtain results approaching very nearly to the greatest possible economy.

Since $h\,k$ in Fig. 6 is equal to $0{\cdot}1747\,\dfrac{e\,t\,d}{\mathrm{D}\,v}$, and $a\,h$ is equal to $2{\cdot}03\,\dfrac{\mathrm{T\,D}}{e\,t}$—T being the retarding force in pounds at the car axle—the tangent of the angle $k\,a\,h$ is given by

$$\tan k\,a\,h = 0{\cdot}086\,\frac{e^3\,t^2}{\mathrm{T\,D}^2}\,\frac{d}{v} \quad \ldots \quad \ldots \quad (18)$$

Hence we can write, Prof. Carus-Wilson.

$$p\,A\,S\,g = 685 \times 10^4 \times \frac{e\,t^2\,d}{T\,D^3\,v} \quad \ldots \quad (19)$$

where p is the numerical constant defined on page 1, A is the number of surface conductors, S is the number of turns per pole in the series winding, each carrying the whole current, and g is the permeance of each polar gap in centimetres.

It will generally happen in practice that the weight limit requires a velocity ratio that is unattainable even with the largest values of d. We have here a difficulty that influences greatly the design of railway motors when spur gearing is employed, namely, the limited clearance between the gear wheel and the ground. We have to get the largest value of v with the smallest value of d. It is obvious that the greatest possible ratio of v to d is determined simply by the clearance.

If single-reduction gearing is used, the largest ratio of v to d is limited by the number of teeth in the pinion for a driving wheel of given diameter. For example, let us take a driving wheel 33 inches in diameter. If the clearance between the casing of the gear wheel and the level of the rail is limited to 4⅝ths of an inch, we cannot get more than 67 teeth in the gear wheel. If the least number of teeth in the pinion is 14, the velocity ratio is limited to 4·78, and the ratio of v to d is limited to 0·145. These dimensions and numbers are taken from the standard street railway equipment made by the General Electric Company

In our example, if the driving wheels were 33 inches in diameter, the velocity ratio required to get the best results would be 7·15. This would be impossible with single-reduction spur gearing. We should therefore have to use a smaller value of v than the best.

If the series-parallel controller is used, the maximum current from the line at the moment of starting is reduced by one-half. Since the current per motor is the same as with the parallel controller, the acceleration will be unaltered. The motors can be held in series until the speed is 5·7 f.p.s.; the result then is to reduce very nearly by one-half the expenditure of energy due to heat. In estimating the energy required to cover any distance,

Prof. Carus-Wilson. we may generally assume that the effect of series-parallel control is to halve the heat lost.

As an illustration of the application of these principles to the heavier class of railway work we may take the Metropolitan Elevated Railroad of Chicago. Particulars of this railway have been given by Mr. M. H. Gerry, and may be found in a paper published in the *Proceedings of the American Institute of Electrical Engineers* for 1897.

The rolling stock consists of motor cars and passenger cars. The former measure 47 feet in length, and weigh 62,000 pounds when fullly loaded. They are mounted on locomotive trucks, with driving wheels 33 inches in diameter, the velocity ratio being 3·18. One truck of each motor car is equipped with two motors.

The passenger cars are 47 feet in length, having trucks fitted with 30-inch wheels, and weigh 46,000 pounds when fully loaded. Trains of two, three, and four cars are made up according to the demands of the traffic at different hours. We shall consider a train of one motor car and three passenger cars, weighing in all 90 tons. We shall take the case of two stations separated by a distance of 2,500 feet of level track, that has to be covered in 100 seconds from start to stop.

If the distance covered during the period of retardation bears to the time occupied the same ratio as the whole distance to the whole time—*i.e.*, if the mean speed during retardation is equal to the schedule speed—the value of $\frac{\mathrm{M}\,v}{d}$ will be independent of the time during which the brakes are on. For this quantity depends only on the ratio of t to D, and by our supposition this is unaltered by the length of the retardation period. The final speed will therefore be unaltered, and hence the energy expended in accelerating will be independent of the rapidity of stopping.

Again, the accelerating current varies as $\frac{\mathrm{D}^2}{t^3}$, hence it will decrease as t increases; *i.e.*, the accelerating current will decrease with the time occupied in braking. But the work done in heating will be nearly the same, since C_a constitutes by far the greater proportion of the whole starting current.

The energy spent in overcoming friction, however, will increase with the distance during which the motors are working, but the amount of increase will generally be a small proportion of the whole energy thus spent. If, then, the mean speed of retardation is equal to the schedule speed, we may determine the time occupied and the distance covered during the retardation period simply with reference to the ability of the brakes to stop the train. In the case before us we shall allow 20 seconds and 500 feet for retardation, leaving 2,000 feet to be covered in 80 seconds. Prof. Carus-Wilson.

The tension of the line is 500 volts. If the drop at full speed is limited to 5 volts, we find from equation 15 that $\frac{M\,v}{d}$ must be 3·46. If we adopt the existing values of v and d, we get $M = 35{\cdot}9$.

From the results of tests made on this line, the retarding forces at 15 miles an hour, including gear friction, amount to 13·6 pounds per ton of load, or 614 pounds horizontally per motor. Hence the current at full speed will be 63 amperes, and each motor must have a resistance of 0·0795 ohm. The train resistance, excluding gear losses, amounted to 450 pounds per motor.

We have thus found one point on the induction curve, namely, $M = 35{\cdot}9$ for 63 amperes. In Fig. 9, horizontal ordinates represent current, and vertical ordinates values of M. Take a point, a, giving $M = 35{\cdot}9$ for 63 amperes.

The accelerating current is found from equation 16 to be 226 amperes. If the induction factor at the start is twice that at full speed, the current then required for friction is only 31·5 amperes, so that the total current at starting must be 257 amperes, and the corresponding induction factor 72. This gives us a second point on the induction curve, and is plotted at b in the figure. We shall suppose that A is the best curve that can be obtained passing through the given points.

The diagrams of current and acceleration with motors having A as their induction curve are given in Figs. 10 and 11, and are drawn in full lines. The time taken to cover 2,000 feet is 78 seconds, the saving of two seconds being due to the series winding. Full speed is 37·3 f.p.s., but is not reached, the highest speed

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being 33·5 f.p.s., or 23·6 miles an hour. The initial acceleration is 1·27 f.p.s. per second.

The induction curve for the motors actually used is given at B in Fig. 9, and the curves of acceleration and current for these motors are shown in Figs. 10 and 11 by dotted lines. The brakes were applied at the end of 77 seconds, when 1,930 feet had

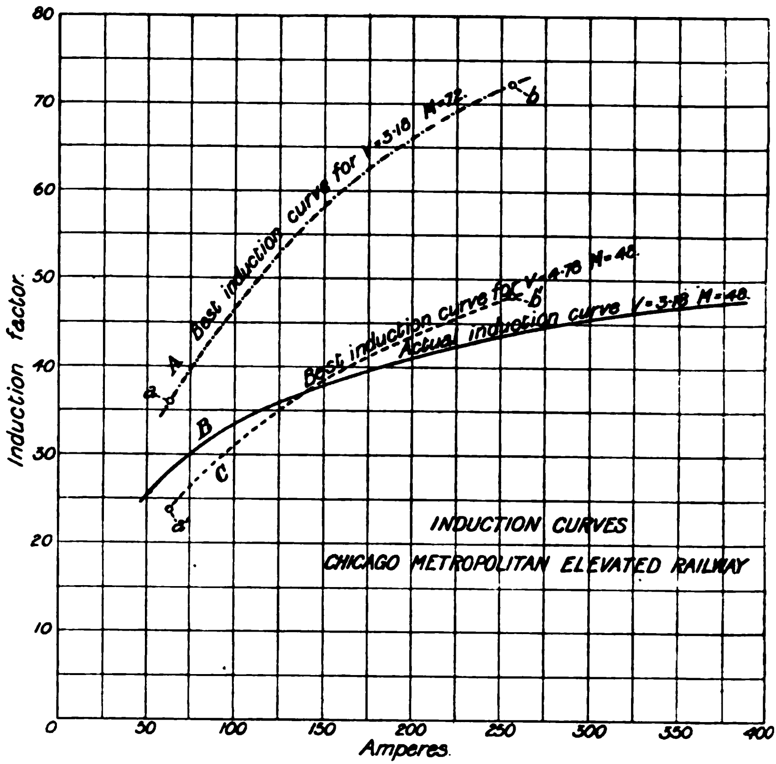

FIG. 9.

been covered; and the remaining distance of 570 feet, making up the total of 2,500 feet, was covered in 27 seconds, making the whole time 104 seconds.

The irregularities in the current-curve are the result of the uneven handling of the controller. The motors take 380 amperes each at the moment of starting, and are allowed to speed up in series for 10 seconds, after the starting rheostat is all out. When

thrown into parallel the current per motor is 330 amperes, or 660 from the line. More careful manipulation of the controller would have effected a better start. Prof. Carus-Wilson.

We have already seen that the force of a motor may be conveniently expressed as the product of the current and the corresponding induction factor. Since the ordinates in a diagram giving the induction curve represent current and induction factor, a curve of equal force is a hyperbola. In Fig. 9 the point *b* represents an induction factor of 72 for a current of 257 amperes; in

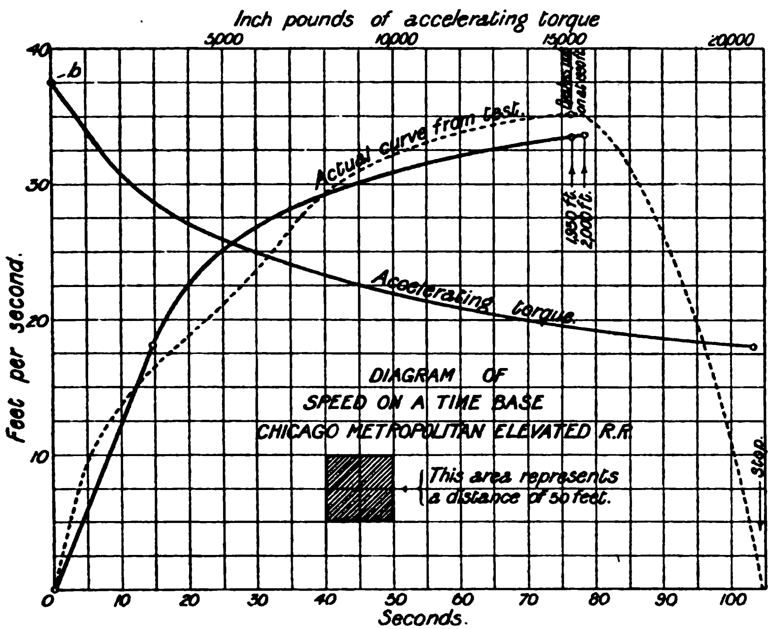

Fig. 10.

other words, the force factor required to start up with an acceleration of 1·27 f.p.s. per second is 18·5 kilodynes. If we draw a hyperbola through the point *b*, it will cut the induction curve B at a point giving the current that the motors in actual use must take in order to get an acceleration of 1·27 f.p.s. per second. The current thus found is 390 amperes. An inspection of the acceleration curves in Fig. 10 shows that the acceleration obtained in the test is rather greater than that obtained by calculation, while

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the current is 380 amperes. The experimental curve, however, is somewhat irregular, and the agreement is as close as might be expected.

The effect of the form of the induction curve B on the current-curve is clearly shown in Fig. 11. The maximum current from the line is 28 per cent., and the maximum current per motor 48 per cent., greater than it need be, while the expenditure of energy in the form of heat is 4·6 times what it would be if curve A had been used.

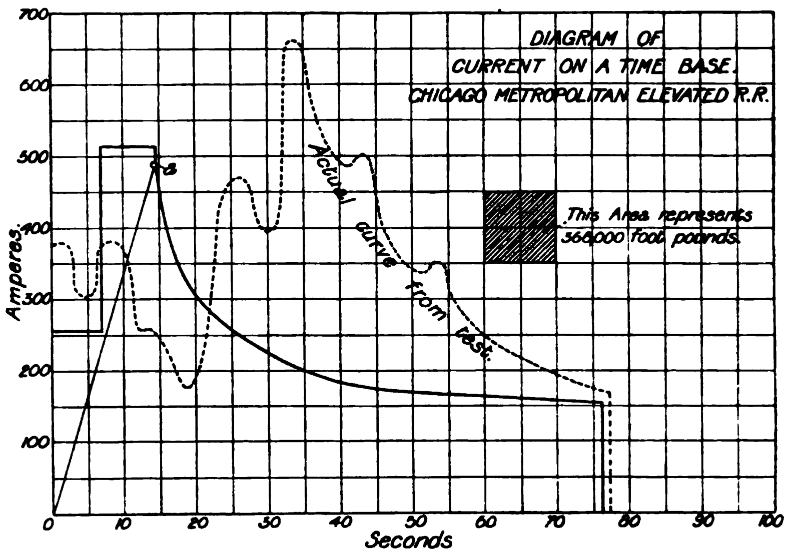


FIG. 11.

The force factor required to drive the train at full speed is given by the product of 63 and 35·9, namely, 2·26 kilodynes. If we draw a hyperbola through the point a in Fig. 9. it will cut curve B at a point giving the current taken by the actual motors when running at full speed, and it will also give us their maximum speed. This will be nearly inversely proportional to the minimum induction factor, and we see that it will be nearly 45 f.p.s., or 30·6 miles an hour.

[1] It does not follow that the highest speeds actually attained in the two cases will be in the ratio of the minimum induction

factors, because the maximum speed is not reached, but we see that the expenditure of energy in acceleration will be greater with the lower induction curve. The values for the kinetic energy in the two cases are 380×10^4, and 350×10^4 foot-pounds. The following table gives the expenditure of energy, expressed in foot-pounds, for a distance of 1,930 feet :— Prof. Carus-Wilson.

	Total Energy.		
	From Test.		Calculated.
For acceleration ...	380×10^4	...	350×10^4
For train resistance* ...	174×10^4	...	174×10^4
For gear loss, at 85 % mechanical efficiency	98×10^4	...	92×10^4
For C^2 R loss	311×10^4†	...	67×10^4
	963×10^4		683×10^4

The greatest induction factor in the motors actually used is 48. If we take this as the limiting value of M, we see that we must increase the velocity ratio in the proportion of 48 to 72 to get the best results. In Fig. 9 curve C has been obtained by taking each vertical ordinate this proportion of the corresponding ordinate of curve A. If, then, we make the motors with this induction curve, they will work with the same economy as those with curve A, provided that the velocity ratio is increased in the proportion of 48 to 72—*i.e.*, from 3·18 to 4·78.

We are thus able to make a clearer comparison between the two motors. The new curve crosses the actual induction curve, but gives higher values of M for large currents and lower values for small currents. The changes that would have to be made to give the best results are: First, the velocity ratio should be increased from 3·18 to 4·78; second, the air gap should be increased in width so that the induction curve may pass through the point a'; third, the section of the iron in the magnetic circuit should be increased so that the curve may pass through the point b'.

* $900 \times 1{,}930$.

† Obtained by deducting the previous amounts from the total expenditure, as found by integrating the current-curve.

Prof. Carus-Wilson.

The economy of working under these conditions may be expressed best in terms of the work done per ton-mile, the distance, of course, including that during which the brakes are on. The energy expended may be conveniently stated in watt-hours.

Thus, in the example we have been considering, the economy attainable with induction curve C is 60·5 watt-hours per ton-mile. We here assume that the velocity ratio is limited to 4·78. With a maximum induction factor of 48 the highest possible economy is obtained with a velocity ratio of 9·75; the heat loss is then reduced to 17×10^4 foot-pounds, and the economy becomes 56 watt-hours per ton-mile. An equally good result could be attained with a maximum M of 72 and a velocity ratio of 6·50. The economy actually obtained with curve B is 85·1 watt-hours per ton-mile.

We may here inquire what would be the economy if the specification had insisted on a gearless equipment. To get the highest economy—*i.e.*, 56 watt-hours per ton-mile—the product $M\,v$ must be 468 for driving wheels 33 inches in diameter. If $v = 1$, the induction factor must be 468—a value much beyond the practical limit. To comply with the conditions as to time and distance, the minimum induction factor would have to be 114; if we take the maximum induction factor twice this value, we get the maximum M equal to 228, and a heat loss of 72×10^4 foot-pounds.

Since we have now dispensed with gearing, we can reduce the item in the table of energy expenditure due to gear loss. Assuming 95 per cent. mechanical efficiency, this becomes 31×10^4 foot-pounds; hence the economy is 55·5 watt-hours per ton-mile, the current for maximum and minimum induction factors remaining the same.

It is, however, unusual to find the maximum induction factor twice the minimum induction factor in motors of this size; a ratio of 1·5 to 1 is more usual. If we take 171 as the maximum value of M, the heat loss is increased from 72×10^4 to 162×10^4 foot-pounds, and the economy of working is 63·4 watt-hours per ton-mile. Thus with geared motors the expenditure of energy is

about 5 per cent. less than with gearless motors of four times the weight. Prof. Carus-Wilson.

The expenditure of energy per ton-mile may be reduced if the track, instead of being level throughout the whole distance, is provided with down grades at the station exits.

The best results would be attained if the train could actually start on the down grade. This, however, is impracticable; but the train should be brought to a standstill as near the top of the grade as possible, in order to get the full advantage of the grade.

Let us suppose that the centre of gravity of the train moves through 150 feet before coming to the top of the grade, and that the force of gravity acts on the whole train throughout the length of the grade, which we shall take to be 300 feet with a fall of 9 feet.

The energy due to the train falling through a vertical distance of 9 feet is 181×10^4 foot-pounds; but this does not represent the gain due to the grade, for the final speed has been increased from 33·5 to 35·5 feet per second, giving an increased expenditure for kinetic energy of 43×10^4 foot-pounds: the difference represents the benefit due to the grade, and is 138×10^4 foot-pounds. With this arrangement there is a gain of about 6 seconds in the time of covering 1,930 feet. We might, then, have taken a larger value of t in our original calculations, relying on the gain in time due to the grade to reduce the time to that specified.

If, then, we take 82 instead of 76 seconds for a distance of 1,930 feet, the kinetic energy would be reduced to 162×10^4 foot-pounds, the $C^2 R$ loss to 47×10^4 foot-pounds, and the gear loss to 60×10^4 foot-pounds; giving a total of 466×10^4 foot-pounds, and an economy of 39·4 watt-hours per ton-mile. The grade thus effects a saving of about 33 per cent. of energy with the geared motor, but only 28 per cent. with the gearless motor.

The results are summarised in the following table. The energy expended is expressed in terms of a unit of 10,000 foot-pounds. The distance is 1,930 feet throughout, and the weight of the train is 90 tons. In each case we suppose that the minimum

Prof. Carus-Wilson.

value of M is the best possible—*i.e.*, that the expenditure of energy in accelerating is a minimum.

Track:	Level.	Level.	Level.	Level.	3 % Grade.	3 % Grade.
Maximum M	48	72	468	171	48	171
Velocity ratio	4·78	6·48	1	1	4·78	1
Kinetic energy ...	350	350	350	350	162	162
Train resistance ...	174	174	174	174	174	174
Torque loss	92	92	31	31	60	18
C^2 R loss	67	17	17	162	47	162
Total energy	683	633	572	717	443	516
Watt-hours per ton-mile	60·5	56·0	50·6	63·4	39·4	45·6

Mr. Taylor.

Mr. A. M. TAYLOR: There is only one point I wanted to call attention to as rather of interest in the paper, and that is Fig. 5, on page 590. A remark which is apt to be misleading occurs at the top of the next page (page 591), viz.: "The error involved in "assuming that the acceleration is constant up to full speed does "not amount to one foot of distance." There was an interesting paper read not long ago in America, by Professor S. H. Short, showing the great advantage that would be obtained if we could continue the acceleration uniformly through a greater range than we at present do; and, although the author's remark applies only to the shunt motor curve, it seems to me that this question which is raised here about the point on the curve where the acceleration ceases to be uniform is one of importance, because, if we are dealing with series-wound motors—as we almost always are for traction work—and if the motor is to run on the level at a uniform maximum speed, the pull required on the level will be so very much less than when accelerating that there must be an intermediate period between the two conditions, and during that period the acceleration of the car will be rapidly diminishing. Professor Short in his paper gives two cases, and shows in the one case the distance that will be covered in a certain time by the ordinary method of control, and in the other case the distance that will be covered by the method of control that he advocates. I should like to ask Professor Carus-

Wilson if he could tell us something about that method, because Mr. Taylor. it is a very interesting one. This method aims at continuing the acceleration with nearly its initial value until practically the final speed is reached; and where we are dealing with high-speed railways, in which it is of vital importance to get from one station to another in the minimum time, that is a matter of great moment. I could give you some figures, if I may, about that With Professor Short's control he runs a distance of 1,810 feet in 61½ seconds; and with the ordinary control he runs the same distance, other things being equal, in 66 seconds. The determining factors which would come in in a question like that are the adhesion of the locomotive, at the start; and also the retardation, which you must put on in braking, at the finish. He (Professor Short) has taken the same values for those two factors in each case, making the results directly comparable. I had recently occasion to work out in detail a similar case, and in that case we had 1,128 feet covered in 50½ seconds before the uniform (limiting) speed was reached. That is with the ordinary controller. Now, if we could have had a controller which would keep the acceleration constant until uniform speed was reached, we could have covered that 1,128 feet in 47½ seconds, instead of 50½ seconds, and there would have been 3 seconds saved. Further, since the full speed, which was 47½ feet per second, would have been reached 3 seconds earlier, we could in those 3 seconds have covered no less than 142½ additional feet, instead of the one foot instanced by Professor Carus-Wilson. So I think we must not quite neglect this matter, because 142½ feet additional on a run of 1,128 feet is not altogether a negligible quantity. Therefore I think that this question of the constant acceleration control which Professor Short has advocated is really a very important one, and we ought to pay some attention to it. I should be very glad if anybody here has considered it, and would give us his views. I might just mention in passing that there is an interesting device, which to some extent lessens the necessity for such a controller on most high-speed railways, such as the Central London and others which we are now working, namely, a down

Mr. Taylor. grade arranged to help the start. That down grade, of course, cannot commence in the very centre of the station, and it is always arranged to commence a little outside it. The consequence is that the train runs on the level part in the station with uniform acceleration, and just when the series motors, through their inherent property, are failing to maintain the acceleration, the down grade comes to help and to add acceleration. I have notes of a case here which I had recently occasion to work out (the diagram is too small to show you) in which this down grade actually brings up to a straight line the part of the curve where otherwise the acceleration would fall off, due to the reasons already given. This shows the great advantage of these down grades arranged as in this class of railway. Of course they have other advantages, which are well known.

[*Communicated.*]—Professor Carus-Wilson is to be congratulated on the admirable manner in which he has, in his paper, brought out the essential differences between the results that may be expected if series-wound, or if shunt-wound, motors be employed.

As his diagram (Fig. 5) forms a sort of *resumé* of the subject, I propose to confine my remarks thereto.

It is, I think, tolerably clear that Professor Carus-Wilson intended his paper to apply more particularly to that type of railway (such as the Central London) where we have stations in fixed positions and at frequent intervals, necessitating rapid accelerations and high speeds, rather than what we term tramways in this country, in which such speeds are not generally required.

Referring to his Fig. 5, it will be evident at a glance that the reason of the marked superiority (both in point of time required for covering a given distance, and in energy required during the run) of the series motor over the shunt motor is due to the shortening of the period that elapses before the resistance is all taken out of the circuit. Professor Carus-Wilson obtains this reduced period by rather more than doubling the initial acceleration in the case of the series-wound motor; and he gives his reasons for this change.

The questions that at once arise in one's mind are these: (1)

Does this constitute a fair comparison? (2) Is it a practical case? Mr. Taylor.

The answer to the first seems to me to be in the negative. The crucial point in his deductions seems to be the selection for the series motor of a final value of the induction-factor of 35·5 with 15 amperes.

Is this the value which, with a series motor and with the range of induction-factor chosen, will get the train over the given distance in the minimum time?

Also, is the energy consumption least when the time is least, where we are dealing with a variable induction-factor?

Also, would not the introduction of a series-parallel control necessitate a change in this induction-factor (see the author's remark about the speed, half-way down page 591)? and does it affect the series and shunt curves equally?

In answer to the second question raised above, I must say that Professor Carus-Wilson appears to be going outside the range of the practical in the case he has chosen. His shunt curve is worked out for an acceleration of 1·25 feet per second per second—a sufficiently high figure, and one seldom exceeded—certainly not with locomotives—on account of the limits of adhesion; but in order to keep to his adopted value of the induction-factor he does not hesitate to take for the series motor an initial acceleration of over 3 feet per second per second.

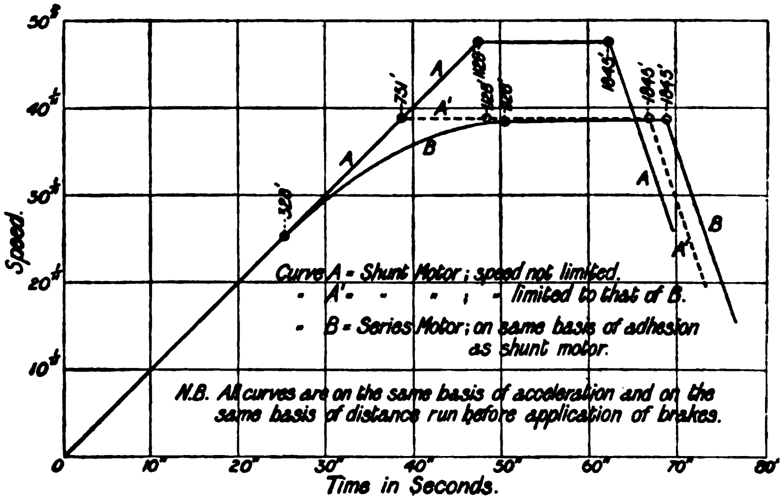
Now this means 210 lbs. pull per ton of train for acceleration only—say 225 lbs., including friction—and if, as in the case of the Chicago Elevated Railway, about one-third of the total weight were on the drivers, we should expect, taking the adhesion at one-seventh, to get only 107 lbs. per total ton of train before the wheels skidded.

At Liverpool this value was 115 lbs. per ton of train, and on the City and South London only 90 lbs.; on the District Railway, 61 lbs.; and in the case I instanced during the discussion of the paper, 85 lbs.

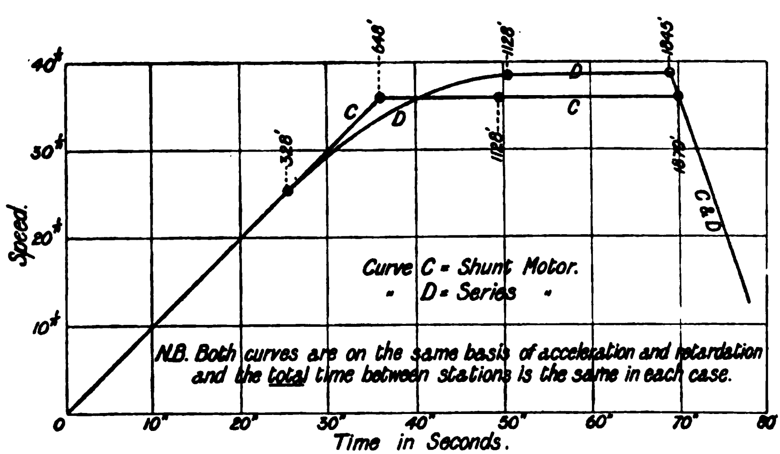
These values do not permit one to raise the acceleration sensibly above 1 foot per second per second, unless all our data as to adhesion can be considerably modified.

Mr. Taylor. (In this connection I would like to ask whether the initial acceleration shown in Fig. 10 was really actually obtained ?)

The question then arises, If we are only able, on account of adhesion, to work at about a limit of acceleration of 1 foot per



T I.



T II.

second per second, should we not, in comparing the series-wound motor with the shunt-wound motor, put both on the same basis as regards initial acceleration ?

If this were done in Professor Carus-Wilson's Fig. No. 5, we should find that not only would the series motor take longer to cover a given distance than the shunt motor, but the saving of the former in energy would be inconsiderable. Mr. Taylor.

In my remarks during the discussion of the paper I gave figures to show that the saving in distance covered in a given time by the employment of a method of constant acceleration—if such a thing were practicable—would be very material; and I am forwarding to you copies of two curves, A and B, drawn to the same basis as regards initial acceleration, the result being a saving in time to the credit of the shunt motor—*i.e.*, of the constant acceleration method.

If, on the other hand (see Curves T I. and T II., which I am forwarding), the maximum speed with the constant acceleration control were lowered so as to make the period of time during which the train is in motion equal in each case, the total energy required would be more nearly equalised; since the energy wasted in the brakes (it has to be supplied from the line as current) will, for an equal rate of retardation, be reduced, owing to the reduced speed employed.

Of course the diagrams, like those of Professor Carus-Wilson, are not strictly correct, in that they assume the resistance to be divided into an infinite number of steps, and the traction resistance to be independent of the speed.

Mr. W. M. MORDEY: I rise, not so much to discuss the paper—which I have been unable to study—as to congratulate the author on having returned to England after having spent eight years in a country where he has had opportunities of studying these problems on the scale of 12 inches to the foot. And I may congratulate him also on having made a first use of his return to give this paper before this Institution. I have just returned from a two months' visit to the United States, and have been greatly interested in all I have seen there. I need hardly say there is much to be learnt there, not only about things worthy of imitation, but as to things which we shall be wise to avoid. The problems which are dealt with in the present paper are now of most pressing moment to very many of us. I would therefore Mr. Mordey.

Mr. Mordey. ask, Sir, that the discussion should be made as full as possible, and that the Institution should have the benefit in the *Journal* of any communications of persons who may not be able to speak to-night. My general observations on the other side have led me to the conclusion that engineers designing traction motors make them as big as they possibly can, and work them with as strong a field as they possibly can. Progress has been entirely in the direction of increase of power. In the main principles they seem to be very much the same as they were years ago. The problems connected with controllers are, I believe, very difficult indeed, and I think there is plenty of room for study and for ingenuity in improving controllers for traction work of all sorts. The two figures Curve 10 and Curve 11 particularly show how very far from perfect controllers are, even after all the experience that has been gained on the other side. The marked drop in current shown in Fig. 11 corresponds to the dip in the curve in the former figure. This controller problem is not one that can be tackled with a very light heart. When I was in America I came across a telling illustration of this. I learnt that one very large concern, in spite of very great experience, and the fact that it had made a great many controllers, and had gradually improved the design, in one instance introduced a new controller which was supposed to embody all the good features of all the previous ones, and none of their bad ones. Yet this new arrangement was a total and absolute failure—a failure that was not realised until many hundreds had been put into use. I mention this as a warning to any person who may be disposed to underestimate the difficulty of this part of the subject.

Prof. Smith. Professor R. H. SMITH: There are only two points really that I wish to direct attention to. Before mentioning them, it would be well to notice the superiority of the American method of rating motors by their force-power, rather than by the method—which is still a common English one—of rating them by horse-power. It is very clearly brought out in these calculations that what is called the horse-power of the motor is of comparatively little importance for the service for which for a good many years to come electric motors will be chiefly used in traction work—

that is to say, quick time service, with many stops. The calculations are very interesting. I cannot criticise them, because I have had no opportunity of reading the paper before I came here this evening, and I could not pretend to follow all the reasoning during the reading of the abstract. But it seems to me that the calculations—I may be mistaken—were carried out on the assumption of a constant train resistance. There are three ways in which the whole energy goes, and one of these is called by the author "train resistance." I think that he assumes it as approximately, for practical purposes, constant. The train resistance, of course, may be roughly and usefully divided into two parts—internal resistance and external resistance—the external being the rail and air resistance. The internal resistance is, to a very useful degree of approximation, constant, but that law does not apply with any degree of approximation to the other part. And then, even with regard to the internal resistance, there is always an extra frictional starting resistance put in at the first moment, just before you get any motion at all. This paper also seems to me to view the whole matter from one point of view, without taking account of another essential point of view. The author makes the calculations chiefly, if not solely, with regard to the possibility of covering a certain distance in a certain time, or finding out the least number of seconds which are required to cover a certain distance. There is another point of view which appealed to me strongly some years ago, when I was calculating the acceleration problems for a different class of machinery, and that is how to get the work done as quickly as possible with minimum stresses in the mechanism and machinery; and I think it is quite certain that, if you put down as one of your conditions of the satisfactory solution of the mechanical problem that the stresses caused throughout the whole machinery shall be as low as they can usefully be made, then you will find that a uniform acceleration right up to the point at which you begin the retardation is the best law to follow; and that should be followed by a period of uniform retardation. The whole distance from start to finish should be covered by two periods — one throughout which uniform

Prof. Smith.

Prof. Smith. acceleration reigns, and the second throughout which uniform retardation reigns.

[*Communicated.*]—Although the general trend of Professor Carus-Wilson's conclusions seems to be justified, so far as regards the problems of shortening the time and minimising the energy spent in covering a given short distance are concerned, it seems that they cannot be accepted as accurate, because his method of mathematical calculation appears to be incorrect. Using his equation (13), he minimises the time to cover the given distance, D, by putting $\beta^3 = \frac{k_1}{k_2^2}$ D. The differentiation leading to this assumes that k_1 and k_2 remain constant while β is varied. Now $k_2 = \frac{1}{4}$ (E − C R), and $k_1 = 0{\cdot}04 \frac{C_a}{W}$. But the variation of β involves inverse alteration of the final speed, s, or n, in such a way that if C, R, E, and, therefore, k_2, remain unchanged, then C_a, the excess of current devoted to acceleration, cannot remain the same. Otherwise stated, the variation of β and the constancy of k_2 form of themselves a law of variation of $\frac{k_1}{\beta}$ inconsistent with equation (7).

Besides this inconsistency, his calculation proceeds on the assumption of uniformity of acceleration during the period of acceleration. Now during this period the current in armature and magnets varies, so that M varies very largely, and C_a also varies, dying away to zero. The manner in which these vary depends, of course, among other things, upon the variation of the time rate at which ohmic resistance is taken out; and the variation of C_a, in particular, depends upon the law according to which the mechanical resistance other than inertia accumulates as speed gets up. This latter law is complicated, as the frictional resistances are high at first, and lessen with increase of speed, while the wind resistance increases throughout. The result seems to be that, although theoretically possible, it is practically inconceivable that the ohmic resistance should be varied exactly, or approximately (except by accident), according to the time law which would produce uniform acceleration.

As a rough approximation, indicating the general bearings of Prof. Smith. this mechanical problem, the assumption of uniform acceleration may probably be very useful; but when rules for minimising certain quantities are arrived at by differentiation, often very large errors in the determination of "best" values result from comparatively small inconsistencies among, or deviations from, the data assumed for the calculation.

Thus, near the foot of page 588, the author too rapidly assumes that the same rule for best proportion between accelerating distance and total distance results when the problem is to minimise the accelerating current for a given time and distance, as when it is aimed to minimise the time for a given accelerating current and distance. k_1 is the term involving the accelerating current, and taking his equation (13) as it stands, it may be transformed into

$$k_1 = \frac{k_2^2}{2} \cdot \frac{\beta^3}{k_2\, t\, \beta - D},$$

where t is the time, NOT the torque. Taking k_2, t, and D as constants, and equating to zero the β gradient of k_1, there results

$$\beta = \frac{D}{2\, k_2\, t};$$

but this gives a NEGATIVE value of C_a; and, in fact, it gives a merely mathematical (that is, a physically impossible) minimum value of the accelerating current. Under the stated conditions the acceleration current required continuously increases as $\beta = \frac{d}{M\, v}$ increases.

The "force-factor," as defined by the author, seems to have little advantage. It practically MEANS the same as the torque, but it expresses the torque as an equivalent force acting at the periphery of a pulley 31·83 kilometres, = 19·7 miles, diameter. This cannot be called descriptive; it conveys the faintest idea to the imagination of the mechanical engineer.

Although I cannot recognise the accuracy of the rule that half the whole distance should be covered during the acceleration in order to minimise the time occupied in covering that distance, still it seems a first approximation to a true principle, the exact results of which may differ a good deal according to circumstances. It may, therefore, be interesting to state this principle in general

Prof. Smith. terms. The power, or working capacity, of any driving machine may be measured by the product of a force and a velocity. This need not be the maximum, or the normal, or any other particular horse-power exerted by the machine; but the measure is in the nature of a horse-power. Call it H, and call the factors F and V, so that $H = F\,V$. Now, suppose that H is fixed by conditions outside the control of the designer of the machine, but that in his design he is at liberty to choose F and V (under the condition $F\,V = H$) so as to best suit his purposes. If the machine is to be started from rest, and is to be run up to and not beyond the velocity, V, then, whatever time variation may be followed by the part of F spent in producing acceleration, if it only follow some definite law during the period spent in acceleration, the time average of the accelerative force during this period will be some definite fraction of F; that is to say, it will have a definite proportion to the force-factor of the power measure of the machine. The time spent in accelerating from zero velocity to V will have, therefore, a definite proportion to the quotient $\frac{V}{F}$. Call it $a\frac{V}{F}$. Among other things, a is proportional to the mass to be accelerated, due allowance being made for the excess of the inertia acceleration of the rotating parts over those moving in rectilinear motion. Again, the time average of the velocity during this period will be a definite fraction of V. Call it ϕV. Then the distance run during the acceleration period is $a\,\phi\,\frac{V^2}{F} = a\,\phi\,\frac{V^3}{H}$.

The whole distance to be run is D, and the remainder is run at the uniform speed V. The time spent on this remainder is therefore $\left(\frac{D}{V} - a\,\phi\,\frac{V}{F}\right)$.

Adding this to the time, $a\,\frac{V}{F}$, spent on acceleration, there results—

$$\text{Whole time spent in running } D = \frac{D}{V} + a\,(1-\phi)\,\frac{V}{F}$$
$$= \frac{D}{V} + a\,(1-\phi)\,\frac{V^2}{H}$$
$$= \frac{D\,F}{H} + a\,(1-\phi)\,\frac{H}{F^2}$$

We suppose D and H outside our power to vary, while we are able to vary V and F inversely without altering the proportions a and ϕ. To minimise this whole time, equate to zero either its V or its F gradient. The result is that the final velocity giving this least possible time is $V^3 = \frac{D\,H}{2\,a\,(1-\phi)}$, and therefore the distance travelled during acceleration, $a\,\phi\,\frac{V^3}{H} = \frac{\phi}{2\,(1-\phi)}\,D$. The factor a strikes out of this, but it must be noted that ϕ and a depend on the same law of growth of velocity, and therefore ϕ is really a function of a. In order that this distance should be $\frac{1}{2}$ D, ϕ must be $\frac{1}{2}$; which it is for the uniform acceleration assumed by Professor Carus-Wilson. It equals D if ϕ be $\frac{2}{3}$. It is less than $\frac{1}{2}$ if ϕ be less than $\frac{1}{2}$. Note that the small increase of ϕ of $\frac{2}{3} - \frac{1}{2} = \frac{1}{6}$, increases this accelerating run from half to the whole of D. Prof. Smith.

This adjustment involves the use of the force-factor,

$$F = \frac{H}{V} = \sqrt[3]{\frac{2\,a\,(1-\phi)\,H^2}{D}}.$$

If, however, instead of asking that a given distance, D, should be run in the least possible time with a machine of given power, H, it were demanded that the power H and the final velocity, V, should be adjusted so that the distance D should be run in a given time, T, with the least possible F—that is, the least possible stresses and strains in the machinery—then we note that

$$F = a\,(1-\phi)\,\frac{V^2}{T\,V - D};$$

and that $\frac{d\,F}{d\,V} = 0$ gives

$$\left\{\begin{aligned}
V &= \frac{2\,D}{T} = \text{twice the velocity necessary to cover D in time T at uniform speed;}\\
F &= a\,(1-\phi)\,\frac{4\,D}{T^2};\\
H = F\,V &= a\,(1-\phi)\,\frac{8\,D^2}{T^3};\\
a\,\phi\,\frac{V^3}{H} &= \frac{\phi}{(1-\phi)}\,D = \text{distance run during acceleration} = D \text{ if } \phi = \tfrac{1}{2};\\
a\,\frac{V}{F} &= \frac{T}{2\,(1-\phi)} = \text{time occupied in acceleration.}
\end{aligned}\right.$$

Prof. Smith. This distance $\frac{\phi}{1 - \phi}$ D is exactly twice the distance given for the acceleration travel under the condition that T is to be minimised for given H. If ϕ were $\frac{1}{3}$—an improbably low value—the distance would be $\frac{1}{2}$ D. If ϕ be more than half, as it must generally be, the mathematical solution is physically impossible, and the best arrangement is to make the acceleration period cover the whole of D.

In tramway and suburban quick service the velocity here given is far below the greatest safe velocity, and therefore I venture to think that this solution accords more with the conditions likely to be set before an engineer than the previous one.

Railway locomotive drivers find it an economical practice to put on full power at starting, maintain it until the greatest permitted speed is attained, and then shut off steam until the velocity has gone down considerably. The running with steam shut off is, I believe, termed "sailing." There are evident advantages in this method of driving. After full speed is reached, the power required to maintain it is a long way below the most economical power of the locomotive, and it conduces to economy to use no steam at all rather than to use it so wastefully. The highest safe speed is, of course, very much greater than "schedule" speed. In England the schedule speed is seldom, if ever, above 50 miles per hour; but there is no danger in from 90 to 95 miles an hour, if the road be in good condition and without sharp curvature, the carriages well coupled, the axle-boxes well greased, the wheels and axles without crack or flaw, and no other train in front on the same rails.

The question of retardation without braking seems to me quite as important from the engineering point of view as that of rapid starting. The ideal system is to have a short steep gradient on each side of the level length at each station. The objection on long lines to the interpolation of these station hillocks is that they obstruct the passage of the through express trains. In running over the hillock the express train loses very little energy by having to climb over it, but its average speed up and down the double incline is necessarily less than if these inclines were levelled out. I cannot see, however, what objection there can be

to this most economical (from every point of view) method of braking on local lines, such as the Metropolitan, where nearly every train stops at every station. Prof. Smith.

Professor W. E. AYRTON: I must follow Mr. Mordey in congratulating Professor Carus-Wilson in having been able to study this question in the country which is the home of electric traction, where he could deal with electric trains using 1,000 or 1,500 horse-power, and having weights of 700 or 800 tons. I also appreciate the importance of reckoning motors on the American system. When you ask about a particular railway motor in America, the answer given you is the draw-bar pull at a particular speed, and the horse-power is not, as a rule, mentioned. There is no doubt, as Professor Smith has said, and as Professor Carus-Wilson has pointed out in his paper, that this is the proper way to rate motors, as long as the runs of electric vehicles without stopping are only over short distances. Prof. Ayrton.

There is unquestionably in this paper which has been presented to us a very large amount of information, and also there are a number of very important suggestions. But, if Professor Carus-Wilson will allow me to say so, there are certain little blemishes which he may like to remove before the paper is finally published in the *Journal*. In the first place, I do not fall in with this unnecessary use of new words. Formerly, everything had to be a "coefficient" of something or other; now, everything must be a "factor"—a "power-factor," a "load-factor," a "form-factor," an "induction-factor," a "force-"factor," &c., &c. We have already had instances of factorial names being suggested at meetings of this Institution which have caught on with English electrical engineers, not because the idea was new at all, but because the word "factor" was employed in them. For example, the ideas involved in the expressions "power-factor" and "form-factor" were first brought before this Institution without the name "factor" being proposed in connection with them, and so they did not attract much attention. Then they were suggested again as "factors," and everyone was struck with the conception. And just as the

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names "power-factor" and "form-factor" have found favour, so we may expect to hear a great deal about "induction-factor," in spite of the fact that there is such an expression as "E.M.F. "generated at one revolution per second," which is simply what the author means by the "induction-factor." But, I would ask, is not more meaning conveyed to many by using a familiar name than an unfamiliar one? and is not the "E.M.F. generated at "one revolution per second" more intelligible than the expression proposed in the paper—an expression which adds yet one more definition of the already hard-worked word "induction"?

What exactly is the object of employing the expression "force-factor" I do not know. To realise its value in dynes, as proposed in the paper, you must apply to the motor a pulley of 10^7 centimetres diameter, or, if your electric car be directly driven by the electro-motor, the driving wheel of the car must have a diameter of 10^7 centimetres, or of about 63 miles. Now is this not somewhat fanciful? Strictly speaking, there appears to be a mistake on page 584 of the paper, since the diameter of the pulley should apparently be $\frac{1}{\pi}10^7$ centimetres. But even that is about 20 miles, which sounds enormous for the diameter of a pulley, or of a driving wheel of an electric locomotive, even after our experience of the big wheel at Earl's Court.

Next, coming to the very interesting question which is dealt with on page 586, viz., that of finding how to proportion the time of accelerating to the time of running at full speed, and of fixing the size of the driving wheels to enable an electrically propelled vehicle to cover a given distance in the least time: When the fixed distances are such as 200 or 300 yards, it is clear that an electric tramway, and not what we call an electric railway, is intended; or perhaps it would be better to say that an electric railway, and not an electric railroad—using the nomenclature of the other side of the Atlantic—is what Fig. 4 (page 586) of the author's paper is intended to apply to. The conclusion arrived at is that for certain given conditions the time is least when the distance travelled while the speed is being increased is equal to that travelled at a constant speed.

Prof. Ayrton.

But this assumes that the vehicle is running at full speed when it arrives at the end of the particular distance, and the time taken on stopping is ignored altogether.

Of course we know that in America the street cars do go very fast; that Americans do get on to the cars very often without their stopping at all, but for the sake of a phlegmatic Englishman, for example, they do occasionally stop; and under these circumstances have not you to consider the problem from rather a different point of view—viz., how to go in the least time from one stopping place to the next stopping place? and might not the conclusion then be different? For the solution that Professor Carus-Wilson finds to be the best for the 200 yards distance, in his Fig. 4, is one in which the vehicle at the end of the 200 yards distance is going four times as fast as in the first case considered. Now to take this higher speed out of the vehicle will require a longer time, and this cannot be entirely neglected in the practical case of electric tramways.

Further, as Professor Perry has suggested to me, it does not follow that a *constant* acceleration is the best condition to employ, and that a driver might find it more expeditious to run with a *variable* acceleration. Also, as Professor Smith has suggested, the consideration of the stresses involved introduces an objection to the running at constant speed at all.

I am therefore inclined to think that the author's solution is perhaps rather academic in not taking into account several conditions that must be considered in practice.

There are one or two minor suggestions which the author may like to avail himself of before publishing his paper. These are rather Professorial suggestions; but he is a Professor, and, therefore, perhaps he will not mind a Professorial criticism. On page 2 the equation he gives for the speed is not quite right, if you are dealing with a shunt motor. And this he obviously is contemplating, because later on he comes to the shunt motor. In the explanation of Fig. 1 there should be "resistance" inserted after the words "and the current." I do not like the expression "tension of the line" at all: the tension of the line, of course, means the strain on the wire; the mechanical strain is

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the tension of the line. Mr. Mather has been kind enough to check the actual numerical calculations for me with his usual care, and has made a number of suggestions. Most of the calculations are numerically correct; 0·53, in the middle of page 584, however, he tells me should be 0·505.

Why has the author neglected air friction, or, at any rate, considered it as a constant throughout his paper? As air friction is about proportional to the velocity at low speeds, and to higher powers at higher speeds, and as it is considerable when a long electric train, as distinct from a short electric tram-car, is considered, it occurs to me that the introduction of this very necessary consideration may alter to an important degree some of the results arrived at in this paper. He also does not seem to me to have introduced any correction for the change in the mechanical friction at the axles of the wheels which will be introduced by a variation in their diameter. Further, the author assumes that the frictional resistance is only 9 lbs. per ton; whereas Professor Short, if I remember rightly, took for overhead electric railroads in America something like 13½ lbs. a ton.

The point which is brought out about the series motor, and shunt motor, at first would strike one as not new; but I think there is more novelty in what Professor Carus-Wilson has drawn attention to, than would at first sight appear. Of course, to adduce theoretical reasons at this late period to prove that a series motor is better than a shunt motor for the propulsion of electric vehicles would seem to be unnecessary, seeing that in every electric tramway and electric railway one or more series motors are employed. Indeed, it was pointed out in this very room by Sir William Siemens, as long ago as 15 years, that it was better to use a series motor than a shunt motor on an electric tram-car in order to get up the speed quickly; but I do not know that therefore it was seen that there would be a smaller amount of energy expended in covering a certain distance. That is the point which has been brought out by the author on pages 594 to 598 of his paper—viz., that not merely is it better from the speed point of view to use a series motor, but that it is better from the economy point of view.

From a speed point of view, of course, it is better to run a steamer or a train as fast as you can, but from a coal point of view it is just the other way. If you take twice the time, you use much less coal. This economy conclusion, then, in connection with the use of series motors strikes me as novel. Prof. Ayrton.

There is also a very interesting result which the author has arrived at, at the bottom of page 598—that "by rightly proportioning the air gap and the velocity ratio, we can obtain results approaching very nearly to the greatest possible economy."

One would like to ask the author, in conclusion, how far the numerous figures given on page 608, at the end of the paper, represent the actual results obtained in practice. Or do they, on the other hand, merely represent the results that would be obtained on the author's supposition? There is possibly, of course, a great difference between values obtained in these two different ways. I mean there are considerations that I have alluded to which the author has not taken into account, and therefore I think it would be very interesting if he would tell us whether the figures at the end of his paper are merely calculated from theory or are deduced from practice.

I end my criticisms—which I hope the author will accept in the spirit in which they are offered, viz., not with the slightest intention of undervaluing his paper, but merely to enable him, if he so wishes, to modify it a little before having it finally printed in the *Journal*—I end, I say, as I started, by congratulating him on the advantages he has had on the other side of the Atlantic in the study of the question, and on the way he has availed himself of these advantages.

Mr. E. K. Scott: When the very full series of curves of the Central London Railway were published in the technical papers, long before the railway had actually begun to work, it was thought by some that they were just a little too previous. We now see, however, from Professor Carus-Wilson's paper, how very accurately these acceleration curves, &c., can be fore-determined; Figs. 10 and 11 especially showing how closely the calculated results compare with the actual. Mr. Scott.

Mr. Scott. I would just like to ask Professor Carus-Wilson how he thinks the motors of railways—not tramways—should be connected. On a tramway where the stopping places are close together, it is now generally acknowledged that we must have series-parallel control; but where the distances from station to station are considerable, need we go to the extra complication of series-parallel control? For instance, take the Liverpool Overhead Railway and the City and South London Railway;—in one case the motors are always in parallel, and in the other case they are always in series, yet both these railways give perfectly satisfactory results in starting, acceleration, &c.

Mr. Taylor mentioned that on the Central London Railway they were using down grades at the stations so as to assist the train in accelerating with minimum current directly the series connection is cut out; and it would almost appear that this kind of thing would do away with *series-parallel* control, even for railways such as the Central London. It would be interesting to know how the use of roller bearings would alter the train resistance curve in Fig. 7. The area enclosed by the lower curve (Fig. 7) would, of course, be reduced, but just how the curve is altered would be worth knowing. A most interesting feature in connection with this paper is that we have for the first time a gentleman from Greater Britain—from one of the Universities of Greater Britain—and it is to be hoped, now a start has been made, that we shall have other gentlemen from the Colonies to give us the result of their experience.

Mr. Grove. Mr. C. E. GROVE: One thing that I had proposed to mention has been very much better done by Professor Ayrton. I, too, have been bothered, in reading the paper, by new expressions like "induction-factor," and I found that one of the first things to do was to translate the formulæ into others which are more common in this country. [Professor AYRTON: Into English.] I think it is a point of some importance that we should, as far as possible, use a uniform notation. Following out that idea, I would mention—and I will throw this out as a suggestion which Professor Carus-Wilson may perhaps adopt—that numerical

constants are introduced into the formulæ which require explanation. Formulæ such as we have in the paper are either empirical or rational. In an empirical formula of the "pocket-"book" order one quite commonly gets figures, and they are not much good to anybody not familiar with the process by which they are derived. A formula which pretends to be rational should, I think, have sufficient explanation added to enable these numerical constants at once to command assent. There is another thing which I would like to ask Professor Carus-Wilson to be good enough to do. The paper is spotted all over with assumptions made for the purpose of calculation: "Let us "assume that the mechanical efficiency is so and so," "Let us "assume the drop is so and so;" but the author does not give us the benefit of saying to what extent these assumptions arise out of the study of the particular case, or to what extent they are purely arbitrary assumptions, made for the purpose of illustrating the subject by numerical calculation. Professor Carus-Wilson would add greatly to the value of his paper if he would kindly add these explanations. It is possible to check some of the figures from one's own experience, but there are some which elude one, because they get beyond one's own knowledge, and I rather think that one or two at least must be wrong. Mr. Grove.

May I add one word as to the question of what is the best acceleration? I think that is a problem that comes very much later usually in the stage of design and study than one would be apt to suppose by reading this paper. One would think that the design of the motor is the first thing done, but that is not the case. A railway or a tramway is laid out with a view to the accommodation of a given amount of traffic; the commercial considerations come first — so many people will have to be transported from place to place per hour—so that one first gets a notion of the loads and speeds to be dealt with. Then the trains have to be arranged to suit the expected traffic, and the schedule is usually fixed before the job comes into the hands of the electrical or locomotive engineer. Then, of course, it becomes a question, not whether it is theoretically best to have a uniform acceleration, or a falling acceleration, or a rising

Mr. Grove. acceleration, but what is the best thing to do under the given conditions. Again, electrical engineers have not been the first people to drive trains or trams, and we must not ignore the experience gained on steam railroads. I have had opportunities of noticing what is done on suburban railways. The general practice appears to be to strive after the maximum possible acceleration. The driver when he starts out of a station puts his starting lever right over; he takes as much steam as he can, and starts off as fast as he can, and he keeps his valve full open until a given point in the journey—which he finds by experience—when he shuts his steam right off. The result is that, if you go on any of the railways about London where they have stations at from half a mile to a mile apart, you will find that the train runs faster and faster up to a certain point, during which time the steam is on, and then runs at a falling speed for the rest of the journey. The driver has learned just where to shut off his power; and on many railways we know that a study of that kind of thing has been encouraged by giving the drivers premiums on the amount of coal they can save from a given allowance. Probably there is something in this which is arrived at in a way quite as sound as from any of the theoretical considerations in the paper.

The President. The PRESIDENT: Before I ask Professor Carus-Wilson to reply to the criticisms made on his paper, I wish to join with Mr. Mordey in expressing pleasure at having Professor Carus-Wilson with us. One of the last duties we performed at the Council table to-night was the acceptance of Professor Wilson's resignation as our Honorary Secretary at Montreal. That was a loss to the Institution which we very much deplore; but in having his presence with us, as we have to-night (and I hope we may often have it), we shall find in the loss a gain to match. The subject of Professor Carus-Wilson's paper is an exceedingly interesting one, and it comes very opportunely, when we are more at the beginning of the construction of electric railways than our friends across the Atlantic, whose experience I hope we can to some extent utilise. The subject of the paper is out of the line of my experience, and therefore I will not make any observations of a technical kind on it.

But I am very much interested in it. I look at it from a personal point of view in connection with the Central London Railway, one of the stations of which will come very near my house. When I remember that there are to be 11 stations between me and the Mansion House, it becomes a matter of deep interest to me the length of time it will take to start and stop the trains between these stations. I therefore fully realise the great importance of acceleration in starting and retardation in stopping the trains on an electric railway. These *factors* enter deeply into the question of the time it will take to get from my house to the City. Bearing in mind the remark of Professor Smith as to the necessity for minimising stresses in connection with quick time service, and that rapid retardation is of equal importance to rapid acceleration, the question occurs to me whether the stresses arising in connection with the stoppage of a train could not be diminished by the use, in an opposite way, of the agency employed in the propulsion of the train, instead of depending entirely upon the old-fashioned mechanical brake. I have very vividly before my mind an experiment I saw made by Mr. Mordey at Birmingham some years ago, where a powerful motor was instantly stopped by the making of an electric contact. The President.

Professor SMITH: That principle is being used on the electric tramways in Rome, I think, to a considerable extent.

The PRESIDENT: Retardation by means of an electro-magnetic brake seems to me to be a means by which a reduction of the stresses pointed out by Professor Smith as necessary, might be accomplished.

I now ask Professor Carus-Wilson to reply to the criticisms which have been made; and I suggest that, if further discussion is desired, it can take place through the medium of the *Journal.*

Professor J. PERRY: Before Professor Carus-Wilson replies, may I ask if he will supplement his paper with a little more information of a practical kind? I have searched through hundreds of pages of all sorts of books and papers on traction, filled with all sorts of useless figures, and I never can get the sort of information one wants for making practical calculations. I am sure that Professor Carus-Wilson can give us such information. I want Prof. Perry.

Prof. Perry. particularly to know the watt-hours per ton-mile, not on a level road, and not fancy figures, but the average values day by day, or year by year, for existing tramways and railways, either as delivered to the cars or as leaving the station. On a railway in the States there is always less resistance than on English railways, mainly because of the better construction of the waggons. I have figures for the City of London Railway and the Montreal tramways, and know of no others yet published. In the one case the watt-hours per ton-mile are about eight or nine times as great as in the other.

Prof. Ayrton. Professor W. E. AYRTON: With reference to the President's remark, the main reason why the plan which he suggested is not employed—viz., that of converting the motor into a generator, and causing it to act as a brake—is a purely practical one, namely, that by so doing you would remove the period for cooling of the motor, which is regarded as of considerable importance in ordinary electric tramway working. The runs being short, it is permissible for the motors to be somewhat warmed during the period of acceleration, as well as during the period of running at full speed, provided that periods of cooling be interposed during the retardation. But if the motor were employed to act as a brake itself, or if it were used to generate a current to operate an electric brake, instead of the ordinary mechanical brake being put on by hand, then periods of cooling would not exist, and the motor would become too hot. For this reason electric braking, although excellent in itself, is not more used.

Mr. Mordey. Mr. W. M. MORDEY: The experiment at the British Association meeting at Birmingham, to which the President has referred—which was made a good many years ago—was not on the lines Professor Smith appears to have in his mind. What I did then was to show that when the current was switched off the motor the discharge of the field magnet was capable not only of stopping the motor very suddenly, but would even make it run backwards. The field magnet acted as a generator to its own armature, and reversed the direction of rotation.

Prof. Carus-Wilson. Professor C. A. CARUS-WILSON, in reply, said: I shall

endeavour, as briefly as possible, to reply to the remarks which have been made. Taking the question which you have raised, Sir, as to stopping the motion of a car or train by the action of the current in the motors: This has already been worked out in considerable detail. Of course a series-wound motor cannot be made to generate a current, except on its own residual magnetism. Therefore the tension it will generate will be small, and the magnets must be disconnected entirely from the armature. Mr. Sperry found that he got better results by utilising the current thus produced to actuate a magnetic brake. Prof. Carus-Wilson.

The PRESIDENT: That was the form of brake which I had in my mind.

Professor CARUS-WILSON: With reference to Professor Perry's suggestion, I did not consider that it came within the scope of this paper to give in detail the particulars which I have on the actual performance of motors; but I should be very pleased to provide Professor Perry with some data, which I hope may satisfy him.

Mr. Grove spoke on the question of the assumptions which were made in the paper. Of course there are assumptions. We must always make assumptions of some kind, but in this paper I have confined myself to such assumptions as necessarily occur in connection with the design of motors. For instance, the question was raised as to whether it was permissible to assume a certain drop when running at full speed. This is, of course, simply another way of specifying the efficiency. I should point out that in this paper I have represented only one aspect of the problem, namely, that of carrying a train of given weight from rest through a given distance in a given time, and the question of running for long distances when the time occupied in starting is relatively short has not been considered. As to the question of the way in which an engine-driver handles the throttle valve of his locomotive, I wish that the steam engineers would provide us with accurate data from which we could ascertain exactly what is taking place when a steam locomotive is being brought up to speed. Information on this point is extremely difficult to obtain. The action of an average engine-driver is very much like that of

Prof. Carus-Wilson. the average motorman: he trusts to instinct to find out what is the best method to adopt. We should not, however, think that there is nothing to be learned from theory and calculations based on actual data.

Mr. GROVE: Might I be allowed to explain that I said the steam engine locomotive driver does so and so? I did not mean to say that by watching his hand you could do that. I am speaking from the actual dynamometer diagrams, which show a rising acceleration.

Professor CARUS-WILSON: Mr. Scott asked whether a series-parallel controller is of value when the distance to be travelled is great. I may say that just in proportion as the distance is great the use of a series-parallel controller becomes of less importance. The effect of the introduction of roller bearings would be to make the current-curve droop and quite alter its shape. With reference to Professor Ayrton's remarks, I am extremely obliged to him for the kind suggestions he has made. With regard to the estimate of 9 lbs. per ton, on page 584, being too low, this data was obtained from the actual performance of the motors on the Baltimore Railway, and was deduced by the engineers from a calculation of the hauling power of the motors when running at full speed.

Professor PERRY: You make an assumption as to efficiency, I suppose?

Professor CARUS-WILSON: No. The current was measured, and, the induction curve of the motors being known, the horizontal pull of the motors for that current determined. The motors were gearless, and the resistance to motion included the friction of the bearings and the hysteresis losses in the armature.

Professor PERRY: But they do not actually measure the pull?

Professor CARUS-WILSON: They measured the pull quite as accurately as it could be measured by a dynamometer attached to the draw-bar.

Professor PERRY: That is what I mean.

Professor CARUS-WILSON: Professor Ayrton thought I had neglected to notice that the alteration of the diameter of the driving wheel would affect the tractive effort. The equation at the top of page 583 shows that the horizontal pull per

Prof. Carus-Wilson.

ampere is inversely as the diameter of the driving wheel. As to the question of the numerical accuracy of the figures, it may be well to state that in working out the results I have used throughout a 10-inch slide rule, and I have not attempted any greater accuracy than can be obtained with that instrument. The point in Professor Ayrton's criticism to which I should like to pay special attention is his suggestion that it would be much better to call the "induction-factor" the "E.M.F. at one "revolution per second." Now I submit that the quantity which is here expressed by the term "induction-factor" does not in any sense whatever depend upon the speed. If Professor Ayrton's suggestion were adopted, we might ask, What becomes of this quantity when the motor is at rest? The answer would have to be that it is indeterminate, the value being $\frac{0}{0}$. Such a statement would convey no information whatever. The induction-factor, on the other hand, is absolutely independent of the speed, and equation (2) shows that by its use we can determine the torque for any given current in the armature, whether the motor is at rest or in motion. Professor Smith is correct in saying that I assume a constant train resistance. I have not, however, ventured to take up the time of the meeting in order to show that the graphic method I have employed in drawing the acceleration curves is applicable in cases where the train resistance is variable. I come now to the remarks made by Mr. A. M. Taylor, in which he quoted some experiments made by Mr. Short. I know that Mr. Short has done valuable work in railway motor design, but on this question of constant acceleration I am afraid that I have to disagree with him. Mr. Short wrote a paper a little time ago, which was published in the *Street Railway Journal*, on the advantages of the constant current acceleration controller. His object was to show that, if the acceleration could be kept constant right up to the maximum speed, very much better results were obtained, both as regards time and economy, than if the acceleration were variable. I examined the paper with interest, and I noted that, while Mr. Short's method reduced the time by a few seconds, the

Prof. Carus-Wilson. consumption of energy, according to his own diagrams, was increased considerably.

[*In reply to the correspondence.*]—In criticising the conclusions deduced from Fig. 5, Mr. A. M. Taylor writes that he thinks the case is impracticable on account of the high initial acceleration involved. Mr. Taylor bases his remarks on the assumption that only one-third of the total weight is on the drivers. It is, however, expressly stated in the paper that the case considered is that of a single car where *the whole* weight is on the drivers, so that on Mr. Taylor's own assumptions there would be sufficient adhesion to get an initial acceleration of 4·6 f.p s. per second, the value taken in the paper being only 3·1.

If Mr. Taylor wishes to see how the theory works out for a case where the whole weight is not on the drivers, he should take the example of the Chicago Elevated Railway throughout, and not take the figures obtained for the single car and apply them to the Chicago Railway. In the paper the acceleration chosen for the Chicago Railway is 1·27 f.p.s. per second, and is actually less than that obtained in practice under precisely similar conditions of weight on drivers. The main point of interest, that Mr Taylor seems to have overlooked, is that, while the acceleration curve obtained from the theory may differ but slightly from that obtained in practice, the expenditure of energy may be greatly reduced by properly designing the equipment.

At the time of writing, I have not had the opportunity of seeing the figures to which Mr. Taylor refers, so that I am unable to criticise his remarks relating to them.

Professor R. H. Smith, in his communication, says that during the period in which the starting rheostat is being taken out "the "current in armature and magnets varies, so that M varies very "largely, and C_a also varies, dying away to zero." This is not the case. During the period referred to, the current through the motor remains constant; M and C_a therefore remain constant. There will, of course, be irregularities if the controller is not carefully handled, but the smooth acceleration curves that have

been obtained in practice entirely warrant the assumption made in the paper. The "inconsistency" discovered by Professor Smith is due to his assumption that C_a cannot be kept constant. Prof. Carus-Wilson.

The problem discussed by Professor Smith as to how to get the work done as quickly as possible with minimum stresses in the mechanism is an interesting one, but, as it is foreign to the object of the paper, which was to find the design for covering a given distance in a given time with minimum expenditure of energy, I shall not here add anything to Professor Smith's remarks.

Professor Smith objects to my conclusion that "the same rule "for best proportion between accelerating distance and total dis- "tance results when the problem is to minimise the accelerating "current for a given time and distance, as when it is aimed "to minimise the time for a given accelerating current and "distance."

This conclusion seemed to me so obvious that it did not appear necessary to prove it mathematically. This is, however, easily done, but we get $\beta = \frac{3}{2} \cdot \frac{D}{k_2 t}$, not $\frac{1}{2} \cdot \frac{D}{k_2 t}$, as stated by Professor Smith. The latter value, in which the 3 seems to have been dropped out, gives, of course, an impossible result; the former value, which is correct, is the same as my equation (15).

The PRESIDENT: I propose, with very great pleasure, that we give Professor Carus-Wilson a vote of thanks for his very excellent paper on this very important subject.

Carried by acclamation.

The PRESIDENT announced that the scrutineers reported the following candidates to have been duly elected:—

Member:

Daniel Frederic Cooksey.

Associates:

Joseph Metcalf Bradley.
Albert Edward Briscoe.
Frederick William Carter.
John Clement Edgcumbe Chevallier.
Arthur H. French.
Tom E. Gambrell.
Walter J. Leeming.
E. S. Lowes.
George William Maddison.
Osborne Pearston.
Edward Goodson Phillips.
Edgar Poole.
Arthur Henry Shaw.
Oliver Tibbits.

Student:

Stanley S. Booty.

ORIGINAL COMMUNICATION.

FEEDER MACHINES.

By REGINALD WOOD, Associate.

The following short account attempts to show how Lord Kelvin's law as to the section of a feeder may be satisfied in the case of many feeders of widely different lengths supplied from one set of omnibus bars.

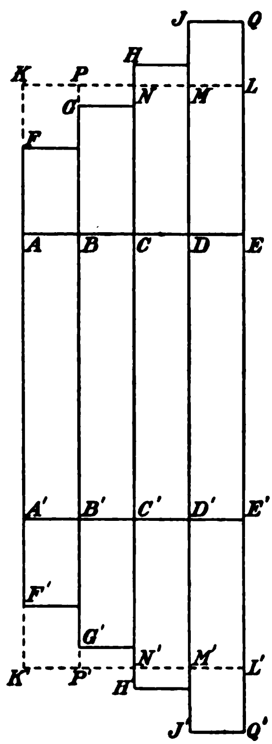

FIG. 1.

This increases the range of a supply at any given pressure, and results in economical working.

Construct a diagram (Fig. 1) for all feeders as follows:—Let vertical ordinates represent volts; horizontal ordinates,

amperes. The diagram is for full load, and the method of using it for any load on any feeders will be apparent.

Let A B be current, A F be volts drop for positive pole of feeder 1;

„ B C	„ B G	„	„	2;
„ C D	„ C H	„	„	3;
„ D E	„ D J	„	„	4.

Draw K L so that K E = F B + G C + H D + J E = power lost in positive pole of feeders.

The "dashed" letters relate similarly to the negative pole.

A E = total current.

A A′ = consumers' pressure.

It is well known that if the main generators generate A E′, and series generators driven at constant speed generate FB, GC, HD, and JE respectively (and similarly for the negative pole), the fall of pressure over each feeder is automatically counteracted at all loads, and each feeder may be of sectional area suitable for the shape and size of its load curve.

The diagram relates to engine-driven series generators. For motor-driven generators the energy K E + K′ E′ should be shown also before conversion in the form of a block similar to A B′.

If series generators be used driven by either engines or motors, the advantage of their employment is balanced by the expense.

Fortunately, by a simple device, the purpose effected by the series generators driven at constant speed by motors, and placed in series with the feeders, can be effected economically, and the area economically supplied at any given pressure can be thereby increased.

On each positive series-generator magnet place a coil excited by AK, and producing in each armature a pressure AK opposing the feeder current. Similarly with regard to A′K′ and the negative pole.

Let the main-generator pressure be now K K′. Then it will be apparent that the system acts exactly as before. The main-generator pressure has been increased by A K + A′ K′, and the pressure applied to the feeders has been increased, but it has been decreased again by a like amount by the action of the

added coil. The motors originally driving the series generators have been dispensed with, and the feeder machines in the short feeders have become motors and drive the feeder machines in the long feeders, which machines continue to act as generators. The excitation of the feeder machines is that due to the difference of the two coils, the machine acting as a generator or a motor according as the influence of the series or of the other coil predominates.

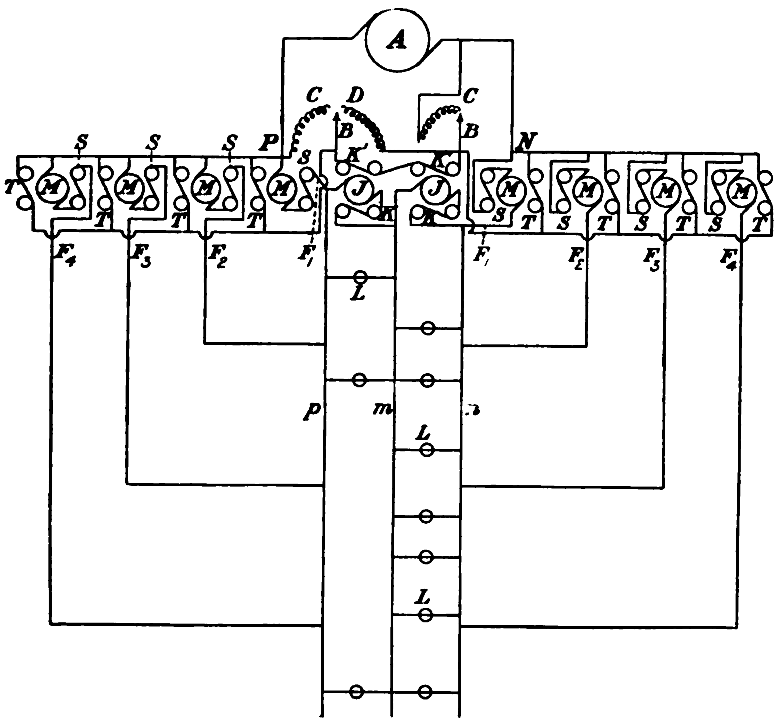

FIG 2.

The purpose of the series generators driven at constant speed has been effected, but the power of the positive feeder machines is (F P + G N) motors + (H M + J L) generators, and the motors are equal in power to, and drive, the generators. Similarly for the other pole.

The total power of the feeder machines is now 4 (H M + J L). against 4 (A Q) for motor-driven series generators; the ratio

being one-seventh theoretically, but between one-seventh and one-quarter practically.

A small fraction of the motors originally driving the series generators is retained, to allow for temporary disagreement between the actual and proper value of the main generator pressure, and in the three- or five-wire system these motors act as balancing machines.

It is advisable that all means should be taken to minimise both the ampere-turns required on the magnets, and also the friction.

At times of light load it would not be necessary to run the feeder machines.

The same feeder machines can be adapted for use with accumulators at the feeding points—a system which is undoubtedly the best when the ratio of the cost of generating plant and feeders to the cost of storing plant is sufficiently high.

The author believes that investigation of the application of this system will repay the labour bestowed upon it.

Fig. 2 represents this method applied to a three-wire system of distributing mains.

A is the main generator.
P ,, positive omnibus bar.
N ,, negative ,,
F_1, F_2, F_3, F_4, are successive pairs of feeders.
M are feeder machine armatures.
S ,, ,, series coils.
T ,, ,, separate magnet coils.
J are balancing machine armatures.
K ,, ,, series coils.
K′ ,, ,, separate magnet coils.
p, *m*, *n*, positive, middle, and negative wires, respectively, of three-wire distributing mains.
B are switches coupled so as to revolve together.
C C are adjustable resistances to shunt T.
D ,, ,, ,, K′.
L are lamps or other consumers of energy.

By regulation of the switches, B, consumers' pressure (A A′ in Fig. 1) is maintained over the coils K′, and the pressure A K

(Fig. 1) is maintained over T. The regulation of B may be automatic. MJ are mechanically coupled so as to revolve together, or are under equivalent mutual control. The M armatures of one pair of feeders may be wound on one core.

Systems of double-trolley or three-wire traction could be connected direct to the mains at any point. There appears to be no valid objection to the double trolley, and the advantages should prove very great.

The system was designed to enable the demand for electric energy for all purposes over a large area to be met economically by one set of main generators, and the subsequent history of electric supply and demand has, in the author's opinion, completely justified the labour involved in the design. Even uniformity of consumers' pressure throughout the country may yet be realised.

COMMUNICATION.

ON ELECTRIC WELDING OF TRAMWAY RAILS.

By REGINALD J. WALLIS-JONES, Member.

Mr. Parshall, in his paper on "Earth Returns for Electric "Tramways," read before the Institution April 28th, 1898, stated (this volume, p. 443) that some unexpected results of the welding process by the Thomson system had made themselves evident in the course of time. No electric welding of tramway rails having been carried out in England up to the present moment, the writer was not able, from his own experience, to reply to these criticisms. But he sent a copy of the paper to Mr. A. J. Moxham, the president of the Johnson Company, Pennsylvania (which holds the exclusive right of welding track rails in the United States by means of the Thomson process), and has since received a reply, which was too late for publication with the discussion on the paper, but is incorporated in the following remarks.

First, in regard to the electrical conductivity of the welded section being less than that of a solid rail, this is necessarily so, inasmuch as the whole sectional area of the rail is not welded up. Nevertheless, Mr. Moxham states there is no bond in the market to-day, in practical use, that gives as much conductivity as the amount of welding that is required for the purposes of the welded joint; and, so far as conductivity is concerned, the need of welding the whole area has never been recognised. Indeed, the company had not found it necessary to experiment with the conductivity of the joint, as, until now, they did not know of it ever being called into question.

In reference to the second point, the experience of the Johnson Company tends to show that the rail does not become softened by the welding operation; and, indeed, instead of wearing unevenly, the universal testimony goes to show that this form of welding leaves an unusually smooth joint.

Thirdly, as to the reported decrease in the power of the rail to withstand shock, owing to the metal taking a very high temper at the weld through the sudden increase and decrease in temperature, the diminished power of resisting shock was due, not to the hardening of the steel, but to an absolute deterioration of the metal caused by the high temperature in the absence of

any action taken to overcome the defect. It was this deterioration which caused the company to stop at this point in their efforts of development; but they have now overcome the defect.

Finally, it may be of interest to summarise and compare certain tests on joints made by electric welding, with those on cast-iron joints made in accordance with usual practice, as reported by Moxham in the appendix to an article on "The "History of Electric Welding" (*The Iron Age*, Dec. 30, 1897, p. 17).

Out of 72 electrically welded rail joints—

	Lbs.
1 broke under a tensile load of ...	216,000
2 " " ...	220,000 – 230,000
15 " " ...	230,000 – 250,000
16 " " ...	250,000 – 270,000
19 " " ...	270,000 – 290,000
12 " " ...	290,000 – 310,000
5 " " ...	310,000 – 330,000
1 " " ...	343,000
1 " " ...	353,000
Average tensile strength of the 72 joints ...	273,000

Out of 15 cast-iron joints with punched rails 6 inches in height, the length of casting being 14 inches—

	Lbs.
1 broke under a tensile load of ...	146,000
4 " " ...	170,000 – 190,000
4 " " ...	190,000 – 200,000
4 " " ...	200.000 – 220,000
2 " " ...	230,000 – 240,000
Average tensile strength of the 15 joints ...	196,700

And out of eight cast-iron joints with punched rails 9 inches in height, the length of casting ranging from 12 to 20 inches—

	Lbs.
1 broke under a tensile load of ...	117,000
2 " " ...	170,000 – 180,000
2 " " ...	180,000 – 190,000
1 " " ...	231,000
1 " " ...	268,700
Average tensile strength of the eight joints ...	191,300

In the latter tests the fracture occurred in most instances through the rail in the casting, but in some cases through the casting at the joint.

NOTICE.

1. The Institution's Library is open to members of all Scientific Bodies, and (on application to the Secretary) to the Public generally.

2. The Library is open (except from the 14th August to the 16th September) daily between the hours of 11.0 a.m. and 8.0 p.m., except on Thursdays, and on Saturdays, when it closes at 2.0 p.m.

JOURNAL

OF THE

Institution of Electrical Engineers.

Founded 1871. *Incorporated* 1883.

VOL. XXVII. 1898. No. 137.

The Three Hundred and Nineteenth Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, Great George Street, Westminster, on Thursday evening, November 10th, 1898—Mr. J. W. SWAN, F.R.S., President, in the Chair.

The minutes of the Ordinary General Meeting held on May 26th, 1898, were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council:—

From the class of Associates to that of Members—

Harry W. Appleby.	William Peto.
Charles Alfred Baker.	James Clifton Robinson.
Horace L. P. Boot.	Franz Roseneder.
John Grant.	Louis J. Steele.
George Johnson.	Arthur D. Stevenson.
Thomas G. M. Ladds.	John E. Stewart.
Charles Frederick Parkinson.	Lewis Buckley Stillwell.

Charles Lucas Turner.

From the class of Students to that of Associates—

C. L. Lichtenberg.	William Denton Perrott.

Donations to the Library were announced as having been received since the last meeting from Mr. Charles Aburrow; the Astronomer Royal; the Editor of "The Gas, Electric Light, and "Water Works Directory;" the Liverpool and London and Globe Insurance Co.; Mr. J. McDonnell; Patent Office Library; Messrs. Swan, Sonnenschein, & Co.; Mr. D. R. Walker; and from Mr. H. L. P. Boot, Mr. C. Bright, Mr. E. Garcke, Mr. R. Kaye Gray, Mr. C. H. Wordingham, and Mr. J. Elton Young, Members; to whom the thanks of the meeting were duly accorded.

The President.

The PRESIDENT: Since our last meeting, we have suffered the misfortune of losing two of our most prominent members—Latimer Clark and John Hopkinson—both Past-Presidents of the Institution, and both of world-wide reputation and eminence as electrical engineers. Partly following precedent, and partly to meet exceptional circumstances, the Council was in each case called together, and at each meeting a resolution was passed expressing their feeling of the great loss occasioned by the death of these distinguished men, and sympathy with their families.

I am sure it will be your wish, and that you will feel it to be a privilege, to be afforded the opportunity of uniting in these expressions of appreciation of regret and of condolence.

Mr. Latimer Clark died on the 30th of October, at the ripe age of 76 years.

During more than 40 years he was connected with electrical engineering, in that department in which it first took organised shape—telegraphy. Many of the appliances incidental to telegraphy, and that have become almost universal, are the inventions of Latimer Clark; several of these are inseparably linked with his name. To the advancement of pure science he made many valuable contributions, especially in the working out of the idea of units of electrical measurement, but his labours and achievements are widely spread over the fields of invention, research, and literature.

There can only be one opinion and one feeling among us with regard to Latimer Clark. As an electrical engineer whose name and work are inwrought in the fabric of electrical industry

and electrical science, as one of the most able of the band of pioneers to whom we owe the realisation and development of ocean telegraphy, as a Past-President of the Institution of Electrical Engineers and one of its founders, Latimer Clark holds a place of the highest honour in the records of the Institution and in the memory of its members. The President.

The tragic circumstances of the death of Dr. Hopkinson, on the 27th August last, are but too vividly imprinted in our memory. By that calamitous event, which quenched in sudden darkness one of the brightest lights of the world, we have lost one of the greatest of our colleagues. Dr. Hopkinson died in his prime, but in the narrow space of his 49 years was crowded work of the most productive and enduring kind.

Elected a member of our Institution November 10th, 1881, he was one of the Council from 1882 to 1885, a Vice-President from 1886 to 1889, and President, first in 1890, and again in 1896.

In addition to his Inaugural Addresses he gave only two papers to the Institution—the first, in 1884, on "The Theory of "Alternating-Current Machines connected to the same Circuit;" and the other, in which he was associated with Professor Wilson, in 1895, on "The Propagation of Magnetism in Iron." His first address dealt with the subject of magnetism, and the second was devoted to a deduction of the facts of electrostatics from the observations of current electricity.

His earlier papers related mainly to the properties of glass and other dielectrics, and to lighthouse subjects. Then, his attention being devoted to the electrical illumination of lighthouses, he began his career as an electrical engineer, and read two papers on electric lighting (in 1879 and 1880) before the Institution of Mechanical Engineers. These papers had a marked effect on the earlier developments of electric lighting. Simultaneously with this work, his investigations into the working of the dynamo led to the study of the laws of magnetism, and produced a series of papers on that subject, some of which were of the highest interest and importance, notably his paper to the Royal Society on the magnetisation of iron, in 1885, and his first Presidential Address to the Institution of Electrical

The President. Engineers. His study of nickel-iron alloys in relation to magnetism, and of the connection between the recalescence of iron and the magnetic behaviour of the metal at high temperatures, formed the subject of several other papers.

But in practical results the most important paper that he ever published was that which, in collaboration with Dr. Edward Hopkinson, he communicated to the Royal Society in 1886, developing a complete magnetic-circuit theory of the dynamo, and, with the aid of the graphic diagrams—which he introduced, and which have since received the designation *characteristic curves*—establishing those principles that led to the theory and design of the dynamo being placed on a sound scientific basis, and bore fruit in the immediate increase in the efficiency of the dynamo.

His name is, as you well know, identified with the origin of electric current distribution by means of the three-wire system, and with many other inventions of the greatest utility.

It is to the patriotic instinct and initiative of Dr. John Hopkinson that we owe the formation of the corps of Electrical Engineer Volunteers. This corps is in itself a monument to Dr. Hopkinson's powers of prevision and of organisation, as well as to his intense patriotism.

I trust, gentlemen, that we may all feel that it is incumbent upon us, as colleagues of his, and as participators in the benefits of his discoveries, and in response to our own patriotic sentiments, to accept as a sacred trust the obligation to support and cherish this child of his, and that it may always be associated in your minds with his memory.

And what shall I say of his character more than that it was in perfect symmetry with his work—simple, direct, unpretentious, complete? Clear-sighted, honest, and self-possessed, everyone trusted John Hopkinson—trusted him implicitly, without any misgiving. No man ever carried so great a weight of learning more lightly, with more juvenile buoyancy. Like the athlete he was, he did the heaviest work without any sign of effort.

We mourn that we shall see the faces of these dear old comrades no more, and that the precious work they did cannot be

added to; but let us rejoice that neither can the splendid heritage they have left us ever be taken away. What is excellent continues. The product of creative minds like these is an eternal possession. The vitality of truth sustains the fabric of their work, and maintains for them an imperishable monument, and for us a stronghold and coign of vantage for further conquests. The President.

The Secretary will read the resolutions.

The SECRETARY: Resolution passed at a Special Council Meeting on August 31st, on the occasion of the death of Dr. John Hopkinson—

> "That the Council of the Institution of Electrical "Engineers do hereby place on record this expression of "their sincere sorrow and deep regret for the great and "irreparable loss sustained by the Institution through the "untimely and calamitous death of Dr. John Hopkinson, "F.R.S., Past-President of the Institution of Electrical "Engineers, Major commanding the Corps of Electrical "Engineers, Royal Engineers (Volunteers), and Professor of "Electrical Engineering in King's College, London."

Resolution passed at a Special Council Meeting on November 2nd, on the occasion of the death of Mr. Latimer Clark—

> "That the Council desire to place on record their deep "sense of the loss that the Institution has sustained by the "death of their very highly esteemed Past-President and "colleague, Mr. Latimer Clark, F.R.S. Mr. Clark was not "only one of those closely connected with the origin of "the Institution, but was also one of the pioneers in many "branches of electrical industry. The Council desire also "to express their sincere sympathy with Mrs. Clark and the "other members of their family in their bereavement."

The PRESIDENT: I would like to feel that there is general concurrence on your part with the action taken by the members of the Council in passing those resolutions, and I take as an expression of that concurrence your maintenance for a moment of perfect silence.

Mr. H. A. TAYLOR: My only qualification for addressing you on this point is my long association of over 30 years with Mr. Taylor.

Mr. Taylor. Latimer Clark in submarine engineering. My remarks will be very brief, and will not delay the business of the evening more than a few minutes. I shall not attempt to give you any sketch of his career. That has appeared more or less fully in the technical Press and in the daily journals. Indeed, I am not qualified to do so, for Latimer Clark was one of the least egotistical of men, and made such sparing use of the "capital I" that he rarely spoke of any of his numerous interests and occupations except to those immediately concerned in them.

Of course, in cable work his name was a household word, but to the majority of those present it is probably best known in connection with his standard of electro-motive force, or the "Clark "cell."

He was working on this subject when I became his pupil, and I was at once told off to assist in the experiments.

His idea at that time was that the Daniell element might be so modified as to fulfil the condition of constant electro-motive force; and, though this result was not attained, his conviction remained unshaken that it would be possible to find some voltaic combination with the requisite properties.

His experiments were necessarily interrupted from time to time by professional engagements calling him abroad, but they were renewed whenever the opportunity presented itself, and four years later, in 1871, his faith and perseverance were rewarded by the discovery of the mercury cell.

The reward was, of course, scientific only, as he gave freely the result of his large expenditure of time and money.

One of these interruptions was near to becoming a total interruption. This was in connection with a cable to be laid by Mr. Clark in the Persian Gulf for the Indian Government. There were two bad shipwrecks on this expedition, the second occurring to a P. & O. steamer on which Mr. Clark was a passenger. On the voyage to India the "Carnatic" ran on to a coral reef, and, sliding off, sunk in deep water. Mr. Clark was severely injured, and drifted on to the reef in a state of insensibility. Some 30 lives were lost, and I had a narrow escape.

The survivors landed on a desert island, and, by rare good

fortune, were rescued by a homeward bound P. & O. steamer and taken back to Suez. Mr. Taylor.

Mr. Clark's injuries, which consisted of a broken collar-bone and dislocated shoulder, were patched up more or less skilfully by the ship's surgeon; and, though suffering a good deal, he pluckily determined to make another start for the Persian Gulf to carry out personally the duties he had undertaken.

On his return to England the experiments were continued, and eventually conducted to a successful issue.

It is not necessary to remind this meeting of the part the Clark cell has played in electrical measurements of late years, but at the time it was produced I think that, with few exceptions, Latimer Clark alone saw the great importance of a material standard of electro-motive force to give, in connection with the ohm, a practical means of measuring current.

Chief of the exceptions referred to was, of course, Lord Kelvin, by whom Mr. Clark's paper on the subject was presented to the Royal Society.

I cannot conclude without expressing my admiration for Mr. Clark's wide range of information, strong common sense, and equable disposition. During a partnership of more than a quarter of a century, I can declare that an unpleasant word never passed between us, and that I never heard him say an unkind word of any living creature.

The PRESIDENT: One of the incidents of the death of Mr. Latimer Clark is the necessity of electing in his place a Trustee of the Institution. The Council have had that matter under their consideration, and have elected—I am glad to say with his consent—Professor G. Carey Foster. I have also to announce that Mr. Hartley Gisborne has been appointed by the Council Local Honorary Secretary and Treasurer for Canada. The President

ROTATORY CONVERTERS.

By Professor SILVANUS P. THOMPSON, D.Sc., F.R.S., Vice-President.

So much interest is concentrated at the present time upon the transmission and distribution of electric energy for motive and locomotive power, as well as for lighting, that attention may Prof Thompson.

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profitably be directed to a class of machines which hitherto have been little discussed, namely, rotatory converters.

In general a rotatory converter may be described as a machine having something in common both with a dynamo and with a motor so far as its structure goes, and of which the function performed by its rotation is to transform the electric energy which is imparted to it in one form into an output of electric energy of some other form. Of such converters there are many varieties, subserving many different uses. For example, one sort of rotatory converter will change continuous currents at one voltage into continuous currents at a different voltage. Another will change two-phase alternating current into three-phase alternating current. Another will change three-phase alternating current into continuous current. Yet another will change an alternating current of a certain frequency into an alternating current of a different frequency.

In the following table of the principal conversions which may thus be accomplished, C stands for continuous current, A_1 for alternate current of single phase, A_2 for two-phase, A_3 for three-phase.

1.—C to C at higher or lower voltage.
2.—A_1 to A_1 " " "
3.—A_2 to A_2 " " "
4.—A_3 to A_3 " " "
5.—C to A_1, or A_1 to C.
6.—C to A_2, or A_2 to C.
7.—C to A_3, or A_3 to C.
8.—A_1 to A_2, or A_2 to A_1.
9.—A_1 to A_3, or A_3 to A_1.
10.—A_2 to A_3, or A_3 to A_2.
11.—A_1 to A_1 of different phase.
12.—A_1 to A_1 of different frequency.

Any one of these conversions might be accomplished by selecting an appropriate motor to receive and be driven by the primary current, and coupling it to a generator to give out a secondary current of the species desired. It will be noted that of these conversions No. 2 is the ordinary alternate-current

transformation, and is better accomplished by ordinary stationary transformers; as also are Nos. 3 and 4. By a suitable combination of stationary transformers No. 10 can also be effected, whilst Nos. 8 and 9 may be more or less satisfactorily effected by the use of auxiliary choking coils and condensers. But all of them may, and all the others must, be effected by means of rotatory apparatus. From the above list there have been omitted several possible cases of conversion, as, for example, that of a current supplied at fixed voltage but of varying amperage, to a current of fixed amperage but delivered at varying voltage. Prof. Thompson.

It is not proposed here to deal, save purely incidentally, with the continuous-current transformer (No. 1 of the above table), which was brought before this Institution seven years ago by Major-General Webber (vol. xx., p. 63, 1891); nor with phase transformers (Nos. 8, 9, 10, and 11); nor with frequency transformers (No. 12); nor with any stationary kind of transformer. This paper will be directed to the machines devoted to the purpose of converting continuous currents into alternating currents of one, two, or three phases, or *vice versâ*. The importance which such machines have now assumed in the electrical industry arises from several causes. Long-distance transmission, with its corollary of the employment of high voltages, has demanded for this service alternating currents. The development of the electric tramway with continuous-current motors has called for methods of feeding at distant points. For this service rotatory converters are required. Also for charging accumulators in cases where the public supply is an alternating one, and for factory driving with three-phase motors in cases where the public supply is one with continuous currents, rotatory converters are wanted. All these cases might, of course, be met by the use of coupled machines, motor and generator. An early example of this method was afforded by the lighting station at Cassel, where the high-voltage alternating transmission was effected by the use of synchronous single-phase alternators, and the town distribution by continuous-current dynamos coupled to the shaft of the synchronous motors. Even now, in the case of a three-phase transmission and a continuous-current distribution,

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some engineers recommend the use of a group converter consisting of a non-synchronous three-phase motor coupled to a continuous-current dynamo. But in all such cases the efficiency of the group is necessarily lower than that of either of its component parts. If a motor having, say, a 90 per cent. efficiency is coupled to a dynamo, also of 90 per cent. efficiency, the efficiency of the group cannot possibly exceed 81 per cent.

Two other solutions to the general problem are possible. One is to wind the revolving armature with two sets of windings—one to receive the primary current and revolve as a motor, the other to generate the secondary current. As only one field magnet and one pair of bearings are required, there is an obvious economy of material, though no great saving in efficiency. Another solution—not, it is true, of universal application, but having the advantage of effecting a considerable increase of efficiency as well as an economy in material—is to wind the armature with but one set of windings, furnished at one end with a commutator, and at the other with appropriate contact rings, the same winding serving both to receive the incoming primary current and to generate the secondary current. It is this specialised type of machine which is the main subject of the present discussion, and which is called *par excellence* a rotatory converter.

At this point it may be convenient to drop the adjective "rotatory," and speak simply of *converters*, leaving the term "transformer" to denote the stationary apparatus.

Converters of a simple kind have been known for many years. Ever since 1885 there has been one at the Technical College, Finsbury, consisting of a bipolar Gramme machine with the ordinary commutator, with the addition of two insulated contact rings at the other end of the armature, these rings being connected to two points on the winding at opposite ends of a diameter. It therefore belongs to the species $C\,A_1$ (or No. 5 of Table I.). The addition of three contact rings connected to three symmetrical points on the winding would constitute a machine of the species $C\,A_3$ (or No. 7 of Table I.).

It may be convenient here to point out the relation between the number of contact rings (called for brevity slip-rings) that

are applied in any alternate-current generator, and the uses to which such generator may be put with respect to the phasal relations of the currents that may be drawn from it. For brevity these are enumerated in the following table. The figures in the second column relate to the angles between the points on the winding at which the slip-rings are connected for the simple case of a bipolar machine. Prof. Thompson.

Table II.

No. of Slip-Rings.	Angle.	Possible Service.
2	180°	Single-phase.
3	120°	3 single-phase in ternature = "three-phase."
4	90°	2 single-phase in quadrature = "two-phase," or 4 single-phase in successive quadrature = "four-phase."
5	72°	5 single-phase in quinature = "five-phase."
6	60°	3 single-phase in ternature = three-phase with separate leads, or 6 single-phase in successive sexature = "six-phase," or 2 three-phase in sexature.
7	$51\frac{3}{7}$°	7 single-phase in septature = "seven-phase."
8	45°	4 single-phase in successive octature = "four-phase" with separate leads, or 8 single-phase in successive octature = "eight-phase," or 2 two-phase in octature.
9	40°	3 three-phase in nonature, or 9 single-phase in successive nonature = "nine-phase."

The general principles of conversion from polyphase to continuous currents, or *vice versâ*, are well known. The relation between the respective voltages on the alternating-current side and the continuous-current side have long ago been investigated, and expressions for their values in the several instances that may arise have been given for those cases in which it is assumed (*a*) that the alternating currents are simple sine functions of the time, and (*b*) that the magnetic flux is distributed as a sine function in space with respect to the periphery of the armature. Calculations for the voltages of two-phase and three-phase converters were given by Professor Ayrton in the *Journal of the Institution of Electrical Engineers*, vol. xxii., p. 340, in a discussion on April 27th, 1893. For the purpose of studying such machines they may be considered either as used to convert

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continuous currents into alternating, or as converting alternating into continuous. The latter function is of more frequent occurrence in practice; the former is rather more easy to follow out in thought. Taking, then, the instance of the converters as used to change continuous currents into alternating, and applying Professor Ayrton's results to the several cases, we obtain the following numerical values. If we take the continuous currents as being supplied at a constant pressure of 100 volts, the voltmeter readings at the alternate-current side will be as follows:—

Number of Slip-Rings.	Angle between Connections to Rings.	Nature of Service Generated.	Voltage Ratio.	Voltage (virtual Volts).
2	180°	Single-phase ...	$\frac{1}{\sqrt{2}}$	70·71
3	120°	Three-phase ...	$\frac{1}{2}\frac{\sqrt{3}}{\sqrt{2}}$	61·23
4	90°	As two-phase ...	$\frac{1}{\sqrt{2}}$	70·71
4	90°	As four-phase ...	$\frac{1}{2}$	50·00
6	60°	As three-phase	$\frac{1}{2}\frac{\sqrt{3}}{\sqrt{2}}$	61·23
6	60°	As six-phase ...	$\frac{1}{2}\frac{1}{\sqrt{2}}$	35·35

A very complete discussion of the voltage relations, with formulæ applicable to the cases of open-coil windings as well as of closed-coil windings, was given by Herr R. M. Friese in the *Elektrotechnische Zeitschrift* of February 15th, 1894. Throughout the series of articles he assumes the sine values of the distribution of the magnetic flux around the periphery of the armature. More recently, Mr. Steinmetz has reconsidered the same problem in the same journal in articles which appeared on March 3rd and 10th, 1898. From these theoretical considerations it is easy to write down, not only the voltages in the several cases, but also to calculate the corresponding relative values of the working currents in the armature and in the line wires. Similar calculations, down to a certain point, have to be

made by every designer of polyphase motors. If we assume the above values for the voltages, and proceed to calculate the corresponding currents for an output of 10 kilowatts, we find the values of the currents generated to be as follows, the circuits being supposed non-inductive. Maximum as well as virtual values are given. Columns 4 and 5 refer to currents in the armatures, and columns 6 and 7 to currents in the lines. Star groupings are out of the question here, as they cannot be applied in armatures of converters. The only two-phase case that is possible is really a four-phase. Prof. Thompson.

WORKING CURRENTS IN ALTERNATING ARMATURES.

Number of Slip-Rings.	Angle between Connections to Rings.	Nature of Service.	ARMATURE CURRENT.		LINE CURRENT.	
			Virtual.	Maximum	Virtual.	Maximum
2	180°	Single-phase	70·7	100·0	141·4	200·0
3	120°	Three-phase	54·5	77·0	94·3	133·3
4	90°	Two-phase [4 wires]	50·0	70·7	70·7	100·0
6	60°	Six-phase	47·2	66·7	47·2	66·7

If the circuits are inductive, there will be a lag of phase in the currents, and wattless currents as well as working currents. For an equal output of power the numbers in columns 4 to 7 of the above table will need to be increased by dividing them by the cosine of the angle of lag.

Let us return to the problems presented by a converter, in which an armature, furnished with slip-rings at one end and a commutator at the other, is wound with but one set of windings, receiving current as a motor and delivering it as a generator. The question for consideration is how the current, in the act of being transformed from alternating to continuous, or *vice versâ*, flows through the windings of the armature. This is a matter that has not hitherto been considered in detail in any publication. Consider the general case of a revolving armature which is at the same time being traversed by continuous currents to drive it as motor, and by alternating currents which it is putting forth as generator. It is self-evident that the currents which it receives

Prof. Thompson.

as motor must be flowing, in general, against the electro-motive forces induced internally by its rotation, while the currents which it gives out as generator must be flowing with those electro-motive forces. Further, since at every instant (under steady conditions of operation) the value of the continuous current is (by hypothesis) unvarying, whilst the instantaneous value of the alternating current is continually changing, it is clear that at some instants the motor current must be in excess of the generator current, whilst at other instants the reverse must be the case. Whilst the average speed of rotation remains uniform it is certain that during each period, or revolution, there will at times be a positive acceleration, and at other times a negative acceleration. Further, it is clear that if there are any irreversible sources of loss of energy, such as friction, hysteresis, or eddy-

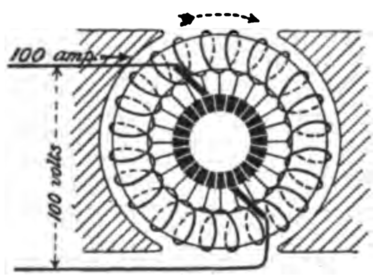

Fig. 1.

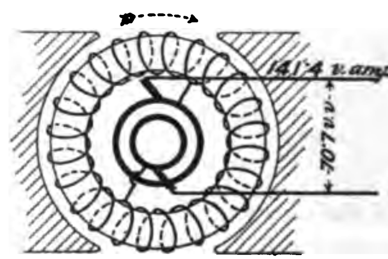

Fig. 2.

currents, the motor current must on the whole be greater than would otherwise be requisite, the power supplied at the motor side being greater than the output of power at the generator side, the difference being equal to the sum of the various items of power wasted in the machine.

To make this clear, as well as to exhibit the way in which the circulation of current in the windings is effected, it is well to take some concrete case. All cannot follow an analytical argument; and therefore, though the analytical treatment has many advantages, I have deliberately preferred, for the purposes of the present paper, to avoid formulæ (though they have been used in its preparation), and, instead, to exhibit the arguments numerically by taking specific cases that are readily followed.

Let us consider a 10-kilowatt bipolar ring armature, having at its periphery 96 conductors, connected symmetrically down to a 48-part commutator, running at 1,200 revolutions per minute, or 20 revolutions per second. There will be two turns of the winding between each commutator bar and the bar next adjacent. (Or the armature might be wound as a lap-wound drum, with two conductors to constitute each element of the winding.) That it may run as a 100-volt continuous-current machine the magnetic flux through the armature core must be 5,208,333 lines, or a little over 5 megalines. Fig. 1 will serve to represent diagrammatically this armature when receiving 100 amperes at 100 volts, and running as a motor. The flow of current in the armature winding will, of course, be 50 amperes in each half of the ring. Prof. Thompson.

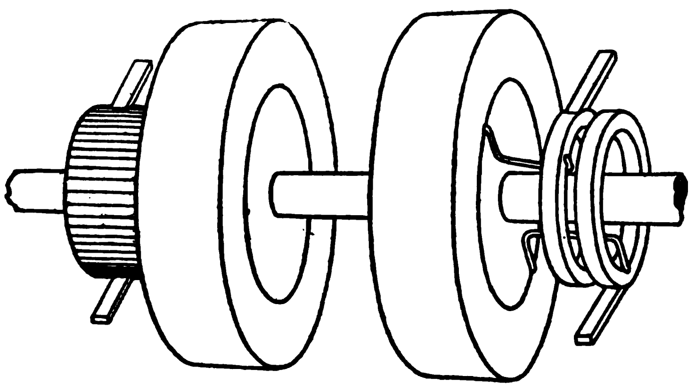

FIG. 3.

Now, suppose a precisely similar armature to be revolving in a precisely similar field, but let its windings be connected at two diametrically opposite points to two slip-rings on the axis. If driven by power it will generate an alternating current. As the maximum voltage between the points that are connected to the slip-rings will be 100 volts, and the virtual volts (as measured by a voltmeter) between the rings will be 70·7 ($= 100 \div \sqrt{2}$), if the power applied in turning this armature (Fig. 2) be 10 kilowatts, and if the circuit is non-inductive, the output in virtual amperes will be $10{,}000 \div 70{\cdot}7 = 141{\cdot}4$. If the resistances of each of the armatures is negligibly small, and if there are no frictional or

Prof. Thompson.

other losses, the power given out by the armature which serves as motor will just suffice to drive the armature which serves as generator. Accordingly, let us suppose them both mounted upon the same shaft, as in Fig. 3, and placed so that each lies in a similar and equal bipolar magnetic field. We have here the well-known combination of a *motor-dynamo.** In every actual case there are, of course, losses (*a*) by friction, hysteresis, and eddy-currents, (*b*) by heating of the resistances in the armatures. The former have to be paid for by an increase in the motor current. Suppose them in the present case to amount to 4 per cent. for each armature, then 108 amperes instead of 100 must flow in from the supply circuit. The heat losses manifest themselves electri-

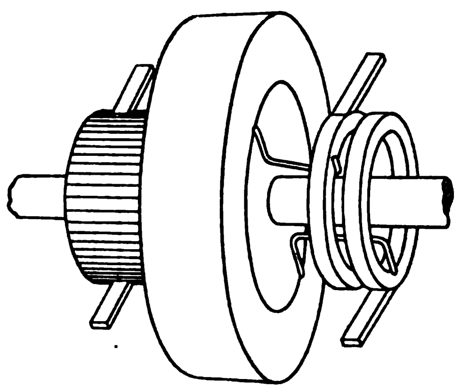

FIG. 4.

cally by a fall of potential at the terminals of the generator, and by a fall of speed in the motor if the primary voltage is not raised. Let the primary voltage be supposed to be raised the requisite small percentage to keep up the speed and to maintain the secondary voltage at 70·7 on the generator side: the output of the generator will then be 10 kilowatts, the input at the motor 10 kilowatts *plus* the number of watts required to make up all the various items of lost power. Though the armatures are of equal resistance, and are respectively receiving and giving out (approximately) equal

* The term *dynamotor* is not only philologically bad, but is a misdescription. When two machines, of whatever kind, are thus coupled together, it is always the motor which drives the dynamo, not the dynamo which drives the motor.

Prof. Thompson.

amounts of electric energy, the armature of the alternate-current side (whether used as generator or motor) will heat more than that of the continuous-current side; for it carries $\sqrt{2}$ times as large a current, and the ohmic heat will be proportional to the square of this, or twice that produced in the continuous-current armature by the 100 amperes of working current. In each armature the heat will be developed equally in all the separate coils around the ring. As the armatures are alike, and as the similarly placed windings in each are passing through identical magnetic fields, there is no reason why one winding should not answer for both purposes. Fig. 4 shows the case in which this change has been made. One armature only is used; it is connected at one end to the commutator, at the other to the two slip-rings, and the machine now becomes a simple *rotatory converter*. The total hysteresis and eddy-current losses will obviously be now one-half of their former amount. The total heating due to resistance will be also reduced, because now the single winding has to carry only the differences of the two currents—a point presently to be considered—and the ohmic losses will be less. The waste of power, in fact, is approximately halved. How far this economy of losses can be carried depends on the number of phases of the alternating current. But there is another consideration involved—the question of armature reactions and sparkless collection of current. In the combined pair of machines called a *motor-dynamo* the brushes on the continuous-current side must be set, exactly as in any continuous-current machine, with a lead, negative or positive, according as whether it is operating as motor or dynamo. In the *rotatory converter* no lead in either sense need be given to the brushes; for the armature reactions of the motor part being, in general, opposed by those in the dynamo part, they cancel one another to a large extent. This property, which is common to all those motor-generators in which there is used, whether with one winding or two, a common core in a common field, was pointed out by the author* in 1888, when giving the theory of continuous-current transformers.

***Philosophical Magazine*, August, 1888.

Prof. Thompson.

The relations between speed and field are peculiar. In the case of those grouped machines, or motor-dynamos (Fig. 3) in which each armature revolves in its own field, the conditions differ from those of the converter (Fig. 4), where there is only one field. If in either case the continuous-current side is the primary (*i.e.*, motor) side, the speed of revolution will depend on the field magnet, the weakening of which will increase the speed. The frequency of the secondary or alternating current will in that case also vary. But the ratio of the primary and secondary voltages will be independent of speed if the fields are alike, or if only one common field is used. The secondary voltage cannot be varied, while the primary voltage is kept constant, unless separate fields and separate windings are employed, as in Fig. 3.

If, on the other hand, the alternating-current side is used as primary, then the machine, whether motor-dynamo or converter, runs as a synchronous motor with a fixed speed. In that case the voltage ratio remains also nearly a constant, even though the excitation of the field is increased or diminished, owing to the peculiar phase relations which take place, as is known, in the currents of synchronous motors when under-excited or over-excited. This question, and the expediency of exciting in series or in shunt from the continuous-current side, or of running without excitation, has been discussed by Mr. E. J. Berg in the *American Electrician* of February, 1897.

Returning to the question how the current in the armature of the converter gets through the windings, it will be found that a simple means of answering it is afforded by the principle of the superposition of instantaneous values of currents. Still taking as a concrete case the 10-kilowatt converter of Fig. 3, we may calculate out the instantaneous values of the currents in its windings at different epochs in its period of rotation. If we do this for the motor currents and for the generator currents separately, and then superpose them, we shall obtain a number of instantaneous values for the combined armature as converter. This has been done in the series of drawings in Fig. 5. If the continuous current acts as motor taking 100 amperes, there will be 50 amperes flowing in each half of the winding

Prof. Thompson.

FLOW OF CURRENTS IN SINGLE-PHASE CONVERTER.

Continuous current: 100 amperes, at 100 volts.

Single-phase current: 141·4 virtual amperes, at 70·7 virtual volts.

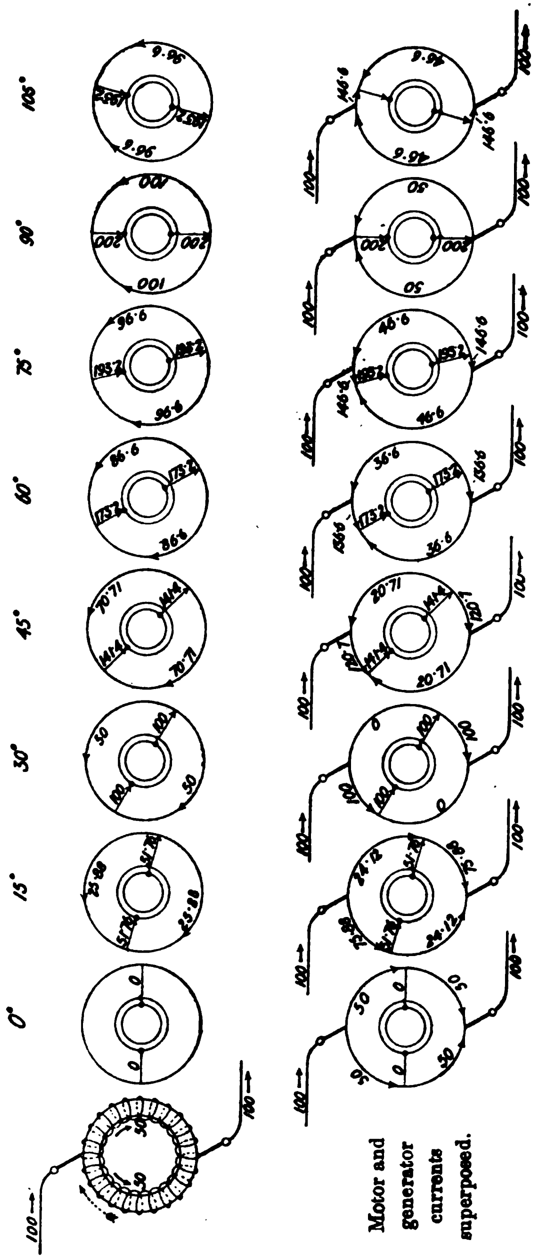

FIG. 5.

Prof. Thompson.

at every instant—*qua motor*—in a direction opposing the electro-motive force in the winding. This is shown in the first diagram in the upper row of Fig. 5. As single-phase generator the armature is working, as already described, with a virtual voltage of 70·7 volts, and an output (supposing the power-factor = 1) of 141·4 virtual amperes. The maximum current will be $141{\cdot}4 \times \sqrt{2} = 200$ amperes; and this will be attained at the instant when the two conductors which lead down from the winding to the slip-rings are turned to the position of 90° to the axis joining the middles of the pole-faces. The eight diagrams which follow in the top row of Fig. 5 exhibit the successive values of the currents flowing in the armature—*qua generator*—in positions differing successively by 15°. By supposing these eight diagrams successively superposed upon the motor diagram which precedes them, we obtain the eight diagrams of the lower row, which exhibit the currents actually flowing in the different parts at the particular times when the armature has the position shown.

Examination of these diagrams shows several interesting points. At the instant when the alternating current is reversing, and has zero value (at position 0°), the armature is operating simply as a motor with 50 amperes in each half of the winding, and is being accelerated. At the 90° position, when the alternating current is at its maximum (200 amperes), the armature is operating wholly as a generator, and with an equal current of 50 amperes in each half of the winding, and the acceleration is negative. The armature is thus adding 100 amperes to the 100 amperes coming in from the primary mains. Nothing has here been allowed for the extra power needed to make up for the frictional and other losses. Suppose that an additional 4 amperes were needed for this, making 104 amperes as the motor current: in position 0° the two downward currents would be 52 each instead of 50, increasing the positive acceleration; in position 90° the upward currents would be 48 instead of 50, decreasing the negative acceleration. The output current at that instant would still be 200, being made up of 104 incoming plus 96 generated in the windings.

Prof. Thompson.

On further examining these diagrams it will be seen that the currents in the armature windings consist of a set of four currents, each in position 0° being 50 amperes. But as the armature turns these change. They increase in the two arcs that are shortening as the points where the slip-ring connectors approach toward the positions occupied by the commutating brushes. But in the two other arcs that are lengthening, the currents first decrease to zero, then increase again to 50. Further, the individual coils in different parts of the ring winding have very different currents to carry. A coil that is midway along the winding between the two connectors has to carry never more than 50 amperes. Four times in each revolution it carries 50, and four times it carries 0 amperes, with values intermediate in intermediate positions. (This will obviously set up four armature reactions in each period.) But a coil that is situated next to either of the two connectors has to carry a current which when it is close to the brush position rises to a maximum of 150 amperes, and changes abruptly to 50 in passing the brush; rising thus gradually twice to 150, or else rising twice abruptly from 50 to 150, and being zero twice, in one revolution. The necessary consequence of this is that the coils which are close to the slip-ring connectors are much more heated than those which lie midway along the periphery between the connectors. The distribution of the heating is here quite different* from that which would obtain in either armature of the motor-dynamo machine (Fig. 3). This unequally distributed heating effect is in total somewhat greater than that in the same armature if used purely as a continuous-current motor or generator, but is less than the total heating effect (for equal power) if used purely as single-phase generator or motor; the ratios of the heating being as 1 : 1·366 : 2 in the three cases. Or, if we consider the output of an armature to be limited by equal heating effects, the several outputs which would give equal total heating in the same armature would be :—As continuous-current generator or motor only, 100 kilowatts; as single-phase generator or motor only, 50;

* *Vide* Steinmetz, *loc. cit.*, p. 154.

Prof. Thompson.

as single-phase converter, 85 kilowatts. Fig. 6 exhibits this unequal heating effect in the case of a single-phase converter. The first line represents graphically by the outline

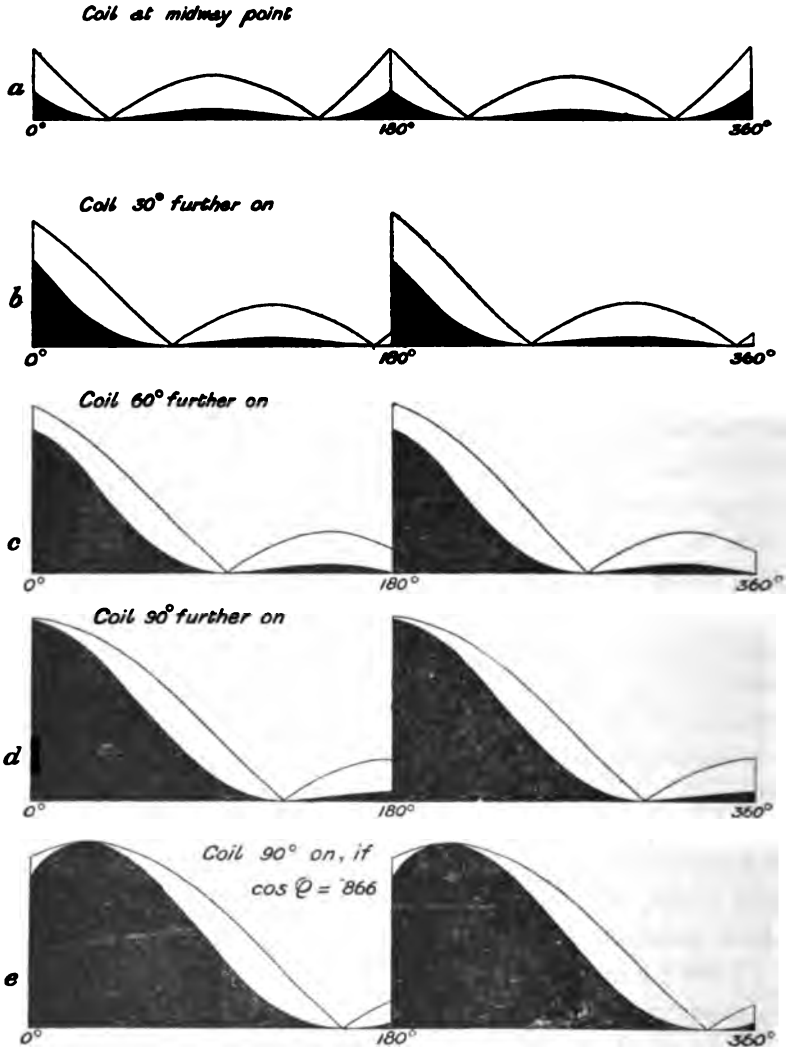

Fig. 6.

diagram the varying current (irrespective of direction) which a coil midway between the connectors will carry in one revolution; and the outline curve of the darkened area within it

is drawn with ordinates squared so as to be proportional to the heating effect at different instants. The total area blackened represents the total heating effect in that coil. The second line represents similarly the current and heating for a coil 30° further towards the connector. The third line represents current and heating for a coil 60° further. Whilst the fourth line represents similarly the current and heating in a coil that is next to the connector. The relative heatings in these four coils are about in the proportions 10, 21, 50, 86. The curves in the fifth line depict the case of one of the coils next to a connector when there is, in consequence of self-induction in the circuit, a lag of 30° in the phase of the current. The result of such phase-difference is an increase of heating for the whole armature, but an extra increase for those coils which are near the connectors. Prof. Thompson

We may now pause to consider what differences will be made in the preceding considerations if the distribution of the magnetic flux around the periphery of the armature does not follow a sine law. It will be noticed that when any arc of the winding (and this applies to two-phase and three-phase cases) lying between two slip-ring connectors is acting as generator, it produces its greatest electro-motive force when its mid-point is passing the mid-point of any pole. Hence, if the poles are narrowed so as to concentrate the field, even though the average electro-motive force for continuous-current purposes remains unchanged, the root mean square value of the electro-motive force will be increased, and the curve of induced electro-motive forces will be more peaked than a sine curve. If the machine is being used to convert continuous current into alternating, the effect will be to raise the relative alternating voltage and lower the relative alternating current, and, on a non-inductive circuit, the current also will have a more peaked curve. If the machine is being used to convert alternating into continuous currents, the converse result takes place, the relative value of the alternating voltage being lowered. An eight-pole converter at the Technical College, Finsbury, constructed in 1891 by the Allgemeine Elektricitäts Gesellschaft of Berlin, has a curve of induction which is remarkably close to a sine curve, and its relative voltage

Prof. Thompson.

for continuous and three-phase currents is practically identical with the theoretical value. Herr Friese, in the article already referred to, gives the following tabular comparison for three machines with ring armatures constructed by Messrs. Schuckert:—

Nature of Service.	Voltages as Measured.			Voltage Calculated.
	Machine No. 1.	Machine No. 2.	Machine No. 3.	
Continuous-current	100	100	100	(100)
Single-phase	71·0	71·8	—	70·7
Three-phase	61·3	62·0	61·8	61·2
Two-phase	71·0	71·8	—	70·7
Four-phase	49·8	50·7	—	50·0
Six-phase...	35·0	35·8	—	35·4

M. Hanappe* has published a careful analysis of a Schuckert converter, capable of yielding alternating currents in one, two, three, four, or six phases, and suitable for laboratory experiments.

If the machine is being used to convert a single-phase current into a continuous one, the form of the impressed voltage curve, and the distribution of the flux around the periphery, are of ever greater importance. The current delivered, though certainly continuous, will not be uniform, but will have a periodic fluctuation superimposed upon it by departures from the sine law. Armature reactions will, as we have seen above, impose fluctuations of a frequency double that of the primary current. Indeed, in any case the single-phase converter is a less satisfactory apparatus, from several points of view, than the two-phase or the three-phase converter. It has more considerable variations of armature reaction, a greater and more unequable heating, and requires a more accurate setting of the commutator brushes than the polyphase converters. Moreover, it is not self-starting from the alternate-current side. Notwithstanding these disadvantages, single-phase converters are in satisfactory use; for example, several constructed by the Elektrizitäts-Aktiengesellschaft (Schuckert & Co.) are in operation on the city circuit at Cologne.

* *L'Éclairage Électrique*, viii., 151, July 25th, 1896.

Prof. Thompson.

A single-phase converter when standing still can act partly as a mere stationary transformer. When running at a certain speed in a field with a given number of poles it will act as converter, converting into continuous current those currents only which come to it with a frequency corresponding to the frequency of the movement of revolving conductors past the poles. Any alternating currents or components of alternating current of any other frequency that may be superposed, can affect the continuous-current output at the other side by producing superposed fluctuations.

Passing on, then, to the case of the two-phase converter, we may at once apply the same principle of superposition of instantaneous currents to study the flow of current through the windings. Fig. 7, which gives the corresponding series of diagrams, requires no further explanation. The generator action during one-eighth of a revolution is illustrated by the four figures on the right of the upper line. The superposed action is exhibited in the lower line. Inductive reactions and energy losses are, as before, supposed to be absent. In position 0° the converter is acting neither as motor nor as generator, the current at that instant simply running through from the commutator to the slip-rings. In the position 15° it is observed that the currents in the armature windings consist of six sets. Two short portions are carrying a motor current of 85·35 amperes each; two others of somewhat longer arc are carrying a motor current of 14·65 amperes; whilst two quadrantal parts are delivering generator currents of 11·24 amperes. On the whole, in this position the armature is acting as motor, increasing the acceleration. In the next position, at 30°, the motor action has decreased. In the position at 45° the action is chiefly generator, the current in that part of the field which is strongest being at its maximum and flowing with the electro-motive force. It will again be noted that the currents in those coils o[illegible] armature which lie close to any of the four connectors [illegible] more heated than those midway along the intervening qua[illegible] the maximum current for the former being 100 [illegible] 50. The inequality of the heating is muc[illegible]

Prof. Thompson.

FLOW OF CURRENTS IN TWO-PHASE CONVERTER.

Continuous current : 100 amperes, at 100 volts.

Two-phase currents : 4 lines each carrying 70·7 virtual amperes ; line voltage, 50 virtual volts.

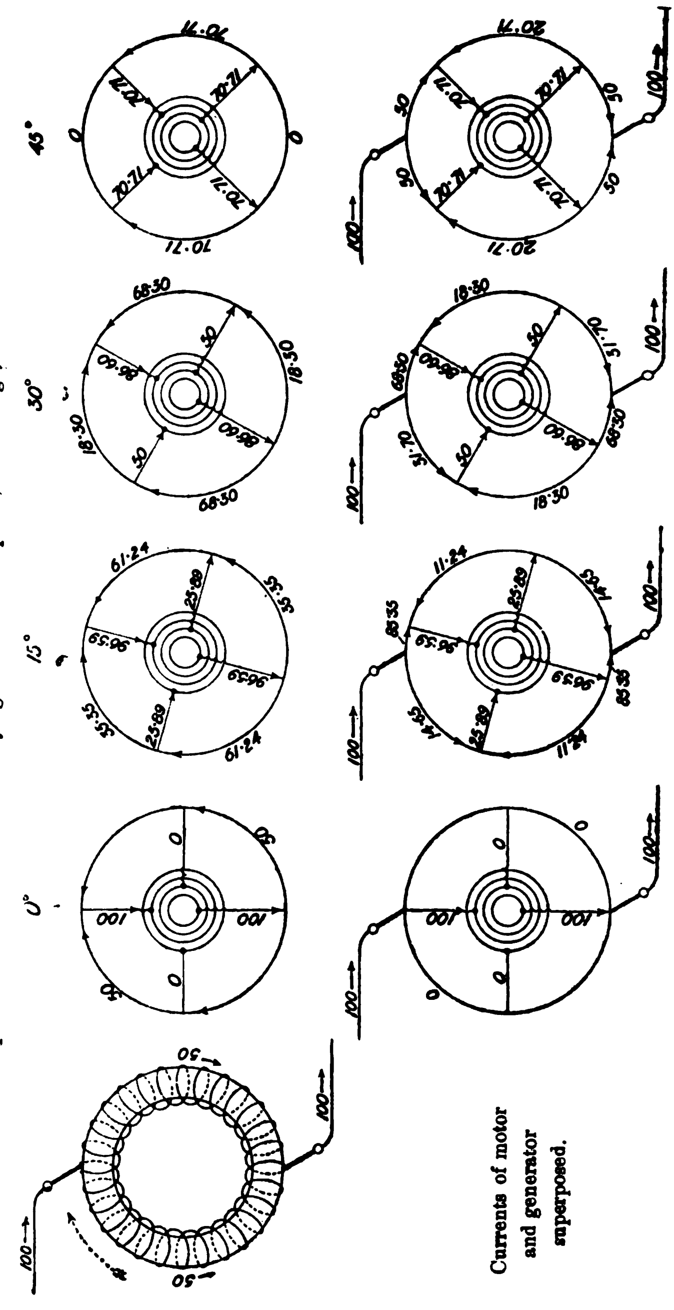

FIG. 7.

Prof. Thompson

Flow of Currents in Three-Phase Converter.

Continuous current: 100 amperes, at 100 volts.

Three-phase current: 3 lines each 94·3 virtual amperes; line voltage, 61·2 virtual volts.

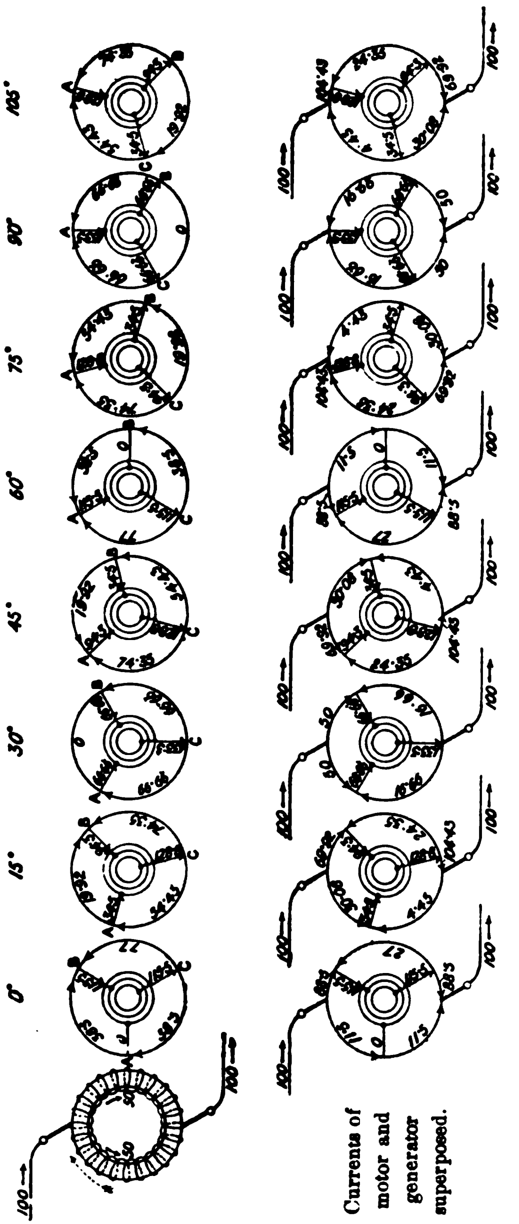

Fig. 8.

Prof. Thompson.

than in the single-phase armature. Also armature reactions are more nearly balanced throughout, and there is much less tendency to impart a periodic fluctuation to the continuous current. Such machines will be self-starting, and will give little trouble from sparking at the commutator.

Fig. 8 exhibits the flow of currents in the armature of a three-phase converter. Comparison of the successive positions will bring out several points. The current in any one of the three lines is at its maximum (on a non-inductive circuit, when generator; or when, as motor, the field has normal excitation) when the connector for that line is just passing a commutator brush; and its maximum is $1\frac{1}{3}$ times the continuous current. In certain positions of symmetry—for example, in position 30°—the motor and generator actions resulting from the flow are bilaterally similar. In other positions—for example, in position 0°—one side (the left here) is acting wholly as motor, the other side partly as generator and partly as motor. A coil which lies midway between two connector ends will carry a current that has a maximum of 27 in one part of its revolution, and another maximum of 50 in another part of its revolution; there being two of each of these maxima in one revolution. A coil situated close to a connector has 133·3 as its maximum twice in each revolution, with an abrupt change to or from 16·66 just as it passes the brush. The inequality of heating of coils is less in a three-phase converter than in a single-phase, but it is greater than in a two-phase converter. The inequality of heating in the case of two-phase converters has recently been examined analytically by Messrs. Woodbridge and Child in recent numbers of the *Electrical World* (Jan. 1st and Feb. 12th, 1898). Their paper, which is one of great skill, concludes with the interesting deduction that, assuming a power-factor of unity, if such a converter is driven mechanically so as to be generating *both* a continuous and a two-phase current at the same time, the heating of the armature will be less than would be the case if, with same output, it were used as either a continuous-current generator alone or a two-phase generator alone.

Amongst Continental firms Messrs. Alioth, of Basle, have

been prominent in the application in the industry of rotatory converters, which they designate as *commutatrices*. Mr. R. B. Ritter, one of their engineers, has kindly furnished many particulars of these machines and of their application. Articles by him on this subject have appeared in *L'Industrie Électrique* for 1896. He points out that in the application of these machines to charge accumulators from an alternating-current supply means are necessary for changing the voltage as between charge and discharge. There are several ways of meeting this need—by the use of choking coils, or of auto-transformers of variable ratio in the alternating side of the circuit, or by throwing in or out of circuit of a supplementary armature. The solution preferred by M. Ritter is the latter, a small auxiliary continuous-current machine in series with the continuous-current side being either mounted on the same shaft or driven by a pulley from it. The desired variations of voltage can be obtained by varying the excitation of the separate field magnet provided for this auxiliary machine. Prof. Thompson.

A large 100-kilowatt two-phase converter, constructed by Messrs. Alioth for Geneva, is described in *The Electrician* of January 8th, 1897, with sectional drawings. This is a 14-pole ring armature machine, having an armature 1 metre in diameter, running at 385 revolutions per minute. As it is designed to supply continuous currents to a three-wire distribution, it is wound with two independent converter windings, each capable of supplying 450 amperes at 110 volts; and is furnished with four slip rings, and with a commutator at each end. It has an efficiency of 90 per cent.

The problem of changing into convenient simple ratios the awkward percentage numbers which subsist between the voltages at the two sides of a converter has engaged the attention of several engineers. Mr. Heldt has described* an ingenious method of obtaining any desired ratio by the device of including on the alternating-current side additional windings between the slip-rings and the points where these rings are connected up to the

* See *Electrical World*, vol. xxviii., 68, July 18th, 1896.

Prof. Thompson.

armature winding proper. Fig. 9 illustrates the case of a single-phase converter. Here auxiliary windings (in two parallel circuits) are joined in so as to bring up the 70·7 volts on the alternating side to 100 volts, so as to make the voltages alike at both sides of the machine. To adapt this suggestion to the case of a three-phase converter, three sets of auxiliary windings must be intercalated between the three slip-rings and the points of connection to the ring winding; these auxiliary windings being chosen, as to number and position, so as to add the desired supplementary voltage. It will be seen that this is equivalent to using a mixed star and delta winding; the delta part being that

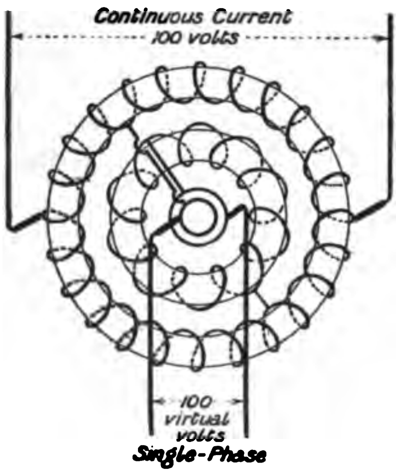

Fig. 9.

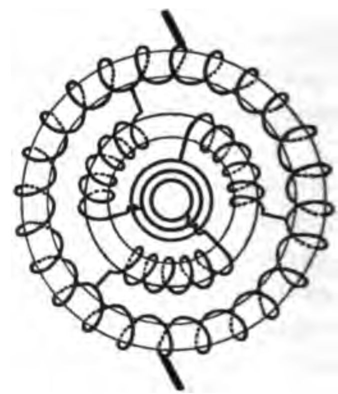

Fig. 10.

which alone constitutes the true converter winding. It is uniformly distributed around the core, and is symmetrically connected to the commutator.

Another solution of the problem, of more limited application, lies in so altering the distribution or concentration of the magnetic field at the poles as to change the irregular ratios into more regular ones. It was pointed out above that any concentration of the field will alter the conversion ratio. For example, the ratio in the case of the three-phase converter with sine distribution being 61·23 per cent., if the poles are narrowed a

Prof. Thompson.

little, this may be raised to 66·6 per cent.; so that a machine which is a 300-volt machine on the continuous-current side will be a 200-volt machine on the three-phase side. An example of this is afforded by a four-pole 55-kilowatt converter constructed by the Oerlikon Company, the description of which has been kindly furnished by Dr. Behn-Eschenburg. Drawings of this machine are given in Figs. 11 and 12. It was designed to receive continuous current at 300 volts, and to run at 600 revolutions per minute. Its armature is a two-circuit multipolar drum 522 mm. in diameter, 360 mm. long, having 117 slots and two conductors per slot. It has four sets of carbon brushes, set at zero lead. It runs quite sparklessly at all loads. As originally constructed the poles were furnished with pole-pieces (as shown) each of about 72° span, therefore having a breadth about 80 per cent. of the pole-pitch; the chord across the tips being 335 mm. So shaped, the coefficient of conversion was found to be 57·7 per cent., as against the 61·23 per cent. if sine distribution had been present. When supplied at 300 volts on the continuous-current side, its open-circuit three-phase voltage was only 173 volts. The pole-tips were then cut away, as shown in Fig. 12, so that the arc of pole-span was reduced to a little under 50°; the chord across the tips being now 228 mm., and the pole-breadth being 54 per cent. of the pole-pitch. This had the effect desired of bringing the conversion ratio up to 66·6, the open-circuit three-phase voltage being now 200. The ratio of conversion was constant within 2 per cent. at all loads. Fig. 13 gives a plot of the performance of the machine under different excitations. It will be seen that the ratio of the ordinates is approximately that of 3 : 2. Two other points plotted show the demagnetising reaction due to wattless currents. They affect, of course, the speed and heating of the machine, but practically have no influence upon its ratio of conversion when the excitation of the field magnets is normal. In its original shape, when operated as continuous-current motor at 300 volts, with an excitation of 2·1 amperes, without load on the three-phase side, it ran at about 470 revolutions per minute. When loaded so as to give out 153 amperes per phase on an

Prof. Thompson.

ROTATORY CONVERTER. Type A, IX., A.

Continuous-current side as motor for 300 volts taking about 190 amperes.
Three-phase side as generator at 200 volts, yielding 3 × 159 amperes, with frequency 20 periods.
Core-discs 117 slots, with two conductors per slot.
Conductors 234, net section 2·2 × 1·6 mm.
Diameter of core-discs, 520 mm.
Commutator, 117 bars.
Brush-holders, four sets, with five carbon brushes per set.
Revolutions per minute, 600.

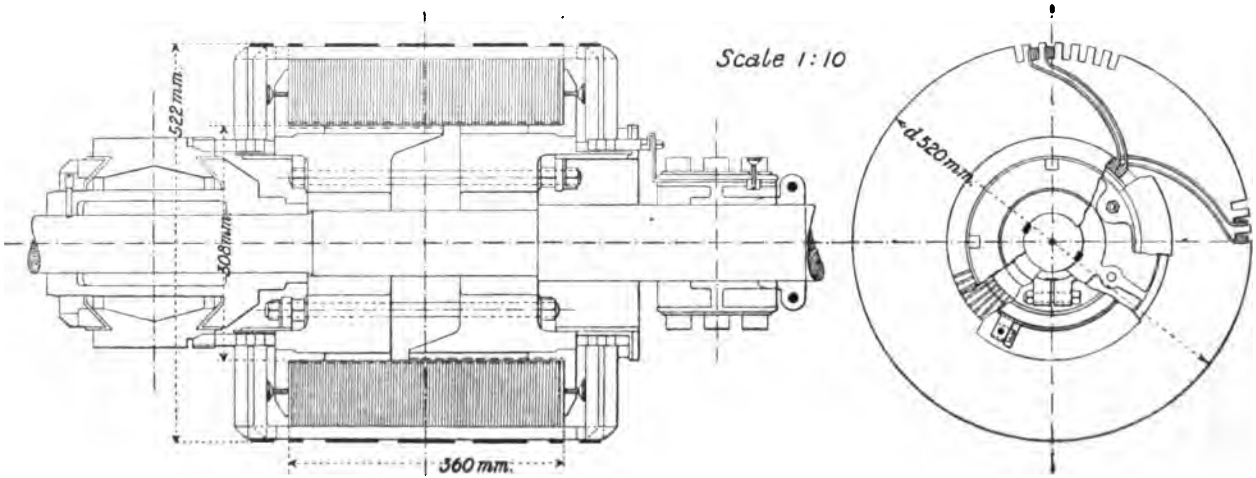

FIG. 11.—Armature of 75-H.P. Rotatory Converter (by the Oerlikon Company).

Prof. Thompson.

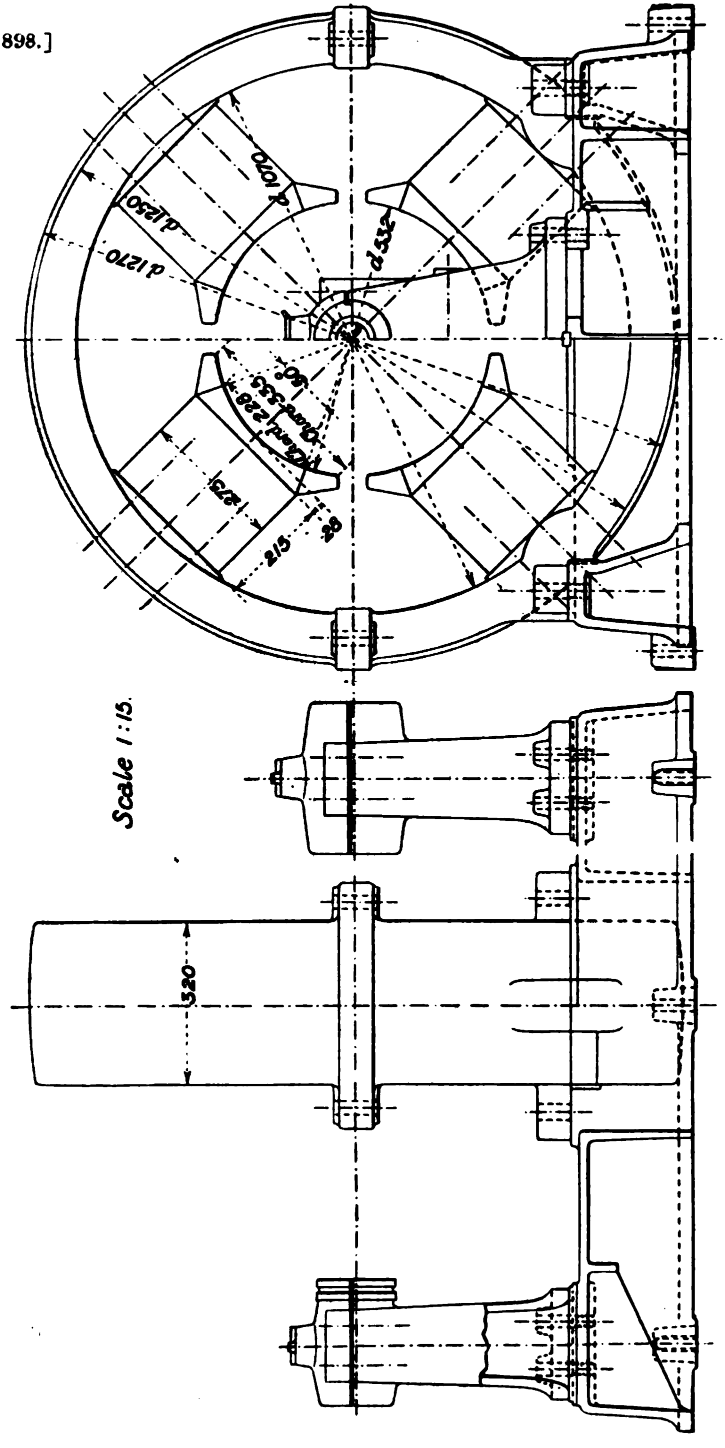

FIG. 12.—75-H.P. Rotatory Converter (Continuous Three-Phase), constructed by the Oerlikon Company.

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inductionless circuit the speed fell, in consequence of reactions, to 440 revolutions per minute. At this speed the measurements were:—Continuous-current side, 295 volts, 153 amperes; on the three-phase side, 165 volts, 3 × 153 amperes. Then the output was changed to one of 235 amperes per phase of lagging current on a load of induction motors running light. The speed went up to 640 revolutions per minute (showing a weakening of the field), and the instruments measured:—Continuous-current side,

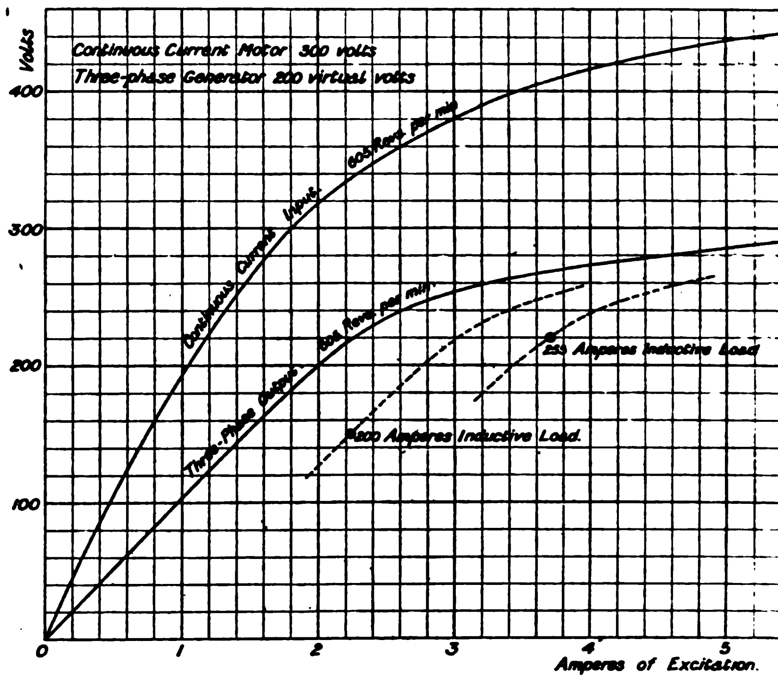

FIG. 18.

298 volts, 32 amperes; on the three-phase side, 157 volts, 3 × 23.5 amperes. To keep the speed, and therefore the frequency, constant under such conditions, the (shunt) excitation would need to be adjusted in accordance with the load and its lag.

The pole-cores and yoke of this machine are of cast steel, each pole being wound with 3,200 turns of a copper wire of 2 square mm. section; the total resistance of the shunt winding being 80 ohms. The armature resistance from brush to brush is 0·028

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ohm. The machine runs sparklessly with zero lead of the brushes. It will also run as a synchronous motor delivering continuous current sparklessly, with or without excitation of the field magnet, and with brushes set at zero, or set with either a large forward or a large backward lead. With a large backward lead, running synchronously, without excitation, at 627 revolutions per minute, giving out 46 amperes at 100 volts, it took in three currents of 155 virtual amperes (largely wattless) at about 100 virtual volts at the three-phase side. The reactions in this curious case are exceedingly complicated.

Quite recently Mr. Gisbert Kapp* has re-examined the question of the influence on the conversion ratio, and on the efficiency, of changes in the distribution of the flux. With the theoretical case of a sine-law distribution he compares analytically other cases, equally unreal in fact, namely, those of a distribution of supposed uniform density over a limited arc of polar span. From his calculations it may be deduced that the conversion ratio will be the same as for the sine-law distribution if the poles are such that the breadth of their faces is about 70 per cent. of the pole-pitch. If the faces are broader, the voltage ratio for a single-phase machine will be lower than 70·7 per cent., and the ampere ratio higher than 141·4 per cent. If the pole-breadth is reduced to $\frac{2}{3}$ of the pole-pitch, or to $\frac{1}{2}$ the pole-pitch, the respective voltage percentages become as follows:—

Voltage for	Sine Law.	Pole Ratio, $\frac{2}{3}$.	Pole Ratio, $\frac{1}{2}$.
Continuous-current	100	100	100
Single-phase	70·7	75	82
Two-phase	70·7	75	82
Three-phase	61·2	65	71
Four-phase	50·0	53	58
Six-phase	35·35	37	42

M. Routin† has made another study of the effect of varying

* *Elektrotechnische Zeitschrift*, vol. xix., p. 621, Sept. 15th, and following numbers of Sept. 22nd and 29th, 1898.

† *L'Éclairage Électrique*, vol. xi., p. 531, June 12th, 1897.

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the pole-breadth, in a mathematical paper, in which also he assumes the uniformity of the field beneath the poles.

In the Oerlikon converter above described the pole-breadth was only 55 per cent of the pitch, yet the voltage ratio was 66·6; showing that the field does not distribute itself uniformly, but is less concentrated, with fringing at the pole-tips.

Mr. Kapp has further treated the question of the limit of output for equal heating, for different pole-breadths, and for different angles of phase displacement in the currents. When the phase displacement is zero, the limiting output of a single-phase converter armature was (see above) 85 per cent. of that when used as a continuous-current armature simply, if the sine law is assumed. For pole-breadths of $\frac{2}{3}$ and $\frac{1}{3}$ respectively Kapp finds this increased to 88 and 95 per cent. This is equivalent to saying that if by concentration of field one makes the midway coils relatively more active, there will be for equal output less heating of the coils next to the connectors, and a less total heating. If there is a phase displacement such that $\cos\phi = 0·8$ ($\phi = 35°$), the percentage output for sine-law distribution is 69, but with pole-breadths $\frac{2}{3}$ and $\frac{1}{3}$ respectively this is raised to 73 and 80 per cent. But in practice the fringing of the field will reduce these latter values. For the more favourable cases of two-phase and three-phase converters these percentages of output for equal heating of the armature are greatly raised. The three-phase converter, assuming sine distribution and no phase displacement, has a limiting output 134 per cent. of that of the simple continuous-current armature. With pole-breadths of $\frac{2}{3}$ and $\frac{1}{3}$ the output rises to 138 and 144 per cent. respectively; and, if there is a phase dislocation such that $\cos\phi = 0·8$, to 117 and 126 respectively. For a two-phase converter the limiting output of 164 per cent., for the case of sine distribution and $\cos\phi = 1$, becomes 167 and 170 per cent. respectively for pole-breadths of $\frac{2}{3}$ and $\frac{1}{3}$; whilst, when $\cos\phi = 0·8$, these numbers fall to 144 and 153 per cent. respectively.

A four-pole 30-kilowatt two-phase converter, constructed by Messrs. Brown, Boveri, & Co., has been described to me by Mr. M. B. Field, who designed it for that firm. The armature

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is wound according to the scheme shown in Fig. 14. It has four poles (two wound, two unwound), and runs at 1,200 revolutions per minute, giving a frequency of 40 periods per second. Its efficiency is about 90 per cent. The input being two currents, of 415 working amperes each, in quadrature, at 40 volts, the output is 500 amperes at 60 volts. The armature is a cylindrical drum-wound, in 56 slots, with two conductors per slot, as a singly

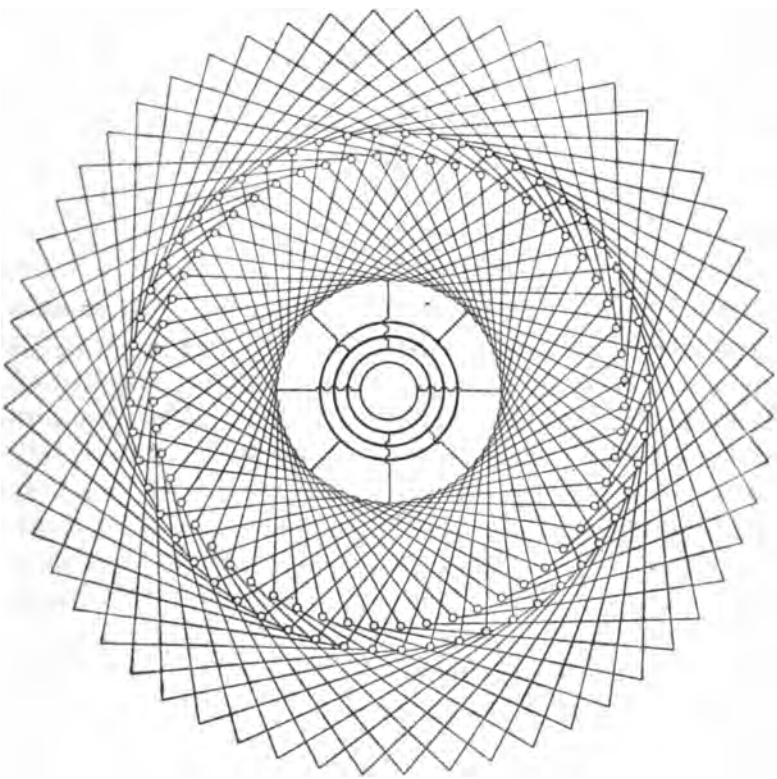

FIG. 14.

re-entrant double winding, being cross-connected at the slip-rings to give four parallel circuits. At normal load, the excitation is such as to give a flux-density in armature core body of 7,800 lines per square centimetre, and in the teeth of 10,700 to 14,900. It runs (as exciter for some two-phase alternators) in parallel with some turbine-driven exciters and with a two-phase to continuous

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motor-generator. The ratio of conversion w
phase motor did not alter greatly with th
following figures show :—

Exciting Current.	Virtual Volts between Slip-Rings.	Volts at Commutator.
2	18·5	30·0
3	27·0	43·0
4	34·0	53·0
5	41·0	62·0
6	46·5	68·5
8	53·0	77·0
10	56·5	82·0
12	59·0	86·0
14	62·0	88·5

It is, however, in America that the converte application on the largest scale, for the purpose of tinuous currents for tramway circuits, electric li and electrolytic processes from distant power static transmission is accomplished by polyphase cur voltage. These high-voltage currents, when th receiving end, are first passed through transform the pressure to a suitable lower voltage, and ar through the converters. The first large converters c Electric Company were designed by Mr. Parshall on ordinary multipolar continuous-current generators, w field magnets, and performed their functions well. designs were tried with relatively weaker field mag assumption that, in view of the lesser amount reaction, commutation might be effected sufficiently of a lighter construction. But though a three-pha will, without any winding on the field magnets, opera of the armature reactions peculiar to the synchronou regulation in such machines leaves much to be desired, a designs exhibit a return toward the construction usual polar continuous-current generators. By the kindn

Parshall and of Mr. Steinmetz, information as to a number of such large converters is available. Prof. Thompson.

At Niagara, where the Cataract Company generates two-phase current at 5,000 volts, converters are employed to supply continuous current for certain electrolytic processes—for the manufacture of caustic soda, and of aluminium For the Pittsburg Reduction Company's aluminium works the General Electric Company constructed a number of large converters. The two-phase current is first reduced by transformers to a pressure of 115 volts. At this pressure it is led into the converters, which give out the continuous current at 160 volts; the plant having a total capacity of over 10,000 amperes, equivalent to an output of about 2,000 H.P. At Schenectady were also constructed the six three-phase converters of 200 kilowatts each, for use in the power house at Brooklyn. As these machines are fairly typical of American practice, some information of their general construction is of interest. They are eight-pole 200-kilowatt machines, running at 375 revolutions per minute, the frequency employed being 25. The voltage on the three-phase side is 82·8; that on the continuous-current side 125, with an output of 1,600 amperes. The armature is 48 inches in diameter, its core-length being 7 inches between heads. This gives 4·95 square inches of peripheral surface per kilowatt.* The core-discs have 240 slots, with two conductors in each slot. Each lap of the drum winding is over 30 teeth. The gap measures 0·25 inch from iron to iron. The commutator is 36 inches in diameter, with 240 bars. The three slip-rings are each 18 inches in diameter, $3\frac{1}{2}$ inches broad, and 1 inch deep. The brushes are of carbon, eight sets of nine brushes each, and each brush with $1\frac{1}{4}$ square inches of contact surface. The brushes on the three-phase side are of copper. The

* The number of square inches of peripheral surface per kilowatt of output gives a basis for estimating to what degree in the design of an armature the specific utilisation of its materials has been carried. Modern dynamos of over 100 kilowatts output have from 4 to 12 square inches per kilowatt. Old machines and small machines vary from 10 to as many as 25 square inches per kilowatt. In this estimate the working part only of the peripheral surface is counted being the length round the periphery multiplied by the total (gross) thickness of iron parallel to the shaft. These values apply to alternators and motors as well as to continuous-current dynamos.

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flux through each pole-face is designed to be 4,380,00C full load, the flux-density in the cast-steel pole-cores bei 80,000 lines per square inch. In the gap the pole-face at full load is no less than 51,300 lines per square inch. teeth it reaches 135,600, and in the core-body 61,300. running as a converter the brushes have no lead. When ı on test as a continuous-current generator a lead equal to fo of the commutator was necessary. The pole-breadth is 6 cent. of the pole-pitch, and the voltage ratio of conversion 6 cent. The $C^2 R$ loss in the converter armature at full l 3,500 watts: it would be 6,250 if used as a continuous-cı generator. The total armature loss as converter is 6,005 v as continuous-current generator, 9,130 watts. The temper rise of the armature at full load is 27° C. as converte continuous-current generator it is 47° C. The temperature of the commutator at full load is 36° C. as converter continuous-current generator, 52°.

Another three-phase converter also constructed at Schenectady works is a 16-pole 600-kilowatt machine, running 188 revolutions per minute. In this machine the pole-brea is 72 per cent. of the pole-pitch, yet the ratio of conversion 71·8; the voltages at the respective sides being 115 and 1(The armature diameter is 100 inches, the core-length 8·25 inch giving 4·3 square inches of peripheral surface per kilowatt. Tl temperature rise after 17 hours with a conversion output of 57 kilowatts was 15° C. in the armature, 23° C. in the field magnet The average flux-density in the gap was 69,000 lines per squar inch.

To the same class of machines belong the four three-phase converters recently designed by Mr. Parshall (Fig. 15*) for the Central London Railway. These are 12-pole 900-kilowatt machines, running at 250 revolutions per minute Each weighs about 30 tons; and their guaranteed efficiency will be 95 per cent. at full load, 93 per cent. at half load. They are designed to take in their three alternating currents of 1,800 virtual amperes

* This cut is reproduced, by the courtesy of *Engineering*, from the description given in that journal, of the Central London Railway.

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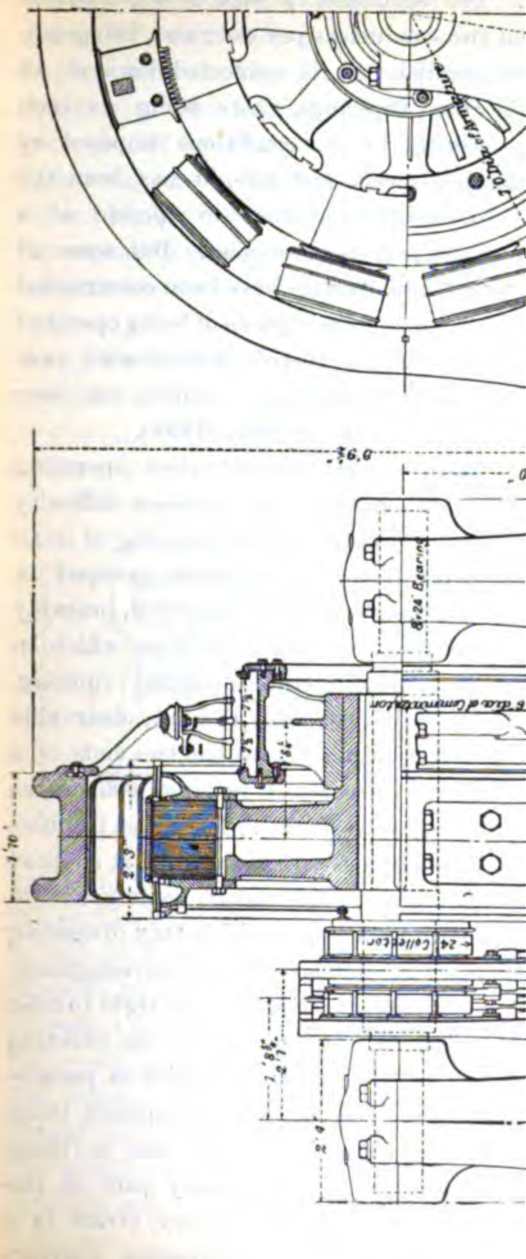

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each at 310 virtual volts; the output on the continuous-current side being at 550 volts. The armature in each is a multipolar drum with 432 slots, and two conductors per slot; and, being lap-wound, has 24 circuits in parallel. It is connected down at 18 symmetrical points to the three slip-rings, there being six such connectors to each ring. Owing to the conditions imposed by the relations between voltage, speed, and size, it has been the practice in the States to design converters to operate at a frequency of 25, or at most 30, periods per second. But some of the General Electric Company's converters have been constructed for frequencies as high as 60, so as to be capable of being operated from lighting circuits. One such, a 16-pole 400-kilowatt two-phase converter, running at 450 revolutions per minute, has been recently described in the *Electrical World* (May, 1898).

One difficulty which has been experienced when operating converters in their application on the large scale—a difficulty which could hardly have been foreseen from the working of small isolated machines—is a tendency for them, when grouped in parallel, to set up a see-sawing interference of slow period, probably in consequence of armature reactions, similar to those which in certain types of alternators trouble their parallel running. Another puzzling defect is a similar tendency to hunt, observable when two converters are arranged in series at the two ends of a long three-phase line for the purpose of feeding continuous currents from one point to another. The cure for these troubles will probably be found in an analogous treatment to that adopted for securing good parallel running in alternators, namely, careful design so as to prevent armature reactions from unduly distorting the magnetic field. But the point demands further investigation.

Before quitting the subject of converters, it is but right to refer to the existence of another class of rotating machine for effecting the same aim, and to which, for distinction, the name of *permutators* may be allotted. In this category are comprised those machines in which alternating currents (of one, two, or three phases) are first transmitted through a stationary part of the apparatus (a species of transformer), where they create in a re-entrant sectional winding a series of alternating currents

which differ from one another in phase by small successive phase-angles; these currents being then led off to a commutator and commuted, section by section, into a continuous current. To fix ideas, consider Fig. 16, which is adapted from a recent article by Dr. J. Sahulka in the *Zeitschrift für Elektrotechnik.* The lower part of the diagram represents the stationary transforming apparatus, having three pairs of projecting poles, wound with three circuits, A A', B B', and C C', to which are brought the three-phase primary currents. The action of these three-phase currents is to produce travelling polarities along the row of

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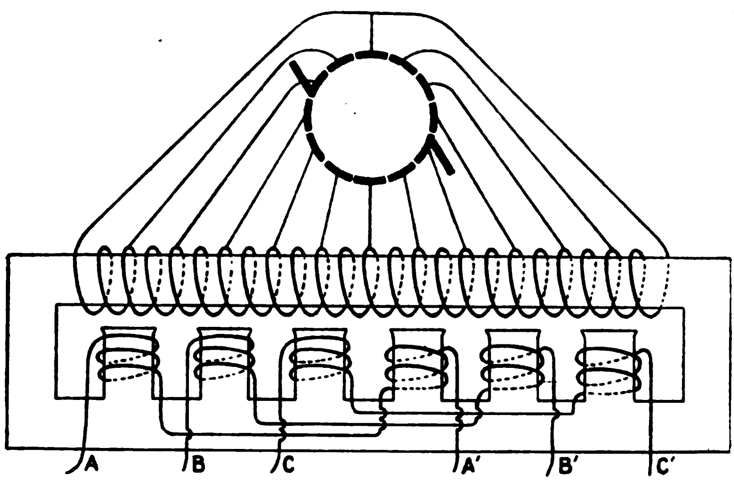

Fig. 16.

poles, and induce electro-motive forces in the coils wound around the portion of the core above the six pole-faces. This winding, it will be observed, is electrically the precise equivalent of a Gramme ring. In an ordinary Gramme dynamo the ring revolves between two poles. Here, instead, the two polarities travel past the re-entrant winding. Under these circumstances, seeing that the commutator stands still, it is necessary that the brushes should be rotated synchronously in order that they may collect the continuous current. An apparatus essentially upon this principle was patented* by Zipernowsky and Deri in 1888, the

* Specification No. 12,856 of 1888.

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brushes of this machine being rotated by a synchronously revolving unwound field magnet. How sparking was obviated does not appear. A further development in permutating machines was made by Messrs. Hutin and Leblanc, who turned the difficulty of the revolving brushes by the following device:—Upon the shaft of the synchronous motor is fixed the commutator and a series of slip-rings equal in number to the bars of the commutator, so that, though the commutator revolves, each segment may still be in connection with its corresponding point on the stationary re-entrant winding. The brushes may then be fixed as in ordinary machines. In *L'Électricien* of April 21st, 1894, a detailed account is given by M. Aliamet of the further ideas of these inventors. It appears that 18 slip-rings are necessary in practice. A further account of an installation at Épinay is given by M. Guilbert in *La Lumière Électrique* of June 16th, 1894. The sole advantage of machines of this type seems to be that they can transform and convert the three-phase currents from a high voltage without requiring the interposition of any other transformer. Their disadvantage in requiring many slip-rings is obvious. It is also very doubtful whether under varying conditions of use the sparking would not be excessive. Nevertheless, this type of converting machine is well deserving of the further attention of engineers. Probably there are cases where it is to be preferred to either the motor-dynamo group or the rotatory converter proper. The motor-dynamo can transform the voltage as well as change the species of current. The converter can change the species of current, but only by adopting a fixed ratio of voltages. As it is inexpedient to operate a commutator at voltages over 1,000, the converter cannot be used in conjunction with a high-voltage polyphase transmission except by the addition of a step-down polyphase transformer. It then becomes a question whether it is cheaper (regard being had to efficiency of operation as well as to prime cost) to instal the converter with the step-down transformer, or to substitute a pair of coupled machines as motor-dynamo. Experience shows that, at any rate in cases where the frequency of the supply is already sufficiently low, the former is generally the more economic. But it is quite probable that

there are cases where the economic conditions of the problem may point to the permutator as affording a solution preferable to either motor-dynamo or converter. Prof. Thompson.

At the close of the reading of the paper, views were shown depicting various forms of rotatory converters by Lahmeyer, Schuckert, the General Electric Company of Schenectady, and the Westinghouse Company; also one by Messrs. Alioth, having a double winding, for the purpose of supplying a three-wire continuous-current distribution from the two-phase supply at Geneva.

Mr. M. B. FIELD: I think that everyone here to-night will feel inclined to congratulate himself on having heard a paper of such interest. The subject of rotatory converters is of particular importance in connection with multiphase machinery. The subject has been but little dealt with in the papers in this country, but a good deal more in the American and Continental papers; but with the introduction of multiphase machinery in this country the adoption of the rotatory converter is sure to come, as the two go hand in hand. I think this paper of Dr. Thompson's is extremely interesting, especially for the way he has dealt with the distribution of the currents flowing in the circuits of the armature and the local heating of the different parts of the armature, without formulæ and without complicated vector diagrams, but in a way that people like myself can readily understand. As we have very little time to-night, I shall content myself by simply proposing that we adjourn the meeting, and that the discussion be opened this day fortnight. Mr. Field.

The meeting was then adjourned.

The Three Hundred and Twentieth Meeting of the Institution was held at the Institution of Civil Engineers, Great George Street, Westminster, on Thursday evening, November 24th, 1898—Mr. JOSEPH SWAN, F.R.S., President, in the Chair.

The minutes of the Ordinary General Meeting held on November 10th were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council :—

From the class of Associates to that of Members—

William John Graham.	Reginald F. Yorke.

From the class of Students to that of Associates—

William Edward Barker.	Alfred Henry Irvine Graham.

Charles A. M. Renton.

Messrs. C. P. Hammond and Leonard Andrews were appointed scrutineers of the ballot for new members.

Donations to the Library were announced as having been received since the last meeting from the Director-General of Indian Telegraphs and M. P. Lauriol, and from Mr. J. Pigg, Associate; to whom the thanks of the meeting were duly accorded.

The PRESIDENT: There are two letters – one from the American Institution of Electrical Engineers, and one from the South African Society of Electrical Engineers—which the Secretary will read.

The SECRETARY read the following letter from the American Institute of Electrical Engineers to the Council:—

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS,
NEW YORK, *October 31st*, 1898.

To The Council,
The Institution of Electrical Engineers,
London, England.

GENTLEMEN,

The Council of the American Institute of Electrical Engineers, in accordance with a resolution passed on the 28th of September, 1898, desire to express to you their earnest sympathy in the untimely loss of your Past-President, Dr. John Hopkinson. Although he was not a member of the American Institute, yet we share with you, and with the entire electrical engineering world, the results of his work, and we feel that his death has deprived us of a leader in our ranks, renowned as a scientist, prominent as an engineer, and universally esteemed as a worthy man.

We remain, Gentlemen,
Very respectfully,

A. E. KENNELLY.
CARY T. HUTCHINSON.
C. O. MAILLOUX.
J. W. LIEB, JUN.
HERBERT LLOYD.
F. A. PICKERNELL.
G. F. SEVER.
SAMUEL SHELDON.
WILLIAM STANLEY.
GEORGE A. HAMILTON.
RALPH W. POPE.

The SECRETARY also read a letter from the South African Society of Electrical Engineers, as follows:—

THE SOUTH AFRICAN SOCIETY OF ELECTRICAL ENGINEERS,
JOHANNESBURG, *October 28th*, 1898.

To W. G. MCMILLAN, Esq., *Secretary, Institution of Electrical Engineers, London.*

DEAR SIR,

At the first meeting of this Society held since news arrived of the sad death of your Past-President, Dr. John Hopkinson, F.R.S., reference was made to the great loss your Institution had thereby sustained, in common with Science generally, and a resolution was unanimously agreed upon expressive of our deepest sympathy with you in this great bereavement.

I trust you will convey to your Council, and that they will accept from the President, officers, and members of this Society, this message of sincere condolence, although it is so feebly expressed.

I am, dear Sir,
Very truly yours,

T. PERCIVAL PASK, *Hon. Sec.*

The President.

The PRESIDENT: Before we proceed to the discussion of Professor Thompson's paper, I think it would be opportune to say a few words with regard to the new Articles of Association.

It will be within the knowledge of most of those present to-night that, during the spring and summer of this year, the Council spent much time in revising the Articles of Association of the Institution, and that they presented the draft of the revised Articles for acceptance at a Special General Meeting of Members only, held at the beginning of this month. With a few verbal alterations, this draft was approved by resolution at this meeting, and was confirmed by special resolution at a second Special General Meeting held 15 days later. The new Articles of Association, which were yesterday duly registered, will not come in force until the 1st January, 1899; but it is desirable that I should at once mention a few of the principal changes and additions that will then be made in the rules.

Among the most important of these are the formation of a professional class of Associate-Members, with qualifications intermediate between those of Members and Associates, and the provision—made in accordance with the usage of nearly all other bodies under similar circumstances—that an Associate whose name is on the register of Associates on the 31st December next, may apply for transfer to the new class without going through the formality of being proposed and supported by Members of of the Institution in the usual way: provided only that he is at least 25 years of age, and that he can produce evidence to satisfy the Council that he is an electrical engineer or an electrician.

In order to enhance the value of full Membership, the new Article relating to the qualifications of Members has been made more stringent than that now existing; and the number of signatures required in support of any proposal for election or transfer, otherwise than to the class of Students, has been more than doubled. In every case two full Members must sign from personal knowledge of the candidate, and other Members or Associate-Members, the number varying with the class, must support the proposal. For the election of an Associate, the support of Associates will suffice, provided that the proposer and

seconder are Members. Students will, after January, require but one proposer. The President

The entrance fees and subscriptions of all classes for those who shall be elected under the new Articles will be slightly different from those now levied. In general terms it may be said that guineas are to be substituted for pounds. Owing to the increasing cost of the publications of the Institution, which are sent to all classes alike, it has been found that a loss is now incurred in respect of every Student on the register; the annual subscription of Students elected after this year will, therefore, be one guinea instead of 10s. 6d.—an amount which was fixed a quarter of a century ago, when the Journal of the Society was less than half its present size, and when there were no Abstracts to be provided. The annual subscriptions of new (full) Members, while residing abroad, will also be £2 2s. instead of £1. Again, owing to the low rate of interest on capital now obtainable, it has been found necessary to increase the life composition fee for all classes. The new rate will be levied from existing members who have not already compounded, as well as from those who may be elected hereafter; and in order to give time for members to determine whether they wish to avail themselves of the present rate, a period of grace has been allowed, and any Member or Associate now on the register may elect to compound at the present fee up to the date of the next Annual General Meeting, which will be held in May or June.

It has long been felt that some specific abbreviation of the titles of membership should be authorised by the Institution; and now the letters M.I.E.E. will hereafter stand for Member of the Institution of Electrical Engineers, and the abbreviations for the other classes will correspond to this. It has been thought, also, that some evidence of a member belonging to one of the professional classes of the Institution should be provided for. The Council have, accordingly, received authority to issue to any Member or Associate-Member a diploma, sealed with the seal of the Institution.

Next in importance to the series of changes just enumerated is the provision that has been made for the formation of local

The President. sections of the Institution. It is here sought to provide means by which local meetings may be held under the auspices of the Institution in large centres away from London where there may be a sufficient number of resident members. The Chairman of each local section will be *ex officio* a Member of Council of the Institution. Should any existing local Society with objects kindred to those of the Institution be desirous of uniting with us, power is now given to the Council to discuss, and, if thought fit, to arrange the details of the scheme by which such union may be effected.

Finally, it has been arranged that the Annual General Meeting shall be the last meeting in the session—that is, in May or June, instead of in December—and that the newly elected Council shall take office at once, so that the Presidential Address will be delivered in November, instead of in January. The Treasurer's statement of accounts will, as usual, be presented at the Annual General Meeting; only the financial year will no longer end on the 30th of September, but on the 31st of December, which is the last day of the subscription year. In order to bridge over the period of transition, it is provided by the special resolution that there shall be no election of Officers or Annual General Meeting in December of this year. The present Officers and Council will therefore remain in office until the first Annual General Meeting under the new Articles in May or June next. These new Articles will come in force at the beginning of the year.

I will now ask Mr. Field to continue the discussion of Dr. Thompson's paper.

Mr. Field. Mr. M. B. FIELD: In the course of his paper last Thursday week, Dr Thompson kindly expressed the hope that I would give a few more particulars of the 30-kilowatt converter he mentions on page 680. Fig. 14 differs slightly from the way this machine was actually connected up, in that the slip-rings are shown joined up to the opposite end of the armature. This makes a considerable difference, for, on the assumption that the commutator and slip-rings are at opposite ends, it will be seen that the short-circuited bars do not lie exactly at 90° apart.

There were 56 slots and 112 bars. Mr. Field.

The winding table began—

1 —— 30 —— 59 —— 88
5 —— 34 —— 63 —— 92 &c.

The pitch was thus 29 bars front and back; this, however, turns out to be 15 slots pitch one end, and 14 the other end.

The commutator was connected to the end at which the pitch was 15 slots, so that the short-circuited bars lay exactly 90° apart.

The best winding was obtained by choosing such a number of conductors that this number is divisible by 16.

The winding is a singly re-entrant double winding, thus equivalent to a parallel winding. The connections to the slip-rings consequently divide the windings into eight parts; these eight parts should not only be precisely similar, but should each contain an even number of conductors, so that all slip-ring connections are brought out at the same end. Hence the reason for making the number of conductors some multiple of 16. The number of commutator segments was 56: this is divisible by 4; thus all the brushes (there being four sets) stand in an exactly similar position relatively to one another and to the segments they are short-circuiting. In this way the E.M.F.'s acting in all parallel circuits at every instant, both on the continuous and the two-phase side, are equal, so that all internal balancing currents are avoided.

The figures given in the table on page 682 were taken when running the converter as a continuous-current motor. The field was separately excited, and the commutator voltage adjusted till the speed was exactly 1,200 revolutions per minute.

Taking out the fourth reading, we have—

X.	Two-Phase Volts.	Continuous-Current Volts.
5	41	62

When running it as a converter transforming from two-phase to continuous, with a continuous-current load of 200 and 410 amperes, the values of exciting current giving minimum two-phase current lay between 5·9 and 6·3 amperes; showing that a

Mr. Field. large increase in the excitation between no load and full load need not be allowed for in calculating the field winding.

I had the good fortune to be in the power station one day when a "short" occurred on the mains. The converter was driven from the two-phase side and fairly heavily loaded, and was supplying the exciting current for the two-phase generators. In parallel with it was a turbine-driven exciter of capacity 75 amperes and 60 volts, if I remember rightly—anyhow, a very small affair.

I was interested to see what would happen, and told the machinist to let everything alone. The alternating volts sank, of course, to zero, and I judge the short lasted 15 seconds, after which it was probably burnt out or a fuse went somewhere; anyhow, the generator volts rose slowly and nothing happened. The converter was still in step, and delivering its proper share of exciting current.

This was very interesting to me, as I had previously had a large amount of trouble in another central station where the excitation was furnished by two exciters directly coupled to 100-H.P. two-phase 5,000-volt motors. In the first days of this station it was no very uncommon thing for a "short" to occur somewhere on the 200 kilometres of overhead line which left the power station. The result of this was that the motors would fall behind, and if the "short" lasted more than a second or so the excitation would die out, and the whole station would shut itself down automatically. I found by experiment that a short-circuit on the 5,000-volt line lasting only five seconds was sufficient to shut down the whole station.

It was, then, very satisfactory to see that this did not occur with the 30-kilowatt converter, if running in parallel with an independently driven exciter of but one-seventh the capacity.

The heating of the poles of this converter was pretty considerable, but I think this was due mainly to the proportion between width of slot and air gap.

I would like to ask Dr. Thompson how the pole-heating of a converter compares with that of the same machine used as a continuous-current generator. The peaky and variable form of the resultant armature currents shown on page 666 would lead

one to expect that the pole-heating might be pretty considerable. Mr. Field.

I notice the converter shown on page 685 has laminated poles; that on page 677 has solid poles; and I think it would be very interesting if Dr. Thompson would tell us which of the many machines he exhibited on the screen had laminated poles, and which had not.

Dr. Thompson exhibited one converter with two completely independent windings for working on a three-wire system. It is interesting to note that two converters working in parallel on the three-phase side cannot be joined up to give a three-wire system on the continuous-current side. We should get the following connections:—

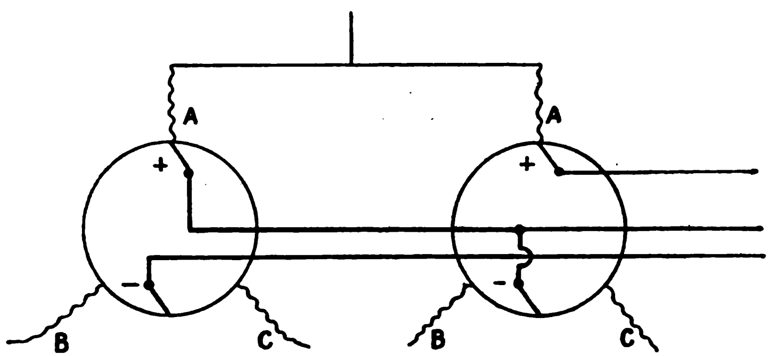


Fig. A.

In the position represented in Fig. A—viz., when one brush touches on a commutator segment to which one of the slip-ring connections is attached—lead A is connected to both + and − brush of right-hand converter, causing a dead short. This would occur six times per revolution. It would be necessary to supply the three-phase current to the two converters from independent transformer windings.

One way of connecting a single converter to a three-wire circuit would be to connect between the three slip-rings a three-legged choking coil wound on a three-legged magnetic circuit, taking the

Mr. Field. neutral point of the same as starting point of the third wire thus:—

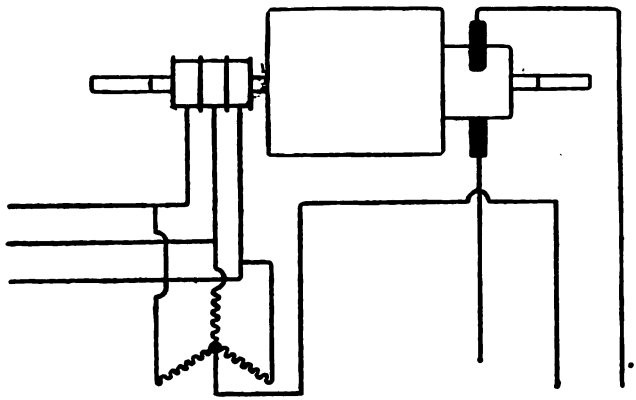


FIG. B.

One can also connect the neutral wire to the neutral point of the transformers, where these are used, doing away with an extra choking coil.

This corresponds to Dobrowolski's method of connecting a continuous-current machine to a three-wire circuit by employing two slip-rings and a choking coil.

If the three-wire circuit be very unevenly balanced, a slight fluctuation of three times the frequency of the supply is occasioned by this method of connection.

With regard to a variable conversion ratio, this is often of considerable importance. At any rate, one often wishes to vary the volts in a simple way. On page 673, Dr. Thompson mentions several ways; they all have their advantages and disadvantages. An auxiliary booster on the same shaft is expensive, on account of the large size of commutator needed. This must carry the full current, and, in consequence, usually a small standard exciter or continuous-current machine cannot be employed, but something quite special must be made. Also, the reaction in the converter is no longer balanced, as the extra power necessary for the booster must be supplied. This means larger armature conductors, heavier field winding, and altogether a larger machine.

The type of armature winding on page 674, with the three auxiliary star arms, should also give a variable conversion ratio depending on the excitation. Dr. Thompson gives this as a method of obtaining an *abnormal* conversion ratio, not a variable one, but I think it would give both. Mr. Field.

The winding is equivalent to Fig. C, and the ratio between the △ voltage and the continuous remains constant or nearly so for all field strengths; the star-arm voltages, however, depend directly upon the field strength; hence the total conversion ratio would depend on the excitation.

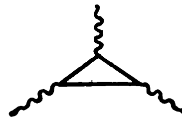


Fig. C.

Here again reaction comes in, and must do so always where a variable conversion ratio is obtained.

There is one important way for making the continuous-current voltage depend on the excitation, and that is by the use of leaky transformers.

If a transformer have much magnetic leakage, everybody knows that the terminal volts drop enormously on load, and especially when supplying a lagging current. If the current lead the volts rise. Now with a rotary converter we have simply to over- or under-excite it in order that the supply current may lead or lag. By altering the field excitation of the converter, we can effect a variable ratio of transformation on the transformers, not on the converter. With the 30-kilowatt machine already mentioned the transformers supplied were very good ones, and I could not much over-excite the converter. The variation obtainable under working conditions was, however, about 5 per cent. Of course it was not designed with this object in view. I have, however, heard of a case where the transformers were made specially leaky, and a variation of 1 : 2 could be obtained by varying the converter field. Such a method would not pay for large units and with a considerable length of transmission line.

With regard to the hunting, or see-sawing, of rotaries, I am very glad this point has been brought up, for it may assume a very serious character.

One must remember that all converters from one form of

Mr. Field. electrical energy to another are, to a certain degree, unstable in their distribution of the load when grouped in parallel. For example, two transformers of different ratings and different makes very seldom divide the load in the right proportion when working in parallel, small differences in their relative magnetic leakages producing great unbalancing effects.

I was working once with two motor-generators; the motors were two-phase 5,000-volt ones, generators continuous-current. When coupled together on the primary and secondary side, by merely varying the excitation of one of the generators, the load might be divided between them, thrown entirely on the one or on the other, or one might be made to work reciprocally, the generator running as motor driving the high-tension motor above synchronism when it acted as an alternator and pumped back power into the supply.

Now, when converters run in parallel, what conditions determine the way they distribute the load? Of course the excitation determines it largely, but there are other factors which enter in, and balance is sometimes very easily upset. I recall having heard lately of a case where two similar converters could not be induced to equalise the load, and the whole trouble was found eventually to lie with the brushes and the unequal resistance they offered at their rubbing surfaces. This corrected, they equalised the load satisfactorily. Under certain conditions, however, they certainly have a tendency to oscillate, and Dr. Thompson throws out the suggestion that a remedy is to be found in making the field very stiff and bristly.

Undoubtedly this helps greatly. Alternator designers nearly all tend in this direction—get the field winding as near the pole-tips as possible, to prevent the field being blown out sideways by the reactionary effect of the armature currents; reduce the air gap to get a stiff, bristly field; and if a larger air gap be necessary to reduce the armature self-induction, do not necessarily put it all at the polar surface, but interpose it somewhere else: as far back as 1894 Kapp constructed his stationary armatures of segments with $\frac{1}{8}$ inch air gap between, and claimed that these

interposed air gaps were of special advantage in keeping down the self-induction of the armature windings. Mr. Field.

I will not wait to comment at greater length now; the whole subject bristles with points which might be discussed. I hope some other speakers will give us some information on the use of centrifugal speed governors in connection with converters working in pairs reciprocally—or in series, as Dr. Thompson calls it—upon starting from the three-phase side, and upon the use of series windings to help or replace the shunt winding for this purpose.

Mr. Swinburne.

Mr. J. SWINBURNE: If we begin with a simple case of a direct-current machine with double-wound armature and two commutators, we have a transformer whose moving system does no work except that of rotating the commutators. There would be no armature reaction or shifting of brushes, as the equal and opposite currents neutralise each other's effects. Professor Thompson treats this as discovered in 1888, but surely every dynamo designer understood it long before that. In this case the power put in is uniformly equal to that taken out, less the loss in the machine. But if by means of a slip-ring one side is an alternating circuit, the machine must store and give out energy twice a period. This it does, not magnetically, but by altering its energy of rotation—that is to say, by going faster and slower. It alters its speed four times a period. This speed alteration is, of course, very small, and very quick—a mere tremble, in fact.

The question of hunting is important, but I think it is just the same as the question of hunting in the case of alternators or motors. This hunting effect was, I think, discovered by Mr. H. E. Harrison. At any rate, he called my attention to it some years ago, and I wrote an article in *Industries** in which I gave what I believe is the correct theory. It is analogous to the hunting of engines whose governors are not properly designed.† The hunting of engines has been very little studied, and the hunting of alternators still less. In the case of engines people

* "The 'Hunting' of Parallel Alternators," *Industries*, 31st March, 1893.

† B.A., 1894; also *Engineering*, 17th August, 1894.

Mr. Swinburne. say, "Put on a heavier fly-wheel," or "Make the governor more "nearly isochronous," or something of that sort. In dynamos people say, "Make the rotating part heavy," or the fields strong or weak, and so on. But really that sort of thing is no use. The first thing is to get a clear idea of the cause of the trouble, the next to cure it. A heavy fly-wheel or a heavy rotating element may in one case do good, and in another harm.

Mr. Esson. Mr. W. B. Esson: I am sure the Institution is very much indebted to Professor Thompson for bringing forward at the present moment the important question of rotatory converters, and I think the author deserves congratulation for the very lucid way in which he has explained the whole thing. To my mind, nothing could be clearer than the diagrams exhibited; they tell exactly what is occurring at every instant, and I cannot imagine anything which would enable us to grasp the principle of the rotatory converter with greater clearness than those diagrams. If I remember rightly, the rotatory converter was first exhibited on a practical scale at the Frankfort Exhibition. Professor Thompson has referred to the work of Lahmeyer exhibited there. I do not remember very clearly about Lahmeyer, but I remember very clearly the many interesting machines which the great firm of Schuckert & Co. exhibited. These machines, I think, fascinated most of the visitors from England to the Frankfort Exhibition. I do not know whether others took the same course as myself, but the first thing I did when I came home was to put slip-rings on a dynamo, and observe the results obtained by taking alternating currents and continuous currents simultaneously from the machine. I exhibit the figures I obtained, and they show very clearly the ratio between the alternating and the direct currents. The machine was a compound 150-light dynamo which under ordinary conditions gave the amperes and volts stated, running at a speed of 1,180 revolutions per minute. First it was tested taking out a direct current and a single alternating current at the same time. The alternating current was kept constant at 30 amperes, and the direct current gradually increased; the effect of the reaction on the field due to the starting of the alter-

nating current being evident from the figures (Table I.). You see that at no load the machine gives 95 volts, and that when the alternating current is put on it drops to 85 volts—that Mr. Esson.

150-Light Compound Dynamo.

Speed, 1,180 Revolutions per Minute.

Amperes ...	0	12·5	20·5	36·0	65·8	74·7	90·0
Volts ...	95	99·0	102·0	102·0	102·0	102·0	102·0

Table I.

Direct.		Alternating.		Direct.		Alternating.	
Amperes.	Volts.	Amperes.	Volts.	Amperes.	Volts.	Amperes.	Volts.
0·0	95·0	0·0	68·2	77·0	99·0	30·0	74·0
0·0	85·0	30·0	58·0	86·5	100·0	30·0	75·0
16·0	92·0	30·0	65·2	92·5	100·5	30·0	75·0
27·0	93·0	30·0	67·5	38·0	94·0	58·5	63·0
36·0	94·0	30·0	69·0	49·0	101·0	32·4	72·0
42·5	96·0	30·0	70·5	54·0	96·5	52·4	68·0
49·0	97·0	30·0	70·5	60·0	99·0	36·5	72·0
71·0	98·5	30·0	74·0	79·0	100·0	31·0	74·5

Table II.

Direct.		Alternating. No. 1.		Alternating. No. 2.	
Amperes.	Volts.	Amperes.	Volts.	Amperes.	Volts.
0·0	70·3	32·0	51·5	32·0	50·5
16·0	86·0	32·0	64·5	32·0	63·0
29·0	89·0	32·0	66·5	32·0	66·0
43·0	91·0	32·0	69·0	32·0	70·0
57·0	94·0	32·0	72·0	32·0	71·5
63·0	95·0	32·0	71·5	32·0	72·0
70·0	95·0	32·0	71·5	32·0	71·5

is, a drop of 10 volts. When the series winding comes into play, however by increasing the direct current, the volts are very soon raised. Then we took out of the machine two alternating

Mr. Esson. currents (Table II.)—the ordinary two-phase arrangement, in fact. Here, due to the further reaction, you see that, instead of being 85 volts, as when one alternating current is flowing, the pressure is at once brought down on the direct side to 70 volts. These figures were published in my articles on the Frankfort Exhibition in November, 1891,* and I exhibit them here as being of considerable interest, because they give the results of, I believe, the first experiments made in this country on a practical dynamo with slip-rings—a rotatory converter, as it is now called.

As a general principle, the more tappings you have in a continuous-current winding from which the alternating current is taken, the more energy you can convert with the apparatus. For instance, in the single-phase there are two tappings on to the continuous-current winding; in a three-phase, three tappings; and in a two-phase, four tappings. The result is that you get out of the two-phase the greatest output for a given size of machine; and I put it to Professor Thompson that, at any rate as regards the rotatory converters, two-phase currents are very much superior to three-phase, because you can get converted by the machine something like 25 per cent. more energy with two phases than with three phases.

With regard to the see-sawing action which Professor Thompson has mentioned, this, as he says, requires more investigation; but I would like to ask him whether he does not consider it very necessary that the form of wave given by the generator should correspond exactly with the form of wave given by the winding of the rotatory converter. I think, again, that a good deal of the see-sawing action is due to the fact that the voltage ratio alters with the excitement of the field magnets, and with the current flowing; and as it is rarely that you can get exactly the same ratio in different machines when working in parallel, currents pass between them and upset the balance of things. This I have been told by American engineers who have had a good deal of experience with rotatory converters.

Now there is great difficulty sometimes in determining whether one should use a rotatory converter or an alternating

* *Electrical Review*, vol. xxix., p. 529.

motor with a dynamo attached to it, for converting alternating into direct current. These difficulties increase when we learn that in some stations where rotatory converters had been put in, they are being pulled out to put in alternating-current motors with dynamos attached; while in other cases where motors and dynamos coupled had been put in, these in turn have been pulled out to put in rotatory converters. I think that the rotatory converter has its sphere mostly in tramway work, where, for one thing, absolute steadiness of the voltage is not of paramount importance. If the voltage rises or falls it simply means that less or more current is taken from the line; whereas, of course, in lighting, uniformity of pressure is the essential thing. Another thing is that tramways always work on the two-wire system, whereas for lighting we want three wires; and it appears to me that when one comes to balance up the cost of an alternating-current motor having an ordinary dynamo coupled at each end of it, against a rotatory converter constructed for the three-wire system, with all the complications of poles and brushes that appeared in the wonderful machine shown on the lantern screen, the balance in cost would not be greatly, if at all, in favour of the rotatory converter; while there is no doubt whatever that for simplicity such a machine is not to be compared for a moment with the motor-dynamo which I have mentioned. Mr. Esson.

It so happens that I am connected with the laying down of the first two-phase central station in this country, and there we have to convert in several districts the alternating current into direct current. Well, we are not doing it by rotatory converters; we are doing it by induction motors having dynamos coupled at each end of the shaft, working on a three-wire system. One of the reasons why we are not using the rotatory converters, I will confess, is that we do not know so much about them as we do about the other machines; but I might add that the accounts we have had of the rotatory converters are not over-satisfactory. There is another point I would mention, and that is, the rotatory converters are not well suited for such frequencies as can be employed for lighting by alternating currents without conversion.

Mr. Beeson. A rotatory converter suitable for a frequency of 50 has just as many poles as an ordinary alternator for the same frequency, with the complication of brushes thrown in; and, as it is rather troublesome to keep such a machine in order, I think this limits the sphere of the rotatory converter considerably—at any rate, for lighting work. Of course, if the whole current generated is to be converted into direct current, the frequency does not come in; but if the system is to be flexible, and permit of the current being distributed either alternating or direct at will, the necessity for having such high frequency for the rotatory converters is rather important. However, as I said, the station referred to is the first of its kind; we may try rotatory converters as well as motor-dynamos, and very soon we shall know a great deal more about their working, I have no doubt.

There is one point I would refer to in Mr. Field's speech. He said that in 1894 Mr. Kapp had designed alternators with air gaps located between the armature coils to reduce self-induction. I agree with Mr. Field's remark that there is no necessity to have all the air gap between the poles and the armature core; but, while that may not be the best place, certainly between the armature coils is the very worst place. My idea is that Mr. Kapp did not put an air gap between the coils for any such purpose, and I may remark that, as a matter of fact, in the first machine constructed Mr. Kapp found fault with the air gaps, and for the second suggested that the armature sectors should be fitted together with planed joints.

Mr. Hobart Mr. HOBART [*communicated, and, by the direction of the President, read by the Secretary*]:—Professor Thompson has referred to tendencies to "hunting" in rotary converters, and has suggested that—

"The cure will probably be found in an analogous treatment "to that adopted for securing good parallel running in alternators, "namely, careful design, so as to prevent armature reactions from "unduly distorting the magnetic field."

From my own acquaintance with rotary converters, I should say that the cure for all erratic tendencies of this kind which they may exhibit, is to be found in supplying steam engines for the

generating plant which shall have a very uniform angular velocity. This is also the cure for most of the difficulties which ever attend the paralleling of alternating-current generators, which Professor Thompson states has been rendered practicable by more careful attention to design. Mr. Hobart.

With regard to these phenomena of "surging" or "hunting" or "drifting," I consider that there is now no necessity for tolerating them. I have some cases in mind where considerable difficulty was at first encountered on this score. It came about in this way: The rotaries were especially designed with laminated pole-faces for the purpose of eliminating eddy-currents therein; and with perfect speed conditions this would have resulted in a higher commercial efficiency than would have been obtainable with solid pole-faces, since with the latter there is a higher eddy-current loss than for the corresponding continuous-current generator, owing to the cyclic rise and fall of the alternating-current component of the resultant current in the armature-face conductors as they sweep past the solid pole-faces. But the elimination of this loss deprives the machine of the use of the induced currents in the pole-face for tending to keep the armature rigidly in synchronism, and it departs from synchronism by a sufficient fraction of a period to introduce disturbance of the smooth running conditions. Consequently, it becomes necessary to resort to devices to correct the trouble.

These devices involve some sacrifice as regards efficiency; the amount of the additional loss entailed being entirely dependent upon the closeness of regulation of the engine used. With first-class multiple-crank engines with liberal fly-wheel capacity, I should say that the loss in efficiency would rarely exceed 1 per cent., and would generally be less. But unless a fair engine specification is rigidly adhered to, it might often run up to 2 per cent. or 3 per cent.

I might also mention that triple concentric cables have been found to introduce troublesome unbalancing of the voltage, and for rotary converter installations should preferably not be employed. Symmetrically arranged, three-core cables are very satisfactory; but instead of employing one three-core cable to

Mr. Hobart. transmit the power, it can with advantage be divided up into two or more component cables, these being joined in multiple at the ends and carried to the switch terminals as one set. I recall cases where the engine speed was troublesome and corrective devices had not been applied, where attempts to run two rotaries in parallel often resulted in failure when only one three-core cable was in service, although this was of ample capacity. The throwing in parallel of another similar cable permitted of stable parallel running of the rotaries. They were subsequently adjusted to run perfectly smoothly and satisfactorily with only one cable.

The main argument in favour of the superiority of effecting the transformation by means of rotary converters and static transformers, as compared with the motor-generator method, is based upon the lower first cost and the higher combined efficiencies.

A good example to cite is that of the Blackrock sub-station of the Dublin Electric Tramways, where 30-cycle 200-kilowatt rotary converters are fed by oil circulation transformers, the combined efficiency being 92 per cent. at full load and 91 per cent. at half load.

Taking the combined cost of rotary and transformers as 100, the cost of the equivalent motor-generator set would have been 110, and its combined efficiency would have been 85 per cent. at full load and 82 per cent. at half load; that is, the total energy wasted in the transformation would have been just about doubled.

But the very high efficiency obtained in Dublin involved higher cost, particularly as regards the static transformers, than in another case where air-blast transformers were used. Here, at rather lower efficiency (90 per cent. at full load), the combined cost was only 80, as against 100 for the Dublin sets, and 110 for motor-generator sets.

We thus see that there is certainly a decided economy, both in efficiency and first cost, in using rotary converters instead of motor-generators.

However, the motor-generator is much more flexible than the

rotary converter as regards independent control of the alternating-current phase relations, and the continuous-current voltage and output conditions. Range of adjustment of the commutator voltage in the case of the rotary converter is only obtainable by virtue of lagging and leading currents, and reactance, or else by resorting to auxiliary boosters or to variable ratio transformers. All such additional appliances tend to cut down the margin of advantage which the rotary converter undoubtedly possesses. Mr. Hobart.

Long ago I investigated the matter of resultant $C^2 R$ values in the armature conductors of three-phase rotaries, pursuing practically the same plan adopted by Professor Thompson; but in addition to investigating the case of power-factor unity, I also deduced values for other power-factors. The results are:—

For power-factor unity, the resultant armature $C^2 R$ is 58 per cent. of that of the same armature in a continuous-current generator of same output.

For power-factor 0·87	... ..	85 per cent.
„ „ „ 0·5	... ...	375 „
„ „ „ 0·0	... ...	∞ „

These results are on the assumption of 100 per cent. conversion efficiency.

I do not think sufficient emphasis has been laid upon the fact, set forth by Steinmetz and by Kapp, that in six-phase rotaries the alternating and continuous currents in the armature cancel each other so much more effectually that, while as regards armature heating the three-phase rotary converter has an advantage of 34 per cent. over the same machine operating as a continuous-current generator, the corresponding advantage possessed by the six-phase rotary is no less than 95 per cent.; *i.e.*, almost twice the output may be taken from it for equal armature heating. Nor does it introduce much complication.

The high potential circuits, both line and static transformers, remain exactly as for three-phase. The only difference is that all six secondary terminals are, in the sub-station, carried from the six secondary terminals of the three transformers to six collector rings on the rotary converter.

Hence the only additional copper is that required for these

Mr. Hobart. short leads. In such a case, one would arrange not to have any switches in the low-tension alternating-current circuits, thus avoiding complications at switch-board.

In rotary converters, the current is incompletely commutated. On a certain plant I once interposed the 300-volt coil of an 80-kilowatt transformer between the negative 'bus-bar and the rail. Now, had the current really been uniform, there would have been but a trifling drop—say 5 volts—through the ohmic resistance of the transformer coil. But there was a far greater drop across it.

The interposition of the resistance of this coil greatly reduced the telephonic disturbances on a branch of the telephone system which used a ground return. Hence, before its interpolation, the pulsation of the current was probably much more marked. A variety of circumstances had rendered this rotary converter troublesome at the time. When subsequently gotten in thorough working order, the telephonic disturbances on this grounded circuit became so slight as to cause no comment.

The 16-pole 600-kilowatt machine running at 188 revolutions per minute, from the Schenectady works, was, I am quite sure, a quarter-phase rotary, and not a three-phaser. This shows the ratio of conversion to have been in accordance with general practice.

Mr. Mordey. Mr. W. M. MORDEY: The question of "hunting," referred to by the author at page 686, is of general interest in connection with generators and motors, as well as rotatory converters. Mr. Field has mentioned the effect of eddy-currents in the poles in checking this effect. I have seen a good many rotatory converters working in America and in Switzerland in the last few months. An interesting fact that I learnt in America with regard to the hunting, or see-sawing, was that it was very considerably lessened, or even done away with altogether, by putting a short-circuited coil of copper round the field. That is a very old plan that we have many of us used for various purposes. That alone has been sufficient to remove the difficulty in some cases where the difficulty was really serious. It is of interest, perhaps, to mention it in view of what Mr. Field said as to the alternative difficulties, in one case of the loss in solid pole-pieces, in the other case of the see-sawing with laminated pole-pieces: the eddy-currents

in the solid pole-pieces were a cause of loss, but prevented the see-sawing; the laminated poles not permitting the generation of eddy-currents, but allowing see-sawing to be set up. Now, if eddy-currents have to be used for that purpose, obviously the best place to put them is outside of the poles, and not in the mass of the iron itself; so that the plan I have seen in use in the States is the right one to follow. The short-circuiting should take place where the effect will be greatest and the loss least; copper, and not iron, should be used for the conductor to carry the eddies. If this view is right, the proper way is to laminate the poles, and to put a low-resistance short-circuit round them. Mr. Mordey.

May I take advantage of the opportunity to express a hope that we shall get before this Institution—we cannot get it on this paper, I am afraid—a broad discussion on the question of two- and three-phase working? I mention the matter now, although it has only an indirect connection with the subjects we are discussing, because I think that now, when we have so little polyphase work in this country, we have an excellent opportunity of making up our minds as to the best plan—if there is a best plan—or, at any rate, of examining the whole question of whether, in introducing polyphase work, we should use the two-phase or three-phase system. There is a good deal, of course, to be said on both sides. It would be extremely interesting to have a paper and discussion on the subject.

On the question of variable ratio transformers, perhaps I am the person referred to who made the transformers with the 1 to 2 variable ratio. The transformers have a variable ratio, but they have no regulating qualities at all. As far as I know, you cannot make a self-regulating variable ratio transformer; it will not have a fixed ratio, except for one particular load.

I wish to add my tribute of admiration of the paper. Dr. Thompson has again shown that he is a master of exposition; he has given us a clear and admirable paper on a very difficult subject—a subject that we all wanted making plain—and I, for one, thank him for having done that without mathematical formulæ.

Mr. A. J. Lawson: I want to take up the subject where Mr. Lawson.

Mr. Lawson. Mr. Mordey did, and deal with the see-sawing of the current in these rotatory transformers when working in parallel, which is referred to on page 686 of the paper. Two years ago I visited the Tivoli-Rome installation with Professor Mengarini, who told me that he had had difficulties in regulation of voltage with rotatory converters (*i.e.*, single-armature machines with two windings, worked off a single-phase alternating-current system). He said he would not advise the use of such machines in sizes above 60 kilowatts, and strongly recommended, especially for tramway work, the use of motor-generators. Since then, I believe, he has put in motor-generators of large size.

Again, two or three months ago I was in Italy, and saw the Paderno-Milan installation, where three-phase generators are used at a distance of 21 miles from Milan, the current being carried by bare copper wires into that city. In the old Edison station motor-generators have been put in to transform the high-voltage three-phase current to 220 volts continuous current for feeding the Edison three-wire lighting mains, and similar machines are used to generate the 500-volt continuous current for tramway work; these machines being both of Messrs. Brown, Boveri, & Co. and the General Electric Company's manufacture. No rotatory converters are used, and it appears to me that you have a flexibility with motor-generators which you cannot obtain with converters. As the generators have been tested to 20,800 volts, and have been worked for two or three months past at a line pressure of 13,500 per phase without step-up transformers, and without their consequent losses, it is evident that these voltages can be taken into machines as well as out of them; and if motor-generators are used, the continuous-current generators, being independent, can be closely regulated, and the voltage either raised or lowered by ordinary resistances in series with the shunt winding —a matter of great importance in a lighting station.

I would like to know, if Professor Thompson has got the figures at his fingers' ends, what are the relative efficiencies and costs of motor-generators and rotatory converters of sizes of 100 or 200 kilowatts and over.

Prof. Carus-Wilson. Professor C. A. Carus-Wilson: In connection with the heating

of converters, to which Professor Thompson has drawn our attention, there is one point which should not be overlooked. It is well known that the load at which a synchronous motor falls out of step depends upon the heating of the armature. This is a different question altogether to the rating of a given machine for output. I do not allude to that, as it has been discussed already, but I want to draw attention to the fact that the torque, or load on the shaft, at which a converter will fall out of step depends upon the C^2 R loss in the armature. Prof. Carus-Wilson.

Mr. Field gave us a too brief account of an accidental experiment which he witnessed. I wish he had had time to give us more particulars. He was present on the occasion of a short-circuit on a large converter. I was not quite clear, from what he said, as to what took place. I would, however, venture to predict, if, instead of having had a converter, he had been dealing with a synchronous motor and separate direct-current generator, that under the same circumstances the synchronous motor would have fallen out of step. I should be inclined to think that the reason why the arrangement held its own under those trying conditions was that, although the current-output on the direct-current side was so large, yet the C^2 R loss in the armature was reduced in consequence of the peculiar relations of the two circuits of which we have been hearing in Professor Thompson's paper. This appears to me to be an important point in favour of the use of converters.

It appears to me that the moment of inertia of the rotating part of a converter must play a very important part in all the functions that such a machine is called upon to perform. In a motor-generator converting from direct current to direct current, by increasing the moment of inertia of the armature we can hold up the speed and the secondary volts to any required extent for any given excess of load. What we do, in fact, is to store up energy in the rotating mass that can be drawn upon in case of sudden excessive demands in the secondary.

We heard a good deal some time ago of a proposal to use motors with heavy fly-wheels, which were to be placed on tramway lines where the loads were heaviest, and which were intended to

Prof. Carus-Wilson. act as generators when the line voltage was reduced, and to equalise the load at the power house. In the use of the converter we have an excellent opportunity of effecting practically the same result; and it ought to be possible, by suitably adjusting the moment of inertia of the rotating part, not only to increase the load at which the converter would fall out of step, but to do a great deal towards equalising the load at the power house. In a case such as that of the Central London Railway, where a direct-current line is being fed by three-phase converters, the load curve ought to be free from those peaks which are such a feature in most power houses.

Incidentally, it occurs to me that the "hunting" difficulty that has been alluded to to-night might be met very largely by a similar remedy. It appears to me that, if we were to get rid of small angular variations of speed by large moments of inertia, a great deal might be done towards getting over the "hunting" difficulty.

With reference to the question of armature reaction, alluded to in the paper: When a converter is being fed from a polyhase alternating-current circuit, and delivering direct current, the effect of the primary current is two-fold. The resultant energy component of the current tends to distort the field, but has no magnetising or demagnetising effect. On the other hand, the resultant wattless component of the alternating current, being at right angles to the induced tension, tends only to magnetise or demagnetise, but cannot distort. Now it is the distortion of the magnetic field that gives rise to sparking, and hence it is only the resultant energy component of the current that we need to consider in discussing the question of reaction. Now, if we have an armature with a primary winding or windings and a secondary winding or windings, and if there is no torque loss, the resultant energy components of the currents in the two sets of windings must be equal and opposite. It follows that the resultant distorting component of the currents in the two sides of the armature must be equal and opposite—that is to say, that there is no resultant distorting action in a polyphase converter which has no torque loss. In practice, however, there always will

Prof. Carus-Wilson.

be a torque loss, and the amount of that loss will be a measure of the tendency to distort the field, and therefore a measure of the tendency to cause sparking.

I am aware that Mr. Steinmetz has arrived mathematically at the same result, but it is not necessary to follow a train of mathematical reasoning to reach this conclusion. If there is no torque loss, the resultant energy components on each side must be equal and opposite; hence the reaction in a polyphase converter depends only upon the amount of current required to overcome the torque loss in the armature. If only on this account, the polyphase converter has a great advantage over the method in which a separate motor and generator are used.

[*Communicated.*]—Professor Thompson has very properly drawn attention to the fact that the equality of the distorting reactions on both sides of a converter, to which I alluded, does not apply in a single-phase converter, since in that case it is only the mean resultant energy components that are equal. This brings into prominence the fact observed by Mr. Steinmetz in 1895—that in a polyphase system, with all branches equally loaded, the total flow of energy is constant. It is this constancy that enables us to transform from polyphase to direct current in a converter with a distorting reaction only measured by the amount of the torque loss.

I had wished to point out that, while the energy component of the current is all that need be considered when dealing with the question of reaction, the wattless component, being at right angles to the magnetisation, will act either with or against the magnet winding, and may, by its magnetising or demagnetising influence, modify considerably the action of the converter.

When transforming from alternating to direct, the magnetising influence cannot affect the speed, since the motor runs synchronously, but it will affect the current. Thus, in the example quoted by Professor Thompson on page 687, the converter there alluded to magnetises itself without any current in the shunt, but with a very large wattless current in the primary.

On the other hand, when transforming from direct to alternating, the demagnetising influence will increase the

Prof. Carus-Wilson. speed. This is well illustrated by the same converter as above. When running as a continuous-current motor and sending alternating current to an inductionless load, the absence of lag causes the wattless component to magnetise, and the speed drops from 470 to 440 revolutions per minute. When, however, the load is altered, and the current made to lag, the wattless component demagnetises, and the speed rises to 640 revolutions per minute.

Mr. Scott. Mr. E. KILBURN SCOTT: Railway work has been mentioned, and the Central London Railway in particular. With regard to this line, I doubt whether three-phase rotatory converters are really necessary at all. For instance, on the Gorner Grat and the Jungfrau Railways, in Switzerland, three-phase currents are used, not for the transmission line alone, but altogether; three-phase motors being fitted on the locomotives. On the Gorner Grat line the motors work up an incline of 20 per cent., and on the Jungfrau 25 per cent. The query has always been, Has the three-phase motor enough torque for traction work? The fact that gradients of 20 and 25 per cent. are being worked satisfactorily is, I think, a sufficient answer.

Then with regard to the question of control: Where the distances from station to station—as in a railway line—are considerable, and where one must exceed 2,000 volts to have a cheap copper line (below 2,000 volts I would use continuous currents and continuous-current converters), then I see no reason why multiphase currents should not be used throughout. On the Swiss lines above mentioned each locomotive is fitted with two three-phase motors. Writing on this matter, Mr. C. E. L. Brown says: "Referring to your further remarks as to the use of multiphase "traction on railways, I quite agree with you that in this case the "series-parallel connection—the one strong point of the continuous-"current system—is not of much importance, so that multiphase "traction could be adopted with great advantage. I see no "reason why the Central London Railway, as you say, might not "have been worked directly by three-phase current, dispensing "with rotary converters altogether."

Mr Thomas. Mr. EUSTACE THOMAS: There has been a good deal said this

Mr. Thomas.

evening about single-phase and three-phase converters, and also about the question of "hunting."

In one or two cases it is said that converters have been taken out and replaced by motor-generators. I think in some of these it will be found that single-phase converters were used, rather than three-phase or quarter-phase.

There is a very great difference in one important respect. In three-phase or quarter-phase machines the torque is quite uniform; but this is, of necessity, not the case with single-phase currents, and the varying torque in the latter case is more likely to give trouble.

As Dr. Thompson has pointed out, there is a great deal of difference in the heating also. I have in mind the figures for the efficiencies and heating by a rotary converter which was first of all connected three-phase, and then, by separate slip-rings, was tested as a single-phase machine.

It was found that with single-phase the total power which could be taken out for a given amount of heating was only 55 or 58 per cent. of that which could be taken from it as a three-phase converter.

The efficiencies at full load were 92 per cent. three-phase, and 75 per cent. single-phase.

These are matters of actual test, and accentuate the results of calculation, as stated by Professor Thompson.

Then, too, there seems to be an idea in the minds of several of the speakers to-night that the hunting of rotary converters is an evil which it is very difficult, or impossible, to get over. I happen to know many cases where rotary converters have been employed, and have seen a good many in use, sometimes working four or five together in parallel, and giving the greatest possible satisfaction. Of course nobody denies there has been trouble from hunting; yet it must not be forgotten that at the present day it is perfectly possible to eliminate it. Indeed, in Mr. Hobart's communication to the Society, he pointed out that in two or three cases where hunting had been observed, he had found no difficulty in getting rid of it. This is true in any properly designed rotary converter with suitably driven generators.

Mr. Thomas. The reasons of the hunting are understood; but at this hour it is not possible to go into that farther.

The question of the armature reaction of rotary converters has come up this evening. There is practically no resultant armature reaction in a three-phase converter when it is adjusted for minimum current intake, and when the losses are comparatively small; that is to say, the reaction of the three-phase currents balances and overlies that of the continuous currents. But as soon as the field is over- or under-excited the three-phase currents lag or lead, and at once produce armature reaction.

In the practical working of rotary converters, in order to maintain the continuous-current voltage constant, it is necessary that there should be in the circuit a sufficient amount of reactance; otherwise, the current will lag or lead at the different loads to a considerable amount, through the use of heavy series compounding coils. This in practice presents no difficulty; and the power-factor can be made unity at three-quarter or full load with no great variation within the usual working limits. For example, after careful consideration, it was decided not long since that a very large proportion of the street railways in New York should be operated electrically, by rotary converters and three-phase transmission. There will be some half-dozen or more generators, each of 3,500 kilowatts; these generators are in course of construction at the present time.

Distribution will take place at high voltage to rotary converters in sub-stations.

From calculations made on the system, based upon experience, it has been found that without regulation of the field rheostats of the generators, for a given number of rotary converters and generators in circuit, the load may vary from nothing up to considerable overload without the continuous-current voltage varying more than by some 2 or 4 per cent. Indeed, even if there were a considerable amount of variation in the proportion of rotary converters in circuit, the amount of variation in the continuous-current voltage without touching the field rheostats on the generators would be extremely small. The armature reaction of the compounded rotary converters becomes reflected,

so to speak, on to the generators, and the voltage of these does not show the same variations as would be produced with non-inductive load.

The armature reaction of rotary converters is rather an interesting one to me also in connection with sparking on the commutator side.

The brushes, as Dr. Thompson says, are almost always fixed dead at the neutral point—in some cases only, a slight lead is given.

The field remains practically unaltered over the pole-tips at all loads. The fact that under these circumstances good commutation can be obtained points very strongly to the insufficiency of the old theory, that one had to depend upon a reversing field whose strength must vary according to the load on the armature.

The rotary converter and the railway motor show clearly that sparkless commutation must be obtained from some quite different cause. There is not time to go further into the matter at the present moment, but I could not refrain from saying a word on it in passing.

Something has been said also in comparison of three-phase and quarter-phase systems. While both systems will give equally uniform turning moment on the armature of the rotary converter, there is a strong advantage in favour of three-phase in the transmission lines. For equal amounts of energy transmitted only three-fourths as much copper is necessary in three-phase circuits as in quarter-phase, for equal maximum voltage between lines; and, moreover, only three conductors have to be insulated instead of four. This would mean, of course, that, if the installation is very extensive, the total cost of the system would be correspondingly diminished.

Sir Henry Mance.

Sir HENRY MANCE [*communicated*]: No one could have listened to the lucid paper of Professor Thompson without coming to the conclusion that rotatory converters are likely in the near future to find extended application for the transmission of energy over considerable distances. The paper is one of the most valuable contributed to the Institution in recent years.

I must confess to feeling a certain amount of disappointment

Sir Henry Mance.

on finding so few references to English practice. That more has not been done in this country is not so much due to the inertness of English engineers, as to the limited demand which has hitherto existed for such a system; we have been well satisfied with the results which have attended the use in this country of the continuous-current transformer. The subject, however, has not been altogether neglected. Professor Thompson refers to Mr. Heldt's description, in the *Electrical World* of July, 1896, of an ingenious method of ensuring a convenient ratio between the voltages on the alternate-current and direct-current sides. This method was described very clearly a year earlier in this country in the specification of Blackburn and Spence (No. 11,153 of 1895), which provides for any desired variation in the ratio.

The methods of regulation practised in the United States have been either—

(*a*) Compounding of the field;

(*b*) The use of an auto-converter (*i.e.*, a static reducing transformer with variable secondary turns) to supply a variable E.M.F. on the slip-rings;

(*c*) The use of an induction booster or reducer with variable magnetic circuit, also for the purpose of supplying a variable E.M.F. on the slip-rings.

I suggest that these methods are not capable of giving any considerable range of variation without reduction in the plant efficiency, the first and third being open to objection on account of the lagging or leading currents to which they give rise; whereas in the above-named patent the field strength of the rotary proper may always be exactly that required for the minimum line current corresponding to the actual load, as in a synchronising motor: the arrangement is essentially a combination of the "Delta" and "Star" couplings, the former being in the main field, and the latter only in the auxiliary or regulating field.

The following particulars of a converter manufactured by the Electric Construction Company early this year for a colliery in Yorkshire may be interesting.

It is a six-pole machine, designed to convert 500 volts, 90

amperes, continuous current into three-phase current at a frequency of 42 periods. The armature is 24 inches external, 17 inches internal diameter, by 6 inches long, having 116 slots with 696 conductors connected to 116 commutator bars; the brushes are 180 degrees apart; the armature resistance is 0·1 ohm, and the magnet resistance 260 ohms; it runs at 840 revolutions, and the full-load efficiency is 90 per cent. After six hours' run at full load the temperature rise was—Magnets, 35; and armature, 40 degrees. Sir Henry Mance.

On page 683 there is a note referring to the square inches of peripheral surface per kilowatt. This expression can hardly be taken as showing the specific utilisation of materials, as no account is taken of peripheral speed or frequency. If we compare two machines identical, except that one has twice as many poles as the other, with the same armature surface the higher frequency machine, working at the same air gap density, would get considerably hotter than the low-frequency machine. The higher frequency demands a longer armature, but having less radial depth, the weight of core being less. The output of machines also varies as the square of the armature diameter; thus large machines appear on too favourable a basis.

Mr. H. F. PARSHALL [*communicated*]: I received, while in the United States, from Mr. Hobart, a copy of Dr. Silvanus Thompson's interesting paper on "Rotary Converters." Mr. Parshall.

The Institution is fortunate in having this matter so thoroughly presented at this time. Engineers are largely inclining to the belief that the distribution of electrical energy by continuous currents is to be preferred, while in the transmission of electricity alternating currents are necessary.

The most efficient method of transformation is by the rotary converter. Thus it becomes one of the most important elements in electric transmission. In general, the theory of the rotary converter has been touched upon by a number of writers. In a paper which I read before the Institution of Civil Engineers on the "Dublin Electric Tramways," I gave a general statement as to the theory of multiphase rotary converters. Dr. Thompson, however, is the first systematically to elaborate and bring the matter down to date.

Mr. Parshall. The rotary converter is different from all other classes of rotating electrical machines, in that the dynamic action between the field and the armature is normally determined by the internal losses, and is not proportional to the current-output of the armature.

While in the United States a considerable portion of my time was spent with the engineers of the General Electric Company in making various experiments on rotary converters. These experiments were aimed to determine the limiting conditions as to transmission drop and variation in angular velocity: the rotary converter could be relied upon to give satisfactory results.

It has been noticed in many installations that the E.M.F. of the rotary converter varies in an irregular and uncontrollable way, and that in some aggravated cases the sparking at the commutator has been so serious as to make the use of such machines troublesome. This can hardly be attributed to the peculiarity of design, although the first large rotaries that were constructed were designed with constants identical with those of the best commutating dynamos. Since then practice has changed considerably, and rotaries have been built tending more towards the induction rotary converter—I mean a rotary converter with very small magnetising force in the field magnets, with small air gap, and with low intensity of magnetisation in the air gap. Such machines, especially when constructed with laminated poles, have been found to give most trouble from inconstancy of voltage, and from sparking. The term popularly applied to these combined effects is known as "surging." These phenomena may be primarily studied conveniently by considering the action of a single rotary converter working on a variable load, or when being driven by a machine having an irregular angular velocity. In the normal case—that is, with the rotary converter running at constant angular velocity and constant phase angle—the force between the field and the armature is that due to the internal losses of the machine, and the armature reaction is *nil*. When, however, the armature is caused to revolve at variable angular velocity, the force between the armature and the magnets is increased by the rate of acceleration or retardation from constant angular velocity. The armature reaction becomes considerable, the E.M.F. becomes variable, and

a sweeping flux is set up in either or both the forward and leading poles, which is objectionable from the standpoint of sparking, since the brushes cannot be shifted to correspond with the resultant magnetic field. Mr. Parshall.

The above constitutes the simplest case for consideration, and it is obvious that the variation in speed from constant angular velocity may be brought about by variation in phase of the current, or by variation in the angular velocity of the prime mover. Undoubtedly, variation in the angular velocity of the prime mover causes variation in the phase between the current and E.M.F., so that both of these in the ordinary case would change together exactly as in a synchronous motor.

The next case that may be considered is that of two rotary converters running in parallel. The additional condition is in respect to the magnetic similarity of the action of the machines: the brushes must be set so that the machines are similar in the armature reaction; otherwise, with a change of load, the machines will not run in parallel. The machines should have the same characteristic curve; otherwise, with change of load, the E.M.F.'s will be different, and large variation in the E.M.F. and serious sparking are liable to occur. The field magnets should, as largely as possible, be similar magnetically, so that the same armature reaction produces the same results as to leakage, &c. I have found that machines that are apparently identical would not run in parallel until adjustments have been made, so that the reluctance of the magnetic circuits was the same. It is apparent from this that shunt-wound rotaries can, in some cases, be operated in parallel with less difficulty than compound-wound rotaries. This is the case, and frequently, in practice, the use of compound coils has been abandoned.

The next case is that of a system in which a number of sub-stations feed into a common system. In this case the difficulties are greatest, and they increase with the voltage drop in the transmitting line. This becomes apparent when we consider that the ratio of transformation for a great range of phase angle is constant, consequently machines feeding into a constant-voltage system become unstable when the difference in

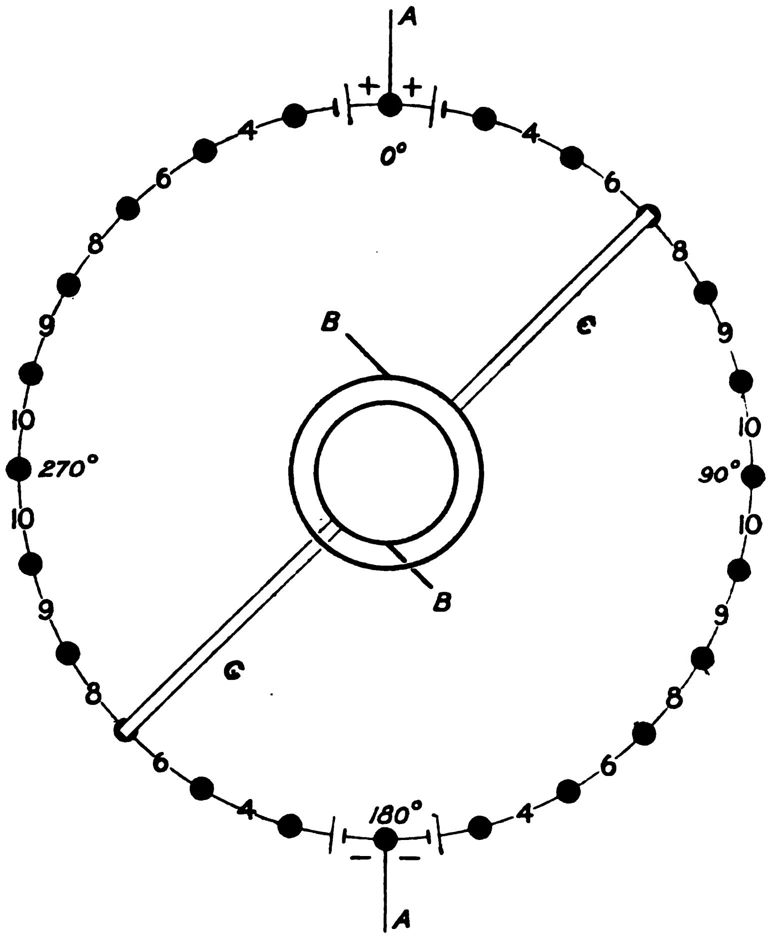
Mr. Parshall. drop in the line feeding the various sub-stations becomes considerable. In a large installation which I have recently designed, I have limited the maximum difference in the voltage drop in the transmitting lines to the various stations to 2½ per cent.

The conditions in this last case become exceedingly complicated in the case of unequal angular velocity in the prime mover, or considerable drop in the transmitting line. The combination of these two elements in the case of a variable load, in many cases makes the use of rotary converters impracticable without considerable diminution in efficiency.

I have recently been testing some 900-kilowatt rotary converters running on an artificial line driven by an engine of variable angular velocity. When the reactance between the machines and the transmitting lines was kept within certain limits, the machines worked perfectly well, both as regards constancy of voltage and sparking. Load could be thrown on or off either of the machines, and the current could be made either a lagging or a leading one to as much as 60° without causing the machines to become unstable either as to E.M.F. or sparking. Considerable adjustment in the so-called copper bridges or connecting pieces between pole-pieces is required to secure this result without sacrificing too much in efficiency. The ultimate result of the test was that these machines could be run perfectly satisfactorily with a reactance between them of about 3 per cent. of the transmission voltage. At the beginning of the experiments the losses induced in the steadying arrangements amounted to some 3 per cent. It was found, however, on making a very careful study of the phenomena, that the added losses could be brought down to about six-tenths per cent., which left the efficiency of the machine, as a whole, at 95 per cent.

In general, it has been found that rotary converters run better with lagging than with leading currents. This is to be expected, since with lagging currents the algebraical sum of the E.M.F.'s acting in the machine is greater than with leading currents, which condition is more favourable for steadying the armature—that is, in keeping the armature from falling back or leaving its proper position in rotation.

Professor E. WILSON [*communicated*]: The Institution of Electrical Engineers is greatly indebted to Dr. Thompson for his valuable paper. The subject of rotary converters is one of great importance, and not generally understood. Dr. Thompson is to be congratulated on bringing forward the right paper at the right Prof. Wilson.



time. I have found the following analogy useful in *teaching*, as it impresses upon the student several points of importance in direct- and alternate-current machinery. In the figure divide up a circle into any number of equal parts—24 parts have been chosen, as in Dr. Thompson's own diagrams—and suppose that a

Prof. Wilson. bipolar single-phase rotary converter has to be dealt with. Suppose a sine distribution circumferentially has to be imitated: then place in each space, say storage cells, in numbers approximately proportional to the sine of the mean angle—these are 1, 4, 6, 8, 9, 10, 10, 9, 8, 6, 4, 1—in one series, and a similar number in the other series, the + poles being connected together at 0° and the − poles at 180°. Let conductors A, A, be attached to the points at 0° and 180°, as shown. Now suppose the brushes C, C, fixed to two slip-rings capable of rotation about the centre: they will in turn make contact with the junctions between the several groups of cells, and the rubbing brushes B, B, can be connected to an outside circuit. If the cells discharge between B, B, through, say, a constant resistance, then the current depends upon the net potential difference between the brushes. For instance, at 45° the net potential difference is that due to 54 cells; the maximum ordinate is that due to 76. If the number of cells exactly represented a sine distribution, the ordinate at 45° should be $76 \sin 45° = 53{\cdot}7$. Obviously any other distribution can be represented. Suppose power be transmitted to this system by alternate currents, then the brushes and slip-rings must rotate at the frequency of supply, in which case the cells get charged; but those near angle 0° are charged on the whole to a less extent than those at 90°, the ratio being about 0·12. On the other hand, suppose, as in a rotary converter, energy is transmitted to the system by direct currents at A, A, and delivered by alternate currents at B, B, or *vice versâ;* on the average, cells will be discharged over one portion of the half-circle, and charged over the other portion—giving the idea of positive and negative accelerations. It is only necessary to apply Kirchoff's law that the algebraic sum of currents at any point in a network of conductors is zero, in order to deduce the direction and magnitude of currents for any position of the brushes. Another point to be impressed is that in Dr. Thompson's Fig. 1, for instance, the E.M.F. in the loops as they pass a given point on the pole-piece is the same for a given field and given speed of rotation, whether the machine be motor or generator, and the analogy above shows this. Obviously any number of phases can be dealt

with by suitably providing slip-rings and brushes at the correct angles. Prof. Wilson.

The subject of induced currents in the pole-pieces of alternators was dealt with at considerable length in a series of experiments carried out in the Siemens Laboratory, King's College, London.* These experiments would seem to have importance in connection with rotary converters. The oscillations set up in the current in the magnet winding of a Siemens alternator were traced out, and, as predicted by theory, had a periodic time half that of the alternator. These oscillations were stopped in the magnet winding, but still th machine gave practically the same electro-motive force, with the same non-inductive resistance in its external circuit. An attempt was then made to exaggerate the effects, by placing sheet-copper rings between the armature and pole-pieces. These copper plates did not make much difference in the electro-motive force actually observed when they were absent. It is a question where the energy dissipated by these currents is best got rid of. If the poles be laminated, the currents in them will be in great measure suppressed. I believe Mr. Parshall† intentionally makes the flanges of his bobbins of solid gun metal, and laminates the pole-pieces, in order that the radiation due to such exposed surface may be better able to get rid of the heat caused by these currents, and at the same time securing stability.

It is well known* that if two alternate-current machines of the same size be run, the one a generator driving the other as a synchronous motor, and that if the machines be equally excited, the current between them leads the potential difference. As the motor excitation is decreased, the generator excitation being kept constant, the curves of current and potential difference come into phase; and on further decreasing the motor excitation, the current lags behind the potential difference. This is an important point, as it affects economy of working, and a very interesting question is how phase difference between current and

* See *Phil Trans. Roy. Soc.*, vol. clxxxvii. (1896), A, pp. 229–252.

† See *Proc. Inst. of Civil Engineers*, vol. cxxvi., p. 220.

Prof. Wilson. potential difference will be affected by variation of excitation in a rotary converter.

Mr. Sayers. Mr. W. B. SAYERS [*communicated*]: The Institution is greatly indebted to Professor Thompson for his very lucid and valuable paper on this subject. One factor which has a most important bearing on the practical working of converters (as defined by Professor Thompson) has been entirely neglected in his treatment of the subject. I refer to the energy stored in the rotating part of the converter. If we take the case of the 10-kilowatt two-phase converter instanced by Professor Thompson, and suppose it to be fed on the continuous side from a 100-volt supply, and suppose that not only are the resistance and inductance of the armature and circuits negligibly small, but that the *inertia* of the armature is also negligibly small, it is clear that the armature will run at a constant speed, and that the power taken on the continuous side will correspond at any instant with that given out on the alternate side—which means that the supply current, though unidirectional, will undulate between zero and a value equal to the maximum on the alternate-current side. On the other hand, if we suppose the conversion to be from alternate to continuous (or, more properly speaking, *unidirectional*), then the angular velocity of the armature will vary between zero and a maximum, and the voltage and current on the commutated side will vary correspondingly between zero and a maximum.

Referring now to Professor Thompson's first set of diagrams (page 663), we observe, as he points out, that in the position called 0° the armature is acting wholly as a motor, with 50 amperes flowing each way. Then passing to position 90° it is acting wholly as a generator, also with 50 amperes flowing each way. In both cases the armature is supposed to be generating 100 volts, therefore the speed must be the same; and it is evident that if this is to be so we must add to the assumption already made another, namely, that the inertia of the armature approaches infinity.

If we allow that the armature has some resistance, we find that the instantaneous velocity when running as a generator must be actually greater than when running as a motor, for in

one case the E.M.F. must be $100 + C_{ra}$, and in the other Mr. Sayers. $100 - C_{ra}$. The conditions indicated in the diagrams relative to the current-distribution in a two-phase converter are therefore impossible ones, and I conclude that the conditions in practice differ very materially from that represented.

If we take a rotary transformer (continuous-current on both sides) and drive it from a constant voltage supply, we know that the power taken on the driving side will correspond with that taken on the generator side, plus the sum of the various losses. Suppose now that we contrive to throw the load on and off the generator side at shorter and shorter intervals, there will come a time when the driving current will cease to fluctuate in correspondence with the load, and will tend more and more towards uniformity as the rapidity of make and break is increased, depending, of course, upon the time interval of make and break and the *vis inertia* of the rotating armature.

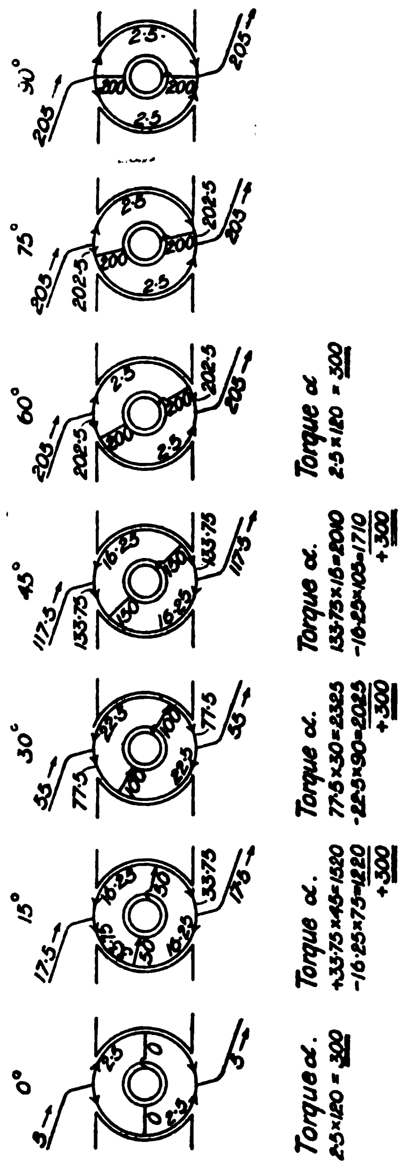
Following out these considerations, I have plotted out the form of the current-wave on the continuous side, on the supposition that the power input corresponds at any instant with the power output, minus an allowance of 5 per cent. for losses.

I take the pole angle as 120°, and assume an even distribution of field over the pole surfaces.

With the current-distribution shown in my diagrams there is a constant excess of positive over negative torque—in marked contrast to the extreme fluctuations which Professor Thompson's two-phase diagrams depict. Speaking for the moment regardless of armature reactions and inductance, the true distribution will lie somewhere between that shown in Professor Thompson's diagrams and my own.

I suggest, therefore, that, in considering the current-distribution in converters, two sets of diagrams should be made—one assuming an unvarying current on the continuous side, and the other a unidirectional current, but varying so that during a short interval, dt, the power represented on the alternating and continuous sides differs only by the average rate of waste. Such a pair of diagrams would show the two extremes between which the truth lies.

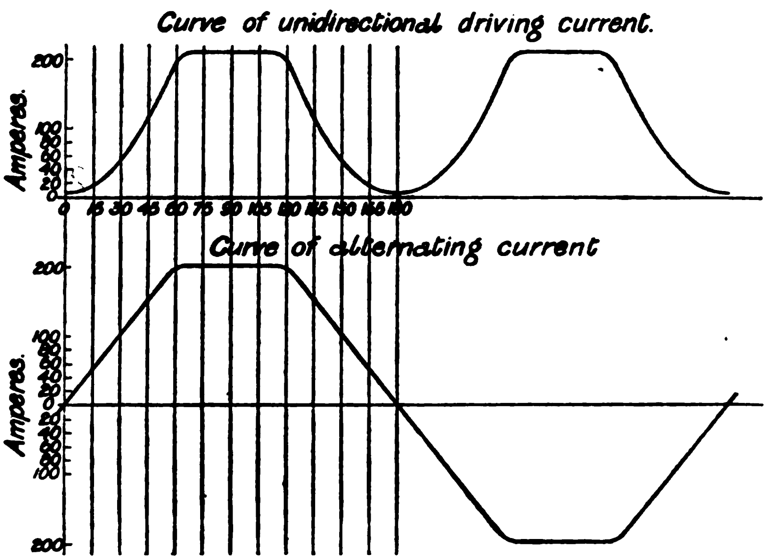
Mr. Sayers. Professor Thompson mentions the fact that the continuous current fluctuates in practice, and also that it does so much more



in the case of single than in multiphase converters, and no doubt

his diagrams of distribution in the case of three-phase differ less from the truth than do those for single-phase. Mr. Sayers.

As regards armature reaction, I have maintained for some years past, both from theoretical considerations and also from experience with machines wound on my principle, that when a continuous-current machine is commutated on the neutral line the total effective field is unaffected by armature reaction, and I was, therefore, gratified to see in a recent instalment of Mr. Parshall's articles in *Engineering* that he has found on experiment that this is the case; but I am of the opinion that in the case of



converters armature reactions *will* affect the value of the total field, and consequently the speed or voltage, or both, but that this will be due, not to any current in conductors under the field poles, but to the current in those conductors which lie between the poles.

Mr. H. L. Mills [*communicated*]: The question has been raised in the course of the discussion, with reference to the nomenclature used in the paper, whether the use of the word "rotary" is admissible. Mr Mills

This word is to be found in the following dictionaries of the

Mr. Mills. English language:—J. Walker (1824), Dr. S. Johnson (1828), Webster (1890), Lloyd (1895), and Nuttall (1897); but in the first two of these the word "rotatory" does not occur.

It appears from the above authorities, therefore, that the word "rotary" is without doubt correct English, and is probably of earlier date than "rotatory."

Prof. Thompson. Professor SILVANUS THOMPSON, in reply, said: I am much obliged to Mr. Thomas for the very valuable information he has just given to us about the behaviour of converters under certain conditions. I am not disposed to question Mr. Scott's proposition that it might have been an advantage on the Central London Railway to do without any converters, and to work the whole thing three-phase; but that is not the point we are discussing, nor of my paper. As long as tramways are worked with continuous-current motors and fed from three-phase generators,—as long as town supplies are laid down with continuous currents while factories are equipped with three-phase motors,—so long will converters be required.

Professor Carus-Wilson has drawn our attention to several points, including one of very great importance that I had not emphasised, viz., that the limiting load of a converter (as also that of a synchronous motor) will depend upon the heating of the armature. He also emphasised the fact that the moment of inertia in an armature is of importance. I think it is of importance in the case of the single-phase converter, but doubt whether it comes in very much in the case of the two-phase or the three-phase converter, for the simple reason that as motors their torques are so uniform. The argument that Mr. Steinmetz put forward mathematically some two years ago, and which has been put forward in simpler form by Mr. Carus-Wilson to-night, is quite right as far as it goes. In Mr. Carus-Wilson's simpler form, it is this: If there is no frictional or hysteresis loss or eddy-current losses in an armature—if, in fact, these losses are negligible—the two torques must be equal, and therefore the distortions will wipe one another out. That will be literally so, provided the ampere-turns of the two sets of circulations of currents were at every instant equal. I should like to

amend his statement. If there are no losses, the *average* torques must be equal. But they are not equal in the case of the single-phase machine. Though they are equal on the average, yet they are varying from instant to instant, because one has sometimes a very large current in the alternating-current side in excess of the continuous-current side, and sometimes a less amount. There certainly will be armature reactions in that case from instant to instant. In fact, as I showed in my paper, there will be in every single-phase converter armature reactions of double the frequency of the alternating current that is being supplied or given out on the single-phase side. Mr. Lawson referred us to a very important case—the transmission from Rome to Tivoli—where converters were not used, but where motor-generators were employed to feed the tramways. Well, in the first place, the alternating current generated at Tivoli is single-phase, and single-phase converters are admittedly bad—that is to say, not so good as two-phase or three-phase—on account of the armature reactions and the trouble from sparking. It is rather a curious point—I do not want to say it to the detriment of the admirable and eminent firm who supplied the machinery which is there—that they have made, not one, but several different designs of motor-generators before they were satisfied with their final type. Therefore, it does not seem to me necessarily a fact that, if motor-generators have been preferred in Rome for the transmission to Tivoli, that preference has any serious bearing on the case. Probably, if the whole station had to be fitted over again *ab initio*, they would put in a two-phase or three-phase at Tivoli, and use converters when the current reached Rome. Prof. Thompson.

Mr. LAWSON: They are putting it in now, Sir.

Professor THOMPSON: Mr. Mordey has referred to the advantage of using short-circuited copper winding round the poles—what we should call, in other words (following Hutin and Leblanc), an *ammortisseur*. Certainly that device is of great advantage in some cases, not for converters only, but for alternating-current machines of certain types when running in parallel. It distinctly stiffens the magnetic field and makes it more "bristly"—if I may adopt Mr. Field's term.

Prof. Thompson.

Mr. Hobart has given us a most interesting contribution to the discussion. I do not want to dispute at all the emphasis which he puts upon the necessity of having the engine power regular if machines are to keep in step, for, remember, the engine power transforms itself from the generator through the line, and through the stationary transformers, if there are any, right into the rotatory converters; and if the engine power that turns the generators is non-uniform, then the alternating current that comes into that converter will not be delivered with absolute equability of period. That may be one reason why the converters, good in themselves, have not run more satisfactorily in parallel with other plant; the steam engines at the far end being really responsible. Incidentally, I must quarrel with Mr. Hobart on one detail. For the first time in this room he has introduced the adjective "static" for something which has nothing static about it, namely, ordinary common-place alternate-current transformers. They are *stationary*, but they are not *static* as we electricians understand the word "static." A static transformer would be simply a condenser. Every condenser is a static transformer, and you can put currents into a group of condensers at any voltage, and take them out if you like at a different voltage by having a different number of them in series or in parallel. Planté's "rheostatic machine" is an example of a true static transformer. We ought not to use an adjective like "static" for anything but its proper meaning. I wish I could only induce Professor Carus-Wilson to adopt the language I have used, and use the word "transformer" only for a thing that stands still, and the word "converter" for a thing that revolves. Then I think we should not require any adjectives in front like "static" or "stationary" or "rotatory." Mr. Hobart has referred to the great advantage there is in using a six-phase converter, as was pointed out by Kapp and by Steinmetz. The six-phase converter certainly is an admirable thing, and, as he remarks, it can be used so very easily in those cases where a stationary transformer is used first to step down from the high-voltage transmission.

Mr. Esson put to me as a query the point whether the efficiency of a converter in doing its work was not increased in proportion to the number of connections that were made from the

armature winding down to the slip-rings. Yes, that is so; in efficiency a two-phase converter (or four-phase, as it really ought to be called, having four slip-rings) is better than a three-phase, and a three-phase better than one which has only two slip-rings; and a six-phase converter is better still. For a given amount of power transmitted through it, it has less heating; or, for a given limit of heating, it may be used to transmit a larger amount of power. Prof. Thompson.

Mr. Field has given some additional data about the machines that he has had experience with. I do not think I have anything to say upon his remarks, except that the problem of changing the voltage ratio by using a leaky transformer seems to me rather a lame solution. One would rather do it in some other way.

Lastly, let me protest against the insinuation that in using the good old adjective "rotatory" I have altered the English language. The old English language has many adjectives like "rotatory," "explanatory," "inflammatory," "sanatory," and "undulatory," but I do not think any of those adjectives would be improved by cutting out what might seem an unnecessary syllable. I think such clipping would not add either dignity or literary—perhaps I ought to say "litary"—form to any communication that might be written in such an abbreviated language.

[*Communicated in writing.*]—My thanks are due to Sir Henry Mance for the additional information respecting the converter constructed at Wolverhampton. I think he has not understood the object of comparing together the square inches of armature surface and the kilowatts of output of the machine. I stated the ratio of these two quantities as giving an indication of the specific utilisation of material in the machine. He objects that I leave out of sight such questions as the peripheral velocity. Surely no; for, assuming current and magnetic flux to be maintained constant, the electro-motive force increases with the peripheral speed, and therefore the kilowatts of output increase also, while the heating of the copper remains unchanged. In that case the ratio of kilowatts to armature surface is higher; and there is a better specific utilisation of the materials. The

Prof. Thompson. output is not proportional to the square of the diameter, at the same peripheral speed, unless the armature length is increased in the corresponding ratio.

Mr. Parshall has alluded to the difficulties found in operating converters in parallel when the supply of current is of a varying frequency owing to unequable angular velocity in the engine which drives the generator. The converters ought not to be condemned for a fault that arises from a defect in the steam engine. It is well to know how to adapt the converters so as to run stably, even under conditions like this; but probably a fly-wheel of greater moment of inertia on the engine-shaft would prove a good remedy.

My thanks are due to Professor Wilson for the useful analogy which he has suggested.

Mr. Sayers has communicated some remarks, the first part of which I do not comprehend, unless he is using the term "two-phase" for that which I have called single-phase. Even then there seems to be some confusion, for he says that if the armature runs at a constant speed "the supply current, though unidirectional, "will undulate between zero and a value equal to the maximum on "the alternate-current side." I think he overlooks, in the argument that the intake and output of electric power are (if the losses are negligibly small) equal, the circumstance that with a very ordinary amount of moment of inertia a very small change in the angular speed will correspond to the difference between ths instantaneous values of the power given to and given out by the armature. In any ordinary alternator the instantaneous values of the power given out electrically vary between zero and a maximum twice in each period, and yet, as driven by an engine, the speed of revolution is sensibly uniform. So is it also when the armature, instead of being driven by a steam engine, is being driven by superimposed motor currents flowing in from the continuous-current supply, and flowing in against a nearly equable counter-electro-motive force. Again, it is not true that if the conversion is to be from the alternate-current to the continuous (or unidirectional) side "the angular velocity will vary between "zero and a maximum." The confusion here is between angular

velocity and angular acceleration. The latter will undoubtedly vary from instant to instant; but with a reasonable amount of moment of inertia such as ordinary armatures have, the angular velocity will be sensibly constant. [Prof. Thompson]

Again, if there is some resistance in the armature, it is certain that the voltage at the brushes will not be exactly 100 volts (for same speed and field), but will be a little less when working as continuous-current generator, and must be made a little more when running as continuous-current motor. I touched on this point on p. 660, but did not suppose that it needed further elaboration to make my meaning plain.

As to Mr. Sayers's diagrams for the supposed case of constant excess of positive over negative torque, they not only do not correspond to the known facts, but are wholly unnecessary. To produce in an armature a practically constant speed, it is not necessary to have a constant difference of torque. There is here the same confusion between speed and acceleration.

I have received since the reading of my paper a letter from Mr. Steinmetz, in which he points out a slip on p. 684. It appears that the four 900-kilowatt converters built for the Central London Railway were designed by the engineers of the General Electric Company at Schenectady.

Mr. Kolben, of Prag, also writes to inform me that he has found that permutators built on the principles of MM. Hutin and Leblanc for converting three-phase into continuous currents give excellent results. One of these has been in constant use in the factory of Messrs. Kolben & Co. at Prag for 18 months for the purpose of charging accumulators directly from the three-phase plant. There is no sign of sparking at the brushes (which are fixed) even with a varying voltage. Its efficiency is at least 95 per cent., including allowance for losses in the stationary transformers, and its cost and maintenance is, he says, much smaller than that of a rotatory converter.

The PRESIDENT: I have often felt, when being driven through the busy streets of London by a hansom cabman, envious of the skill with which he pilots me through the throng, and I have felt something of the same feeling of envy in listening to the splendid, [The President.]

The President.

lucid way in which Professor Thompson has dealt with this most intricate subject. I think we have never had a communication presented to us more lucidly or with more beautifully clear illustrative diagrams. The subject is an exceedingly important one, and it has been presented in a very adequate manner. I feel that it will be your wish to give expression to your feeling of thanks to Professor Thompson, and I ask you to do so in the usual manner.

Carried with acclamation.

The PRESIDENT: I have to announce that the scrutineers report the following candidates to have been duly elected:—

Foreign Member:

Alfred Schaar.

Members:

Baganna Balaji.
John Ray Cowell.
William Edwin James Heenan
Guglielmo Marconi.

Associates:

R. Whitfield Alcock.
Walter A. Amesbury.
Durgadass Banerji, M.A.
John Dalrymple Bell.
Harold Bentham.
Arthur Bentley.
Arthur Herbert Blagden.
F. Bourne.
Christopher Buckton.
Cecil Burman Callow.
John McIlvaine Cater.
Edward George Clark, A. Inst. C.E.
George Douglas Collins.
Cecil Bertrand Davies.
Arthur Henry Dowson.
James Stephen Enright.
T. W. Foinette.
Robert J. N. Franki.
James Harrington.
Walter Jamieson.
Thomas H. Kingscote.
Vernon Lindop.
Pramathanath Mallik, B.A.
Charles Thomas Mitchell.
E. Moorhouse.
Frederick Edward Nosworthy.
S. L. Pearce.
William Mair Rolph.
Tom Rowe.
William H. Smith.
Herbert Stansfield, B.Sc.
Edwin A. Uttley.
Harry Reginald Wickins.
Robert Salisbury Williams.
Noel Woodhouse.

Ernest Henry Wright.

Students:

Edgar Arthur Adey.
Wm. Richard Colville Barrington.
Herbert Greenwood Beeton.
Frederick Thomas Bersey.
Arthur Buckney.
Frederic Horton Clough.
Hilary Henry Dadson.
Harold Frederick Dell.
Lewis Blackburn Draper.
Percy Freudemacher.
Percy J. C. Eve.
Percy Arthur Fisher.
Bertram Douglas Fox, B.A. (Cantab.).
Mitchell Hickman.
George Hicks.
James William Holliday.
Sydney R. Inch.
Herbert Johnson.
Henry Joseph.
Walter James Linford.
Leslie Robert Morshead.
Sidney Ransom.
Emile Rasecki-Morton.
Augustus Reckenzaun.
Leonard Redmayne.
Charles Gage Seeley.
James C. Smail.
Francis Stacey.
George Albert Tatchell.
Sydney John Temple.
Edmund D. Tiddeman.
Reginald N. Torpy.
Leonard Charles Boughey Trimnell.
William T. Trussler.
John H. West.
Charles William Wood.
Percy John Woodward.
Ernest Vanderpoel Young.

The meeting then adjourned.

The Three Hundred and Twenty-second Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, December 15th, 1898—Mr. JOSEPH SWAN, F.R.S., President, in the Chair.

The minutes of the Ordinary General Meeting held on December 8th, 1898, were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council:—

From the class of Associates to that of Members—

Walter Poynter Adams.
Leonard Andrews.
Frank Boulton Aspinall.
Darwin Bates.
Alfred Charles Brown.
Frederick Brown.
Richard Alexander Chattock.
William Henry Collis.
John Dewar Cormack.
Charles W. S. Crawley.
Frank Gill.
Cecil Wilberforce Gwyther.
Bernard Maxwell Jenkin.
Samuel Joyce.
John Edward Kingsbury.
John Douglas Knight.
Andreas Peter Lundberg.
John Macfee.
Mervyn J. P. O'Gorman.
Charles John Phillips.
Robert Cornelius Quin.
William Gould Rhodes.
Thomas Joseph Rorke.
Alexander Gilbert Sanders.
Arthur William Sclater.
Frederic Smith.
James William Speight.
J. C. M. Stanton.
Ernest George Tidd.
John Michell Grylls Trezise.
Thomas Percival Wilmshurst.

From the class of Students to that of Associates—

Comer Sandys Ball.
Frank William Bowden.
William Leonard Carter.
Charles James Cunningham.
John Muir Donaldson.
Herbert Douglas Hodges.
J. H. McDowell.
Alfred Mitchell-Withers.
Austin Henry Peake.
Claude Edward Vance.

It was announced that since the last meeting Mr. Henry Edmunds, Member of Council, had presented to the Institution a handsome lantern, with oxy-hydrogen gas jets and other accessories, for the use of the Students' Section; and the thanks of the meeting were duly accorded to him.

Mr. Howard Tasker and Mr. C. F. Davis were appointed scrutineers of the ballot for new members.

ELECTRIC INTERCOMMUNICATION IN RAILWAY TRAINS.

By W. E. LANGDON, Vice-President.

The first practical means of communication between the passengers, guards, and the driver of a railway train was that established by Mr. W. H. Preece, C.B., F.R.S., Past-President, on the London and South Western Railway in 1864. The agency employed was electricity. Mr. Langdon.

The need for some means of communication between the passengers and the officials in charge of railway trains had been long felt. The Railway Commissioners, as well as the Board of Trade, urged its desirability. On two occasions it formed the subject of inquiry by committees of the House of Commons; and prior to 1866 no less than three committees composed of the general managers of the various railways had specially considered the subject. In 1865 the Board of Trade instructed Captain (now Sir Henry) Tyler, then one of the inspecting officers for the Railway Department of the Board of Trade, to examine and report upon the question. His report, which was issued the same year, was in favour of the establishment of "some" means of communication. In 1867 the Board again drew attention to the subject; and in 1868 an Act of Parliament made it incumbent on all railways to provide a means of communication between the passengers and the guards of every train travelling over 20 miles without stopping.

The railway companies themselves had not been indifferent

Mr. Langdon.

to the need, for, as Mr. Preece, in a paper* read by him before the Institution of Civil Engineers in 1866, relates, the London and North Western Railway had spent a considerable sum of money in an endeavour to establish an electrical means of communication; while experiments had also been conducted, with a like object, on the London and South Western Railway. It was not, however, until Mr. Preece produced his system in 1864 that a satisfactory basis for effecting the long-felt need was laid down.

This was rapidly followed, on the South Eastern Railway, by Mr. C. V. Walker, one of our first Presidents, who produced that known under his name in 1866. Varley about the same time established an electrical form of communication on one or more London and North Western trains. The London and South Western fitted up several trains with Preece's system, and trial trains were also fitted with the same system on the Midland and London and North Western. An improved mechanical "cord" system was produced about the year 1868. This "cord" system is that with which every railway traveller is acquainted, and which, passing along the eaves of the vehicles, is available from the outside and from one side only of the train. Eventually, in 1868, the railway companies elected to employ the mechanical rather than the electrical mode of communication; and to this the Board of Trade extended conditional approval.

The South Eastern Company declined to fall in with this decision. Recognising the advantages which an electrical means possessed over that of the mechanical cord, they desired to retain it; and in this they were subsequently† followed by the London Brighton and South Coast Railway. Each of these companies have now their entire passenger stock fitted with an electrical communication. The London and South Western

* Preece, "On the Best Means of Communicating between the Passengers, Guards, and Drivers of Trains in Motion," Institution Civil Engineers, 1866–67.

† The electrical passenger and guard communication on the Brighton line was introduced in 1874, and from that date gradually superseded the mechanical cord.

—a company having more trains electrically fitted than any other company at the time when the "cord" system was adopted—presumably found the difficulty of keeping together a partially fitted stock too great, and in course of time abandoned the effort. Mr. Langdon.

In 1872 Captain Tyler reported the working of the "cord" system as unsatisfactory, and in the course of his remarks observed, "An electrical apparatus appears, on the whole, to offer "the best chance of success" The "cord" communication has, however, been retained in use on the majority of railways; the only exceptions being the two lines previously referred to; the Hull and Barnsley (the stock of which was electrically fitted at the opening of the line in 1885), together with a portion of the stock of the Chatham and Dover Company. The Furness, Great Central, and Cambrian lines employ to a certain extent a partial application of the vacuum brake.

In considering the action of the companies so far, we have to bear in mind the conditions associated with railway travelling at the date of the inauguration of the "cord" system. That an electrical system afforded the railway passenger a more ready means of access there could be no doubt. That the "cord" system could prove of little service in case of outrage is equally clear. But was the prevention of outrage the object of the communication? It was possibly one reason, but no doubt primarily that which was in the minds of people was the possibility of some derangement of the train. Another point for consideration is the fact that the magnitude of most of our main line trains has greatly increased. Such a means of communication might possibly serve the purpose for which it was established with a moderate number of vehicles, while its utility for a much larger number would have to be largely discounted.

However this may be, the somewhat recent outrage on the London and South Western Railway has not only made it clear that, if this means of communication is to provide, as far as the railway companies can, against outrage, the means for operating it must be within easy reach of the passenger, but it has still further demonstrated that outrage of the direst character does not need the time occupied by a train to travel 20 miles in which to accomplish it.

Mr. Langdon.

The whole question has now been brought to a climax by the report* of the departmental committee appointed by the Board of Trade in 1897 to consider and report upon the subject. As is now well known, the result of the deliberations of this committee was the condemnation of the mechanical cord and the recommendation of an electrical system, accompanied by certain suggestions in relation thereto.

The adoption of an electrical system involves some very important considerations. The question has now to be viewed from a vastly different standpoint to that from which it has been considered hitherto. It is to be applicable to the entire passenger stock. It is desirable it should appeal against outrage as well as meet other demands. The interchange of railway stock between the various railway systems is now a matter of everyday occurrence; consequently the system to be adopted should have a common basis, and the mode of connecting the various vehicles should be absolutely uniform. How is this to be accomplished?

The railway companies have at this moment, practically, an open field before them. The total number of passenger traffic vehicles belonging to the entire railway service of Great Britian and Ireland amounts, exclusive of 18,099 locomotives, to 55,868†—in all, 73,967. The total stock of the three companies which have their coaches already electrically fitted comprises 5,430 vehicles and 977 locomotives; so that, if an entirely new system had to be adopted, the number of vehicles which would require modification would, in comparison to the whole, not prove very considerable; and this especially so, should the modification of the stoek already fitted prove of a very simple character.

I propose to consider the question under the following heads:—

1. The electrical system.
2. Mode of connecting vehicles.
3. The means for claiming the attention of the officials in charge of the train.

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* See Appendix.

† See Appendix, Table I.

1. The Electrical System.

Mr. Langdon

Fig. 1 illustrates diagrammatically the system introduced by Preece in 1864. I reproduce this because it forms the basis on which this form of communication has, in almost all cases, been framed; and it, moreover, constitutes the only basis upon which a practical form of passenger and guard communication can be established.

A is an insulated wire proceeding through the train; B represents the rails upon which the train is travelling; *c* is the engine; *d*, *d′*, guards' vans; and *e*, *e′*, *e″*, coaches. Each of the guards' vans is provided with a bell, battery, and a bell key. The passenger vehicles are provided, in each compartment, with a switch, or

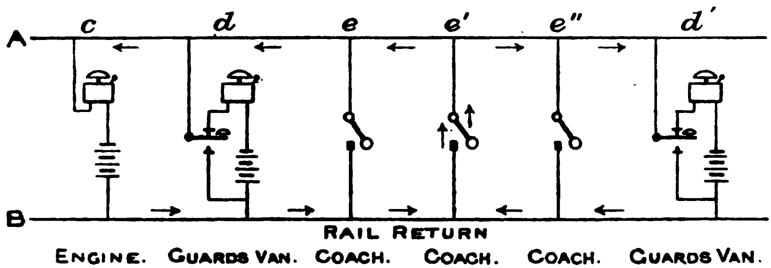


FIG. 1.

means of joining the two conductors, A and B. The batteries are, as will be observed, arranged in parallel. The condition is practically one of balanced currents. All is in tension, but until the balance is disturbed no action sufficient to operate the bells can take place. If the switch at *e′* is closed, we at once form this outlet, and each battery finds its circuit, as shown by the arrows. Every bell in the train is thereon set ringing, and continues to do so so long as the connection at *e′*, or, of course, any other like connection, remains good.

The same occurs when either of the guards desires to signal to the other, or to the driver. By pressing the bell key the two conductors, A, B, are joined in precisely the same manner as when

Mr. Langdon. connected at e', but the guard who makes the signal, in doing so, removes his own bell from the circuit.

Fig. 2 illustrates the arrangement inaugurated by Walker. Considerable modifications have been introduced by Mr. Leonard, Mr. C. V. Walker's successor, but some complication still attaches to it. One insulated wire is employed; the rails, as a rule, forming the "return," although, latterly, Mr. Leonard is supplementing the rail return by an open wire joined to the rail

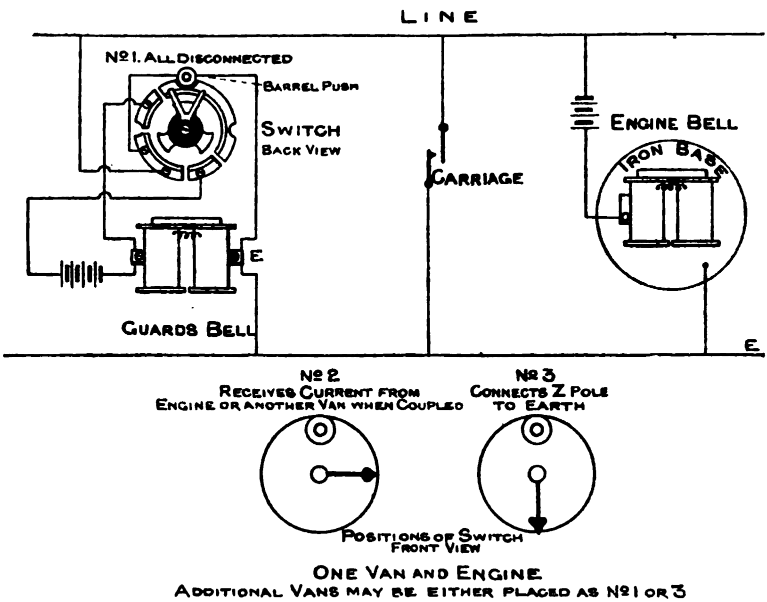


Fig. 2.

connections. The batteries of the two end vehicles only are in use. They are in parallel; but a three-way switch has to be provided in each van, and the guard has to arrange the switch of his van according to the position the van occupies in the train. When intermediate between the engine and another guard's van, it is so placed that his bell may ring but that his battery may be out of circuit.

Fig. 3 represents the Brighton Company's system. It is essentially Preece's. A is the insulated wire, B the rail return conductor, and *c*, *c*, *c*, *c*, are short-circuiting or connecting switches. Mr. Houghton, the company's electrical engineer, informs me that he also is now forming, by means of an open wire carried under the frame of the vehicle, a metallic return—not independent, but supplementary to the earth or rail connections. It is found that the rail "return" is not reliable, especially in very dry weather—a result which might be anticipated. Mr. Langdon.

The London, Chatham, and Dover have somewhat recently applied an electrical form of communication to certain of their trains. The system is termed Varley's, but it has, at the suggestion of the company's electrical engineer, Mr. Burnham,

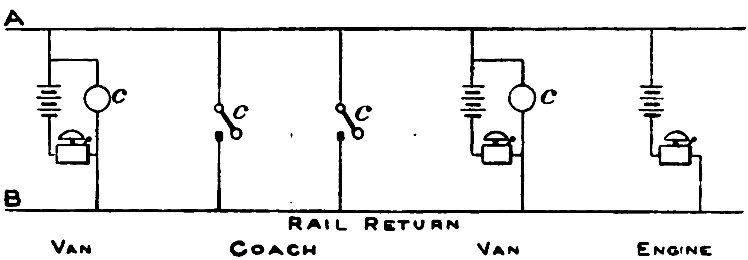


FIG. 3.

undergone some modification. The principle now being pursued is shown in Fig. 4. It is, in practice, very similar to that in use on the South Eastern. The batteries of the two end vans are in circuit. The batteries of intermediate vans are cut out. The system is a two-wire system, both wires being insulated.

It will be observed by the diagram that the guards vans are provided with three connections, two of which are applicable to the connection between the van and the locomotive, and that the bell in the van adjoining the engine is in parallel with that on the locomotive. This dispenses with a battery on the engine, but in order to ensure equal action on the part of the two bells it is necessary that the *resistance* of the conductors and instruments should be fairly equal.

Mr. Langdon.

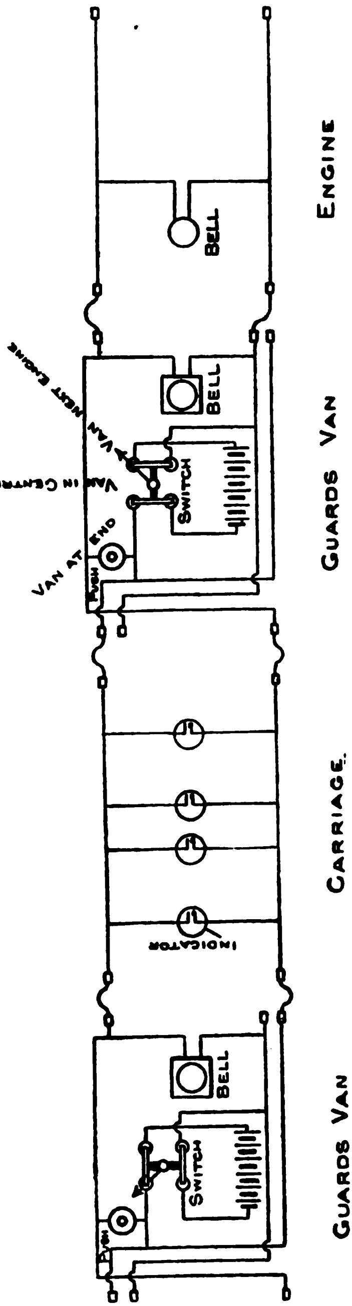


Fig. 4

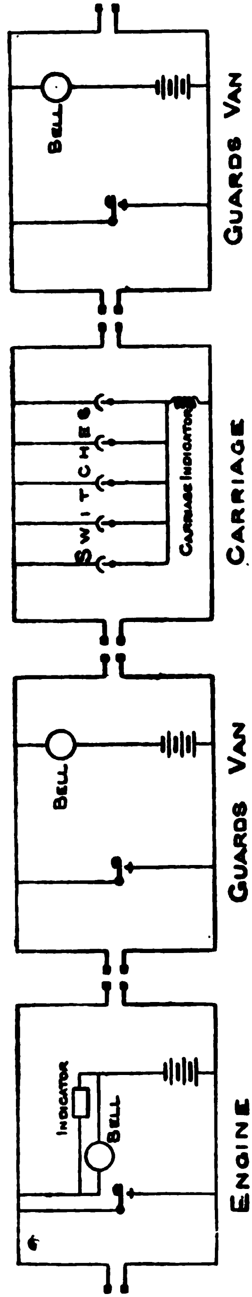


Fig. 5.

The system employed on the Hull and Barnsley is also a two-wire system, and the circuit arrangements call for the provision of adjusting switches in the guards vans, in like manner to those employed on the South Eastern and Chatham and Dover. Mr. Langdon.

Considerable attention has been called to a train fitted up by the Great Eastern. The rails are not in this system made use of. One insulated wire passes through the train. The iron tube serving the brake is taken advantage of to form a portion of the return circuit, which is, consequently, uninsulated. The electrical arrangement is shown in Fig. 5. The batteries are in parallel. The system is that of Preece, with the exception that an open "return," independent of the earth or rails, is provided.

A train has also been fitted by the Midland. The electrical system is that represented in Fig. 1, with the exception that two insulated wires are employed—neither rails nor brake pipe are touched. All portions of the electrical system are protected from moisture, so that there may be no leakage.

We have now before us the various electrical "systems" calling for criticism, and the first question which will present itself for consideration is whether a one or a two insulated wire system is to be preferred.

We have seen, from the experience of the London Brighton and South Coast Railway, that an earth or rail return is not satisfactory. It will also be clear that with an open—*i.e.*, an uninsulated—return any leakage which may arise in the insulated wire, or its connections, will result in short-circuiting the batteries to a less or greater extent according to the amount of leakage; and it will be equally obvious that with two insulated wires any defect in one would not produce such a result. The cost of the second wire is so small that it is questionable if it would amount to that incurred in making the connections to the wheels or other portions of the vehicle in order to ensure a satisfactory earth return, where this course is pursued. Indeed, it is difficult to imagine why a one-wire system should ever have been preferred, unless it is to be accounted for—as is probably the case—by the fact that a two-wire connection may have presented some difficulty in the matter

Mr. Langdon.

of coupling up the vehicles. With this branch of the subject, however, we will deal later on.

In October of last year the author, with a view to obtain data on the action of battery cells joined in groups, in parallel, caused three sets of bichromate cells, composed of two-quart jars, and three sets of ordinary Leclanché, of No. 1 size, to be connected with bells as shown in Fig. 1, the connecting wires being insulated. The working capacity of the cells was taken each day from October 18th to November 20th. The results are given in Table II., Appendix. There was apparently no depreciation. The positive element was free from crystals, and the electrolyte did not show depreciation. Consumption, of course, there was, but it was exceedingly small. This tallies with the experience of Mr. Houghton, who tells me that the consumption of battery power on the Brighton Company's trains—which, it will be observed, are worked under this system—is very little.

The adoption of the parallel battery arrangement, uncostly in battery consumption, possesses also the great merit that it enables the vehicles to be turned about and connected up indiscriminately, independent of the position they may occupy in the train. Switches are not needed, and, manifestly, if this is so, it is a convenience as well as an economy to dispense with them. Their object, except, perhaps, with a view to economising battery power, is not apparent.

2. Mode of Connecting Vehicles.

It has been suggested that possibly the question of coupling up the vehicles has proved influential in the adoption of the single insulated wire and earth return. With an earth return it is necessary to make provision for joining one wire only through from carriage to carriage, the opposite pole being connected to the wheels and frame of the carriage so as to afford metallic connection with the rails. This branch of the question appears to have proved a somewhat serious stumbling-block. In the Board of Trade report we find it laid down as one of the conditions to be achieved, that the electrical coupling should be combined with

one of the couplings already in use—the brake, heating service. the screw coupling, or the side chains. All, except the first named, we may at once dismiss as unsuitable, or too difficult in application to merit consideration. Mr. Langdon.

Fig. 6 is an illustration of the coupling employed by the South Eastern. It is practically an extension of the communication wire with a connecting loop at its end. Its use is purely tentative.

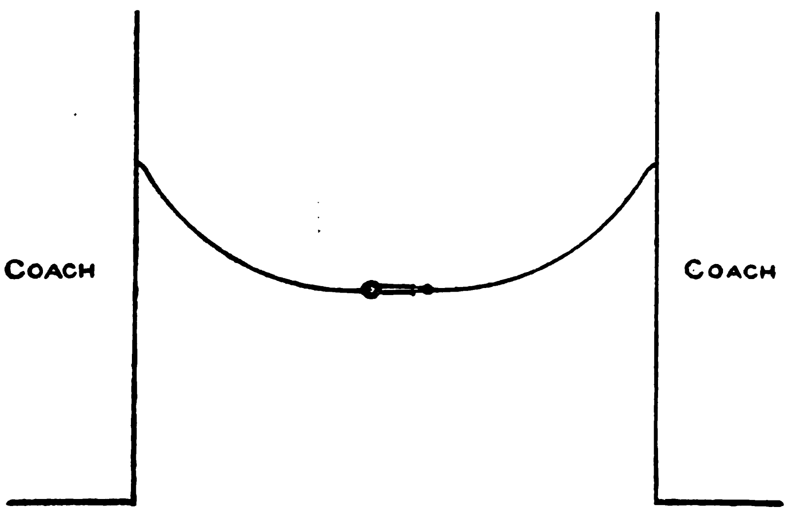


FIG. 6.

Fig. 7 represents the coupling employed by the Brighton Company. It is formed of a spiral of wire enclosed in an india-rubber tube. The wire terminates at each end in a galvanised iron loop, which at the end which is attached to the vehicle passes over a hook forming the termination of the line wire; but the loop at the end which has to be coupled up is divided at, *a*, so as to admit the loop of the corresponding part, applicable to the adjoining carriage, passing through it.

Each of these companies is, as previously indicated, gradually

Mr. Langdon.

introducing an uninsulated return wire, the rails in dry weather not proving satisfactory. This return wire is coupled through by a bare stranded copper wire terminated with a split spring connection, *b′*, as shown in the illustration.

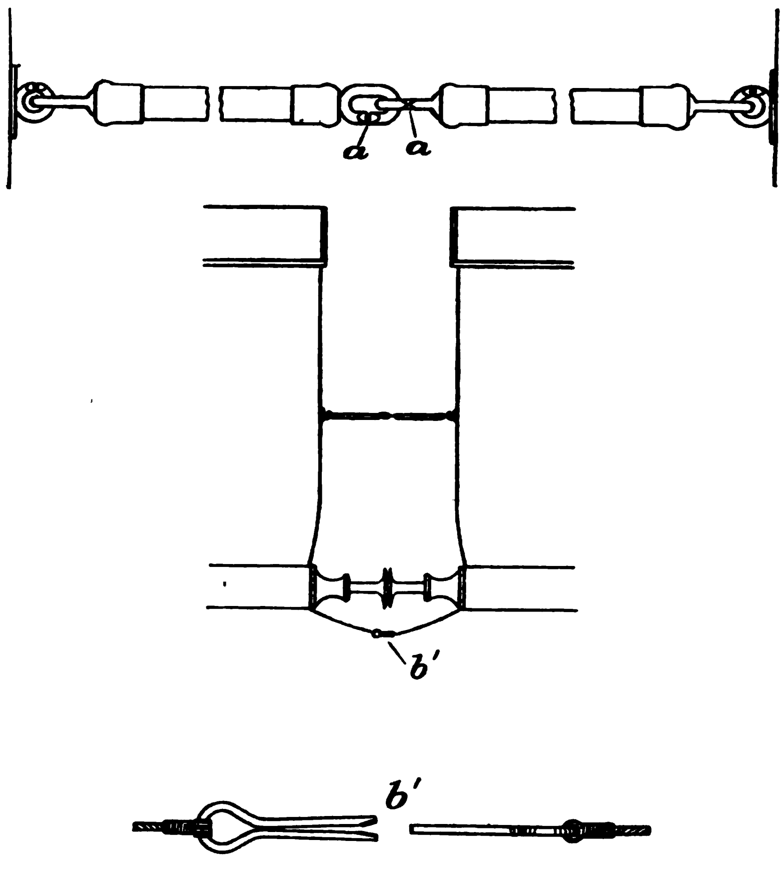


FIG: 7.

Fig. 8 is the coupling used by the Chatham and Dover Company, and 8A is the coupling box. The loop, A, in coupling up, is passed over the hook, B, which, under the influence of a strong spring, holds it in good contact with the projection C.

Following the views of the Board of Trade, Mr. Hollins, the electrical engineer to the Great Eastern Railway, has succeeded in combining the electrical communication required for this purpose with the brake coupling. The electrical parts of the coupling are countersunk in the metal portion of the brake coupling, which

Mr. Langdon.

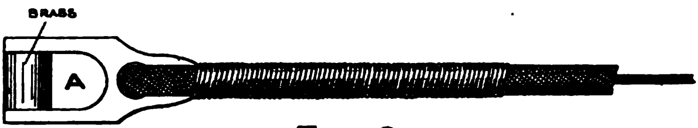


FIG. 8

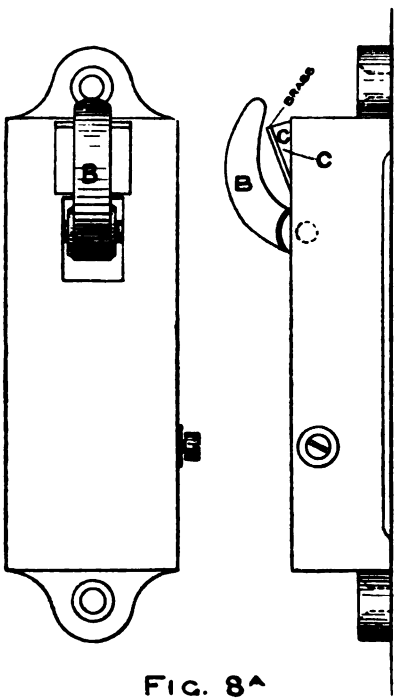


FIG. 8A

is cut away for their reception. The wires are arranged inside the brake tube, being connected, at the coupling end, to the electrical connections, and carried through the tubing at the end which connects with the vehicle as shown in the illustration.

Mr. Hollins has applied his electrical contact to both the

Mr. Langdon.

Westinghouse and vacuum brake. The illustrations Figs. 9, 9A, and 9B refer to the latter, which is perhaps the more simple of the two,

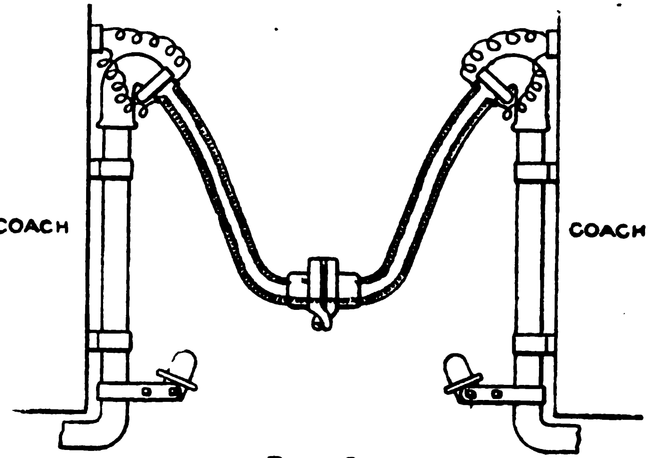


FIG: 9.

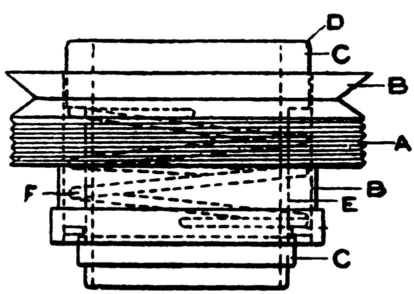


FIG: 9A

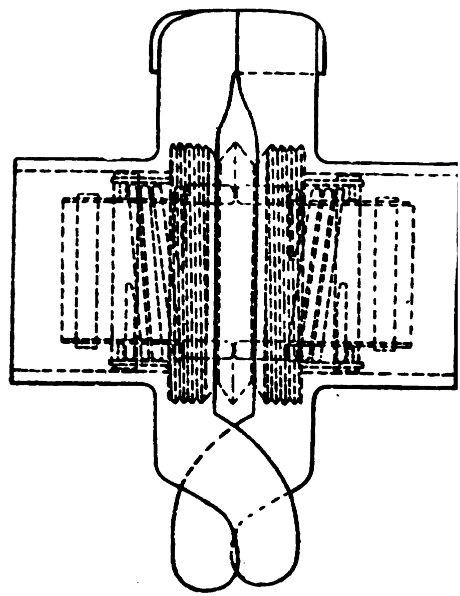


FIG. 9B

although I believe it is Mr. Hollins's opinion that the Westinghouse forms the better contact. Mr. Langdon.

Fig. 9A shows the contact-making arrangement, which is screwed into the metal portion of the coupling as seen in Fig. 9B. A is a ring of insulating material, to which a metal flange and tube, B B, is secured. This tube serves to hold within it the contact-making piece, C, which is formed of a metal tube, reduced, as shown by the dotted lines at E, in order to receive a spiral spring, F, the duty of which is to press forward C so as to insure good contact with the corresponding part of the coupling.

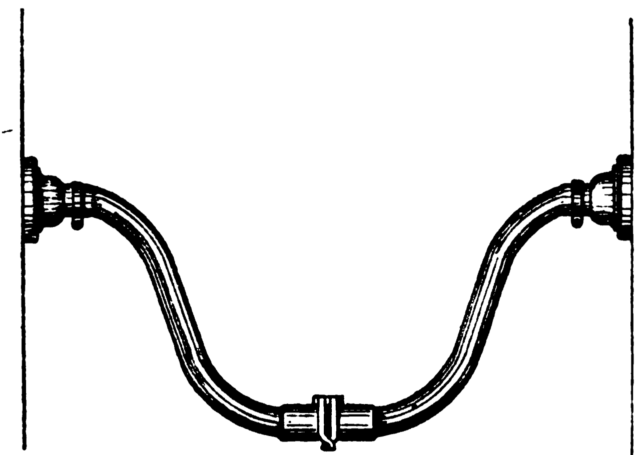


FIG. 10.

The edge, D, of the tube C is milled, to assist it to make good contact. G is a metal collar which limits the action of the spiral spring in that direction.

In 1885 the author was granted letters patent for combining an electrical with the vacuum brake coupling, but in this instance the electrical portion was superimposed upon the brake tube and coupling. It was not, however, allowed to remain long in use, serious objection being raised by that department of the service responsible for the satisfactory

Mr Langdon

operation of the brake. It is believed these are the only practical attempts made with a view to effecting this combination.

The coupling adopted by the author for the Midland train is shown in Fig. 10. The electrical portion consists of two plates of metal, insulated the one from the other, to which the two wires forming the communication are attached. These plates are so cut that that belonging to one part of the coupling shall dovetail into that belonging to the other portion. The two plates are so fixed within an iron case, formed after the fashion of, but smaller than, the vacuum brake coupling, that that applicable to the positive wire of the one coach shall engage with that applicable to the similar pole of the connecting coach. It is thus impossible to cross the wires, while the one coupling serves for the two wires.

The vacuum brake form of coupling has been selected as the protecting and guiding cover, for the reason that, the mode of coupling it up being so well known, no difficulty can possibly attend its application to the purpose of an electrical coupling. The wires are carried from the contact plates to the end of the vehicle in tubing similar in character, but smaller than the brake tubing. The wires of each vehicle terminate on an insulated plate fixed to each end of the carriage, so that any changes needed may be readily effected. The wires throughout are fully protected from moisture.

Samples of all these couplings will, it is hoped, be placed upon the table.

3. The Means for Claiming the Attention of the Officials in Charge of the Train.

In the Brighton Company's carriages this is secured by the operation of a knob (Fig. 11). This knob forms the handle of a commutator, and on being pulled out it, by means of the commutator, connects the line and earth wires. It is fixed in the centre of the partition which divides one compartment from another. Each compartment is thus provided with a means of raising an alarm from one position. On the knob being pulled

out to a certain extent it becomes locked, and requires the presence of the guard to release it. The bells continue to ring so long as the commutator remains extended. The Brighton Company employ no outward indicator to identify the vehicle from which the alarm has been raised. It is the duty of the guard to examine the commutators throughout the train, until he finds that which has been pulled. It is not understood that any difficulty has attended the absence of such an indicator. Mr. Langdon

In the South Eastern Company's stock considerable improvements, in the mode of operating the alarm commutator, have been effected by Mr. Leonard. The commutator is placed over one of the quarter windows, and is provided with a knob as in the

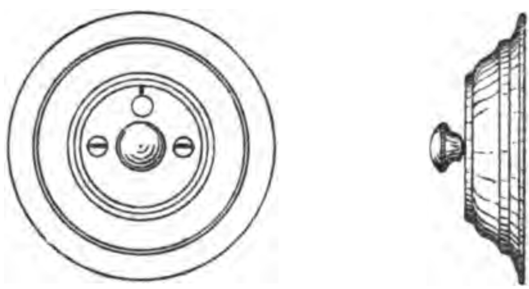


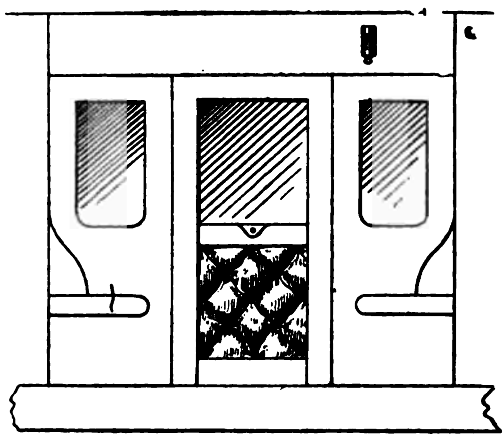
FIG. 11.

Brighton Company's trains; but this knob is arranged to pull *downwards* (see Fig. 12), and is thus more readily operated than if it had to be pulled outwards. Each compartment has but one point from which the alarm may be raised. Each first class compartment, and nearly all second class compartments, are provided with an outside indicator, but to the third class one indicator only is alloted to each carriage.

Fig. 13 illustrates the design of the commutator employed by the Chatham and Dover. Each compartment is provided with one commutator fixed in the centre of the division. There is no indicator to show the source of alarm.

Mr. Langdon.

The commutator in each instance is of the most simple character—merely connecting the line and earth wires or the two line wires—and, where used, discharging mechanically the indicator affixed to the outside of the vehicle or compartment as may be



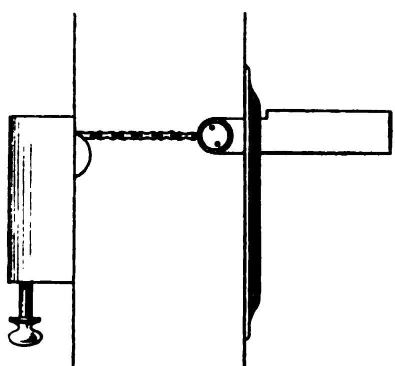


FIG. 12.

arranged. The commutator, once operated, remains locked until reset by the guard

The coaches which have been fitted by the Great Eastern are, in like manner, provided with a commutator fixed in the centre of

the division between the compartments. The user turns a switch handle, capable of being moved in either direction—to the right or to the left—which closes the circuit. Each coach is provided with an outside indicator on either side of the vehicle for the purpose of indicating whence the alarm is raised. On the circuit being closed by any one compartment, the current passes through Mr. Langdon.

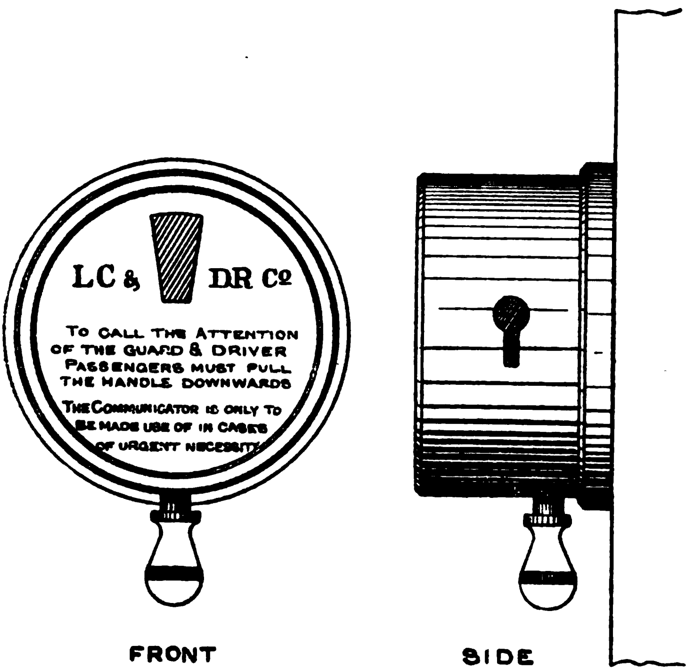


FIG. 13.

the indicators and discharges them. The principle upon which this indicator is constructed is that of the Sykes signal reverser. The current energises an electro-magnet, A, Fig. 14. On the armature being lifted, a heavy bar, B, is discharged, and, falling upon C, forces it out of the notch in the bar D, in which it normally rests. E is the indicator, heavily weighted, so that, on C being lifted out of D, it shall push the rod before it in the

Mr.
Langdon.

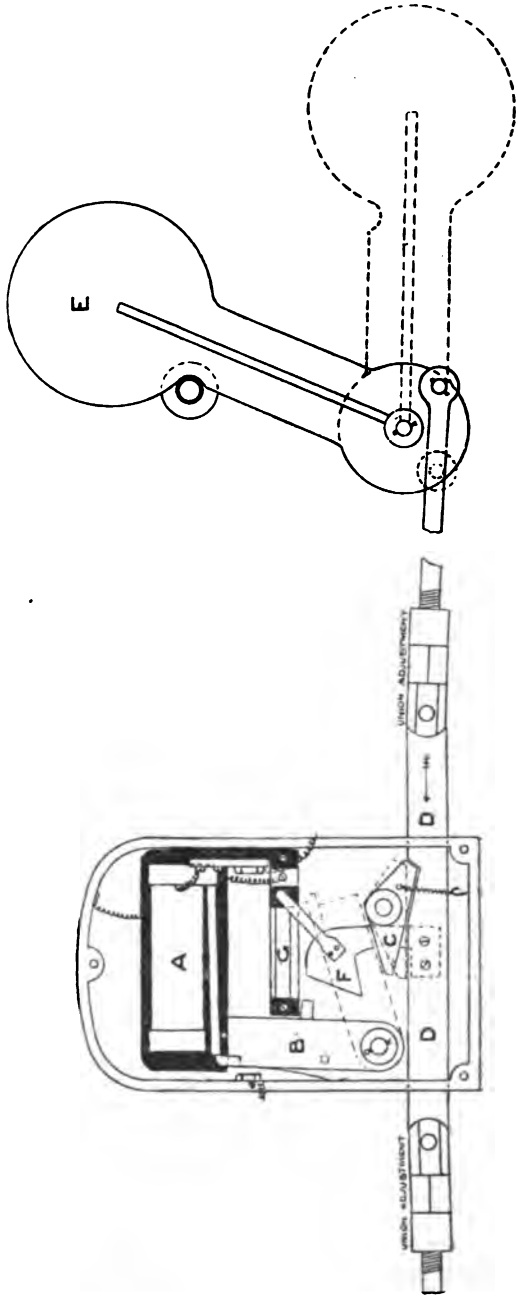


FIG. 14

direction of the arrow. As D is moved forward it carries with it the *replacer*, F, which is attached to it, placing the spring which Mr. Langdon.

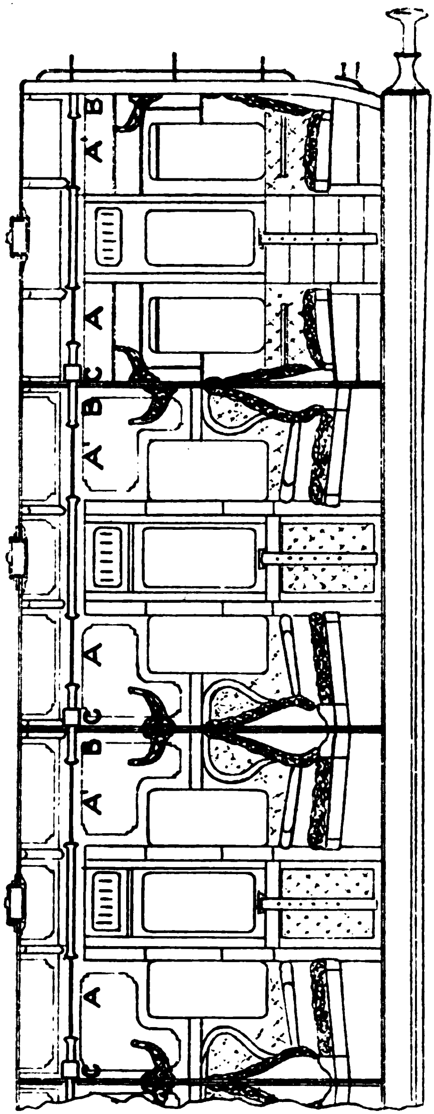


Fig. 15.

projects from it in contact with G, thereby short-circuiting the electro-magnet. F at the same time forces back B into its normal

Mr. Langdon.

position, where it is again held by the armature of A. On the guard lifting E into its upright position, the rod, D, is drawn forward until the detent or locking piece, C, falls into its slot, when the entire arrangement is reset and ready to be again discharged. The re-adjusting portion, F, has, along with D, been moved forward, and the short-circuiting spring attached thereto again rests upon an insulated part.

The means adopted for raising the alarm in the trains running on the Midland forms a marked departure from the course generally pursued hitherto. A flexible steel wire (Fig. 15) is carried inside a metal tube along each side of the carriage over the doors and quarter lights. This wire has one end made fast at, say, B in each compartment, or at the end of the coach. The other end, C, is attached to the lever of a commutator capable of being operated in either direction. The wire, covered by a fabric suitable for handling, is exposed for some 12 or 15 inches at A, over each quarter light. On the wire being pulled from any one of the exposed parts the commutator lever is operated, a disc on the outside of the carriage is discharged, the circuit is closed and the bells set ringing. Each compartment has thus *four points from which the communication may be brought into operation.* Each coach has an indicator on either side, and if necessary each compartment may be so provided. The commutator, at the moment of sounding the alarm, becomes locked, and can only be released by the guard, either from the outside or the inside of the vehicle. The object in view in this arrangement has been to place at the disposal of the passenger a means of communication, accessible from the interior of the vehicle at not less than four points of the compartment, in such a position that it shall be out of the reach of irresponsible persons. If preferred, the cord might be entirely enclosed and operated by a pull-down knob or handle, but it would, of course, be more costly.

Considerations.

We have now arrived at a point when we may, perhaps, with advantage take into consideration the material points.

We have before us, broadly, the principles of the electric

systems in use, as well as those experimentally established. What, then, are the principles which may be suggested as those which should underlie the provision of this means of communication? Mr. Langdon.

Experience tells us, and the Board of Trade Report suggests, that *the communication should be of such a character as to lend itself readily to the interchange of carriages between different railways.* This involves not only the electrical principle, but the mode and form of coupling, as well as the means of restoring the communication to its normal condition. The electrical system must be such as will conform to that in use on other lines of railway! We have seen that all those systems in use do practically stand upon a common basis. They have either one insulated wire, and an earth or an uninsulated "return," or two insulated wires. They differ to the extent of some switching arrangements; but it will be clear that that method which involves no special provision in respect of the position which the van occupies in the train, is that which must commend itself for adoption. To modify the arrangements of those vehicles which do not conform to this demand would be an extremely small matter. It would also be better, as has already been explained, to employ an insulated "return;" but it is possible, though not desirable, to use an open "return."

That the coupling for any electrical communication which might be established on railway trains should be combined with one of the mechanical couplings seems to be a sort of tradition handed down from those days preceding the period when railway companies found marshalling sheds indispensable. We have seen that the companies who have used an electrical communication have for years employed an independent coupling, and that they are now supplementing this by a second. The "cord" communication calls for coupling up, and certainly the time required for connecting together an electrical coupling cannot be so great as that now incurred in connecting up this "cord" communication. The question, moreover, is one which, in respect of the majority of trains, does not in any way affect platform work,

Mr. Langdon.

for the reason that the trains, as a rule, come to the platform ready coupled up.

It may, and very naturally, be asked—But why, if the two couplings—the electric and the brake—can be combined, should they not? The question which here arises is mainly one of responsibility. A question on the cost and convenience will naturally also present itself, but that of responsibility is unquestionably the most material. Will such a combination lessen the responsibility which attaches to the efficient maintenance of the brake? When a failure arises in a brake coupling or tube, how is it to be dealt with without the presence of the electrical attendant, and *vice versâ*? Are these men to hunt in couples—the one tied to the other? What does such a combination entail? It means the replacement of some 150,000 brake couplings and tubes. Will the cost prove more economical, or so economical as if the two were established and maintained independent the one of the other, leaving the two departments freedom of action and individual responsibility? I advance these remarks from conviction, after mature consideration, that to combine the electrical with the brake coupling is, on many counts, undesirable.

It has already been demonstrated that but one coupling, for the two wires composing the communication, is necessary. That the handling of this coupling is a perfectly simple and easy matter will be evident from an examination of the sample on the table.

In the early portion of the paper it was suggested that, in their adoption of the mechanical cord, railway companies were animated by a desire to provide for the possibility of a derangement of the train more than with a view to the question of outrage. Improvements in the lubrication of axles, in points and crossings, in the permanent way, and in the rolling stock generally, render the conditions of railway travelling to-day incomparable with those of 1868. Derangement of a passenger train in motion is now a most unusual occurrence. Unhappily, violence and outrage still exist. In viewing this question it would appear that that which has now to be chiefly provided for is the protection

of the traveller from outrage. Is this adequately provided for by the provision of a means of raising an alarm from one position only in each compartment? It is due to the conviction that this is not so, and that the only effectual mode of doing so is the provision of a means of communication in the neighbourhood of each corner of a compartment, that I have endeavoured to evolve the method referred to as that which has been applied to a Midland train. Mr Langdon.

Although seemingly a very small matter, there is another phase of the question which is material to the adoption of an universal system, viz., the means by which the communication, once used, is restored. This is done by a key carried by the guard. It is usually of private construction, and differs with most companies. It should be common to all, otherwise guards will require to be furnished with a key applicable to each of the companies' stock which may pass over the line upon which he is engaged.

Other points of equal, although not of universal, importance will no doubt present themselves: the description of wire to be used; whether it shall be insulated with gutta-percha or rubber; the character of the indicator, of the commutator, the description of battery, &c.—all questions of moment, but not vital to the question of interchange of stock. In relation to that, we have to bear in mind that it involves three fundamental principles, viz.—

(1) The electrical system.
(2) The form of coupling.
(3) Uniformity of releasing key.

That one system should be adopted throughout, even in matters of detail, is, of course, to be desired, but uniformity in the three points named are essential to an interchange of stock.

There remain but two more points at which to glance—viz., communication with the driver, and provision for calling attention to the division of a train when in motion.

There is no difficulty in providing for communication with the driver, but it is desirable the battery on the engine should be dispensed with. This it is possible to effect by providing

Mr. Langdon. each guard's van with a coupling applicable to the locomotive. The cost of adopting this course would be less than that attending the provision and maintenance of a battery on the engine. Another point is whether it would not be better to sound a whistle on the engine, rather than operate a bell. The whistle would probably be easier to maintain.

In the earliest established electric communication systems a means was provided for claiming attention should a train become severed. There is no difficulty attending this, but it would appear that the need for it no longer exists. Since those days the automatic brake has been introduced, and if a train should at any moment become divided, the progress of all portions of it would at once be arrested

The title of this paper indicates that its purpose is to treat of the subject from an electrical point of view. Reference has, however, been made to the fact that there is in use another means of claiming the attention of the guard, viz., a partial use of the automatic vacuum brake. Each compartment of a carriage is provided with a means of opening a small valve connected with the brake pipe. To what extent this is effective,—whether it varies with the speed of the train, or other conditions, or whether such a system is a desirable one,—I am not in a position to say, and I only refer to it here in order to point out that, should an electrical system be adopted, carriages provided with this means of applying the brake could be looped in on electrically fitted trains by means of a loose coupling; but there would be a difficulty in dealing with electrically fitted vehicles interposed in trains worked under this partial application of the brake system.

In concluding this paper, I desire to tender to the general managers, and to my *confrères*, the electrical engineers, of the various railways, who have so kindly assisted me with information, my most hearty and respectful thanks.

Mr. Langdon

APPENDIX.

Table I.

PASSENGER ROLLING STOCK OF THE FOLLOWING ENGLISH AND WELSH RAILWAYS.

RAILWAY COMPANY.	Engines.	COACHING.								Total Coaching (exclusive Engines).
		Saloons.	1st.	2nd.	3rd.	Composites.	Vans.	Horse Boxes.	Carr. Trucks.	
Brecon & Merthyr	81	2	9 1st	& 2nd	33	4	5	1	1	55
Cambrian	71	...	14	...	110	72	38	23	6	263
Cheshire Lines ..	...	...	67	58	177	37	16	35	31	421
East & West Junc.	...	...	...	...	4	4	3	...	...	11
Furness...	124	5	...	...	190	70	47	27	30	369
Great Central ...	775	3	53	...	710	194	82	53	24	1,119
Great Eastern ...	993	37	487	436	1,492	916	367	422	241	4,348
Great Northern ...	1,069	127	100	156	1,323	579	284	240	145	2,954
Great Western ...	1,863	...	259	179	2,623	1,160	1,114	598	352	6,285
Hull, Barnsley, & West Riding Junction ...	75	2	...	...	47	24	12	10	6	101
Lancashire, Derbyshire, & East Coast... ...	16	1	...	...	39	8	13	8	4	73
Lancashire and Yorkshire ...	1,256	...	244	137	2,124	628	221	134	136	3,624
London, Brighton, and South Coast... ...	463	...	379	245	1,178	454	382	216	146	3,000
London, Chatham, & Dover	210	22	244	224	442	75	173	35	21	1,236
London & North Western ...	2,385	...	1,910	230	2,231	(Included in 1st Class.)	894	651	1,000	6,916
London & South Western ...	702	79	342	110	1,175	674	874	290	150	3,694
London, Tilbury, and Southend	42	3	54	...	237	16	17	6	...	333
Metropolitan ...	80	2	62	85	163	44	6	3	2	367
Midland	2,360	...	295	...	1,890	936	685	443	546	4,795
Midland & South Western ...	18	...	...	...	10	16	7	19	2	54
North Eastern ...	1,963	...	160	16	1,986	509	407	395	164	3,637
North Stafford ...	153	4	11	13	189	58	25	30	21	351
South Eastern ...	489	1	375	255	867	176	397	117	141	2,329
Taff Vale	198	...	5	7	130	47	36	11	8	244
Totals ...	15,286	288	5,020	2,151	19,370	6,701	6,105	3,767	3,177	46,579

Mr. Langdon.

Table I.—continued.

PASSENGER ROLLING STOCK OF THE FOLLOWING SCOTCH RAILWAYS.

Railway Company.	Engines.	Coaching. Saloons.	1st.	2nd.	3rd	Composites.	Vans.	Horse Boxes.	Carr. Trucks.	Total Coaching (exclusive Engines).
Caledonian	788	...	277	...	1,115	297	206	116	41	2,052
Great North of Scotland ...	112	...	75	...	284	51	62	16	13	501
Glasgow & South Western ...	345	...	181	...	563	104	120	82	101	1,151
Highland	183	2	54	...	167	49	71	25	20	388
North British ...	740	...	407	...	1,282	140	288	177	481	2,775
Totals ...	2,118	2	994	...	3,411	641	747	416	656	6,867
IRISH RAILWAYS.										
Belfast & County Down ...	29	...	22	23	64	44	8	8	1	170
Belfast & Northern Counties	73	...	10	6	124	77	32	23	8	280
Cork, Bandon, & South Coast	18	3	...	...	27	14	6	4	2	56
Donegal	9	...	...	...	10	9	9	3	5	36
Dublin, Wicklow, & Wexford	58	...	48	73	101	12	41	18	6	299
Great Northern ...	145	...	36	20	181	87	87	95	43	549
Great Southern and Western	178	1	50	34	178	76	98	86	32	555
Midland Great Western ...	127	1	25	25	93	38	61	60	24	327
Waterford, Limerick, & Western	58	...	2	...	59	37	23	22	7	150
Totals ..	695	5	193	181	837	394	365	319	128	2,442
English...	15,286	288	5,020	2,151	19,370	6,701	6,105	3,767	3,177	46,579
Scotch	2,118	2	994	...	3,411	641	747	416	656	6,867
Irish	695	5	193	181	837	394	365	319	128	2,422
Totals ...	18 099	295	6.207	2,332	23,618	7,736	7,217	4.502	3,961	55,868

NOTE.—The above table is exclusive of the stock of those railways the line mileage of which is approximately less than 50 miles. The entire passenger rolling stock is approximately 18,515 locomotives and 57,430 vehicles.

Mr. Langdon.

Table II.

Daily Test of Batteries joined up in Parallel, October 18th to November 20th, 1897.

Date.		Bichromate 2-Quart Cells.						Leclanché No. 1 Cells.					
		1.	2.	3.	4.	5.	6.	1.	2.	3.	4.	5.	6.
1897. October	18	1·30	1·75	1·75	1·70	1·70	1·65	1·35	1·45	1·45	1·40	1·45	1·45
,,	19	1·35	1·70	1·75	1·65	1·70	1·60	1·35	1·45	1·45	1·40	1·45	1·45
,,	20	1·38	1·75	1·75	1·65	1·65	1·60	1·35 bare	1·45	1·45 bare	1·40	1·45	1·45
,,	21	1·43	1·70	1·70	1·65	1·70 bare	1·62	1·30	1·45	1·40 bare	1·40 bare	1·45	1·40
,,	22	1·45	1·70	1·75	1·70 bare	1·70	1·65	1·30	1·45	1·40	1·40	1·45	1·45 bare
,,	23	1·45	1·70	1·75	1·70 bare	1·70	1·65	1·30	1·45	1·40	1·40	1·45	1·45 bare
,,	25	1·55	1·80	1·80	1·75	1·70	1·65	1·30	1·45	1·45 bare	1·45 bare	1·45	1·45
,,	26	1·50	1·80 bare	1·80 bare	1·75	1·70	1·65	1·30	1·50 bare	1·40	1·45 bare	1·45	1·45
,,	27	1·50	1·75	1·75	1·70 bare	1·70	1·65	1·30	1·45	1·40	1·40	1·45	1·45
,,	28	1·50	1·75	1·75	1·70 bare	1·70	1·65	1·30	1·45	1·40	1·40	1·45	1·40
,,	29	1·50	1·75	1·80 bare	1·70	1·75 bare	1·65	1·35 bare	1·45	1·40	1·40	1·45	1·45
,,	30	1·55	1·75	1·80 bare	1·70	1·75 bare	1·65	1·35 bare	1·50	1·40	1·40	1·45	1·45
November	1	1·55	1·80	1·80	1·70	1·75	1·65	1·35	1·55 bare	1·45 bare	1·45 bare	1·45	1·45
,,	2	1·55	1·80 bare	1·75	1·70	1·70	1·65	1·35	1·50 bare	1·45	1·45 bare	1·45 bare	1·45
,,	3	1·55	1·80	1·70	1·75	1·70	1·65	1·35	1·50	1·45	1·45 bare	1·45	1·45
,,	4	1·55	1·80	1·75	1·70	1·70	1·65	1·35	1·50 bare	1·45	1·40	1·45	1·45
,,	5	1·55	1·80	1·75	1·70	1·75 bare	1·65	1·35	1·45	1·45 bare	1·40	1·45	1·45
,,	6	1·60 bare	1·80	1·80	1·70	1·75	1·65	1·35	1·50 bare	1·45	1·45	1·45	1·45
,,	8	1·65	1·90 bare	1·85 bare	1·75	1·80 bare	1·70	1·35	1·50	1·45	1·45	1·50 bare	1·45
,,	9	1·60 bare	1·80	1·75	1·70	1·70	1·65	1·35	1·50 bare	1·40	1·40	1·45	1·45
,,	10	1·65	1·90 bare	1·80 bare	1·75	1·75	1·70	1·40 bare	1·50	1·45	1·45	1·45	1·45
,,	11	1·60	1·90 bare	1·80	1·75	1·75	1·70	1·35	1·50	1·45	1·45	1·45	1·45
,,	12	1·60	1·85	1·80	1·75 bare	1·75	1·70	1·40 bare	1·55 bare	1·45	1·45	1·50	1·50 bare
,,	13	1·60	1·90	1·80	1·75 bare	1·75	1·70	1·35	1·55 bare	1·50 bare	1·45	1·50 bare	1·45

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DAILY TEST OF BATTERIES, &c.—*continued.*

Date.	Bichromate 2-Quart Cells.						Leclanché No. 1 Cells.					
	1.	2.	3.	4.	5.	6.	1.	2.	3.	4.	5.	6.
1897. November 15	1·60	1·90 bare	1·85 bare	1·70	1·75	1·70	1·40 bare	1·50	1·45	1·45	1·50	1·45
,, 16	1·60	1·90	1·80	1·75	1·75	1·70	1·40 bare	1·50	1·45	1·45 bare	1·45	1·45
,, 17	1·60 bare	1·90	1·80	1·70	1·75	1·70 bare	1·40	1·50	1·40	1·45 bare	1·45	1·45
,, 18	1·60	1·95	1·80	1·70	1·75	1·65	1·40	1·50	1·45	1·45	1·45	1·45 bare
,, 19	1·60 bare	1·90	1·80	1·70	1·70	1·65	1·40 bare	1·50	1·45	1·45	1·45	1·45
,, 20	1·60	1·90	1·85 bare	1·75	1·80 bare	1·65	1·40	1·50	1·45	1·45	1·45	1·45

RAILWAY PASSENGER COMMUNICATION.

Extracts from the Report of the Committee appointed by the President of the Board of Trade.

SYSTEMS IN USE ON RAILWAYS IN OTHER COUNTRIES.

The Committee thought it desirable to ascertain the means of communication used on railways in other countries, and they therefore requested the Board of Trade to obtain for them through the Foreign, Colonial, and India Offices information as to the means adopted on the principal railways in Austria-Hungary, Belgium, France, Germany, Italy, Russia, Switzerland, the United States of America, Canada, New South Wales, and India.

The replies received show that in Austria all fast and express trains are fitted with an electrical communication (system Rayl). In Hungary the use of corridor carriages is said to be nearly universal, and, when these are fitted with an automatic brake, means are provided to enable the passengers to apply it, and thus call the attention of the servants in charge of the train, and no further means of communication is thought necessary. A system of electrical communication was formerly employed,

but it has gone out of use since automatic brakes were generally adopted on main lines. In Belgium a means of communication by the application of the Westinghouse brake, similar in principle to that used in connection with the vacuum brake by the Great Central and other railways in this country, is in force on the State railways; and it is instructive to note that it was thought desirable to increase the size of the valve through which the air escapes from the brake pipe, when the brake is applied by a passenger, so as to ensure the speedy stoppage of the train. The same arrangement is used on the principal Italian railways. In France the system to be adopted is left to the discretion of the individual companies, some of which employ electrical means, generally "Prudhomme's" system, whilst others prefer the Westinghouse brake system. Mr. Langdon.

In Germany almost all passenger trains are fitted with automatic brakes, and means are provided to enable passengers to apply the brake in case of emergency.

In Russia and Switzerland corridor carriages are generally used, and, when automatic brakes are provided, passengers can apply them.

In Canada there is direct means of access from one part of a train to another, each carriage being connected to the next by a gangway, but a cord runs through the train by which a signal to stop can be conveyed to the driver.

In India various means of communication have been tried, but the Indian Government state that the only one that has given satisfaction is an electrical method, invented by the late Mr. G. K. Winter. This system, in which the electric connection between the carriages is made by means of the side or safety chains, has been in use on the Madras Railway since 1880, and is being introduced on the South Indian Railway.

Conclusions as to Existing Systems.

Most of the witnesses examined by the Committee agreed that the ordinary means of communication by means of a cord outside the carriage is unsatisfactory, and with these opinions your Committee entirely concur.

Mr. Langdon.

Owing to the difficulty of obtaining access to the cord in cases of emergency, and to the uncertainty of its action when pulled, this system cannot be regarded as affording a reliable means of communication, and the Committee have no hesitation in condemning it.

Some advantages over this system are no doubt possessed by those in which a cord or wire passes inside the carriages, but the Committee cannot hold that they are satisfactory.

As regards the electrical systems of communication referred to above, the Committee had the advantage of making trial of that in use by the London Brighton and South Coast Railway Company, on a train placed at their disposal by the company. They found it to work efficiently, whilst the company, as previously mentioned, have no record of its having ever failed.

Evidence was received by the Committee indicating that the electrical methods in use on the South Eastern, and London Chatham and Dover Railways had given satisfactory results.

The Committee also had an opportunity of inspecting an electrical system fitted by the Great Eastern Railway Company to one of their trains, in which the wires are carried along the brake pipes, and the coupling of the pipes between two carriages automatically gives the necessary connection between the wires. This system worked satisfactorily when tested, and it has the advantage of obviating the necessity for any additional coupling—a point which the Committee think will be found of considerable importance to railway companies in working their traffic.

The Committee are of opinion that all these electrical systems, though some of them may be capable of improvement, may be considered as providing efficient means of communication.

The London and South Western Railway Company have been good enough, at the request of the Committee, to fit up a train with a somewhat similar system, in which the electrical connection between the carriages is made by coupling the side chains.

The system was invented by the late Mr. G. K. Winter, and, as has already been stated, is largely used in India.

As the train has only recently been fitted with the apparatus,

there has not been time for a prolonged trial of the system, but, so far as the tests have gone, they have, we understand, been satisfactory. Mr. Langdon.

The conducting surfaces, however, being open to the atmosphere and to dust or snow, are not likely, in the opinion of the Committee, to be as efficient as those which are placed within the brake coupling.

A system of communication by means of the automatic brake has been applied in this country only on trains using the vacuum brake.

Through the kindness of the Great Central Railway Company, the Committee have had an opportunity of practically testing this method of communication. It worked satisfactorily, and companies by whom it has been adopted speak of it in the highest terms. One of its greatest advantages is that each carriage is complete in itself, and the coupling of the brake pipes between two carriages of necessity connects the means of communication. It has also been pointed out to the Committee that, in the case of interchange rolling stock fitted with both the vacuum and Westinghouse brakes, the same apparatus within the compartment can be made to actuate valves connected to either brake pipe, so that, whichever brake is in use, the means of communication is available.

As already stated, the system is largely applied on Continental railways to the Westinghouse brake, and appears to have given satisfaction, but in the opinion of the Committee it would be improved by an additional arrangement by which the attention of the driver and guards might be audibly called to the signal, and by which drivers and guards could communicate with each other.

It has been suggested that the increasing use of corridor or other similar carriages will do away with the necessity for means of communication, but the Committee cannot agree with this view, as they are of opinion that some method of communicating with the driver and guards is as necessary upon corridor as upon other trains.

Your Committee have carefully considered the second point

Mr. Langdon.

referred to them, viz., whether any system in use or available is so efficient as to make its general adoption desirable.

The Committee do not think that any one of the electrical systems of communication in use at the present time possesses such advantages over the others or over the brake system as to justify them in recommending it for general adoption.

On the other hand, many railway companies entertain a strong objection to granting to a passenger even a limited control over the brake, and the Committee do not think that a system which involves the grant of such a control, however efficient it may be, should be pressed upon a company against the advice of its responsible officers.

Of systems not in use, but available, the Committee have had a number brought before their notice. Many of these are very ingenious and do credit to their inventors, but they have in most cases been anticipated by other inventions, and either closely resemble, or possess no great advantage over, means now in use.

Conditions which a Method of Communication should Fulfil.

The following are the conditions which it appears to the Committee a method of communication should fulfil to be entirely satisfactory:—

1. It should be one which would lend itself readily to the interchange of carriages between different railways.

2. It should be easily applicable, and should communicate directly with the driver as well as with the guards, whilst the means by which it is actuated by the passenger should be in a conspicuous position, either in the centre or at both ends of the compartment, without affording too great a temptation to passengers to tamper with it.

3. It should be reasonably cheap, as to both initial cost and maintenance.

4. It should afford an indication outside the carriage of the compartment from which the communication has been made, and a passenger should not be able to replace the means by which the alarm is given.

5. It should not entail the use of additional couplings to those already existing, viz., the screw coupling, the side chains, the automatic brake, and the heating apparatus (where in use). Mr. Langdon.

6. It should be capable of being used as a means of communication between the driver and guards of a train, as well as between the passengers and the driver and guards.

Extension of the Existing Law.

The Committee were also directed to consider whether any extension or amendment of the law on the subject is desirable, and to this point they have given careful attention.

Under the present law means of communication need only be provided on trains running 20 miles without stopping. It is obvious, however, that serious harm can be done, either by accident or design, in a journey of much less duration. Experience has, unfortunately, shown that a very short journey affords sufficient time to enable murder and outrage to be committed.

Several railway companies have already fitted, or are fitting, means of communication to all their trains, irrespective of the distance to be run without a stop, and representatives of other companies agreed in evidence that such an extension was reasonable.

The Committee are of opinion that the law should be so amended as to require efficient means of communication to be provided on all passenger trains.

Conclusions.

On the three points to which the inquiry of the Committee was directed, their conclusions may therefore be summarised as follows:—

I. That of the methods of communication at present adopted, the outside cord system should be condemned as inefficient, while the systems in which the cord or wire is inside the carriages cannot be regarded as satisfactory. The principal electrical systems and the communication by means of the brake may, however, be held to be efficient.

Mr. Langdon.

II. That no one of the electrical systems so far excels the others as to enable the Committee to recommend it for general adoption, although they prefer the system of communication described above as experimentally used on the Great Eastern Railway.

III. That the law should be extended so as to require the provision on all trains of an efficient means of communication between passengers and the servants in charge of the train, which could also be used as a means of communication between the guards and driver.

The President.

The PRESIDENT: The subject of Mr. Langdon's paper is one with which the name of Preece is quite inseparably associated. In the very first paragraph of the paper the name occurs. If he would be kind enough to give us his views on this very interesting subject, I am sure we should all be only too delighted to listen to him.

Mr. Preece.

Mr. W. H. PREECE: Some of the facts mentioned by Mr. Langdon resuscitate the past. It seems something like bringing a fossil out of the ground. It is 34 years since I worked at this subject. From 1864 to 1868 I took as much active interest in this application of electricity to the working of trains as I have done in any other application of electricity. I remember very well having brought the thing to a very fair state of working order, when my visions of fortune and success were dashed to the ground by two very silly and foolish blunders. The system which Mr. Langdon has shown in that first diagram was applied on the London and South Western Railway, and it met with a great deal of attention and approval, and I read a paper here somewhere about that date.

The PRESIDENT: It was in the Institution of Civil Engineers in 1866.

Mr. PREECE: I thank you. In those days any application of electricity attracted an immense amount of attention, and when I brought this subject before the Institution in 1866 the room was crammed with the general managers and chairmen of directors and all the swells of the railway world. Well, I bad actual

models, as Mr. Langdon has here, and some of them worked very well. But there was a mode by which the breaking away of a train was communicated to the guards and to the driver. The coupling was fixed on to a hook, and the hook was worked by a very strong spring. When the carriage broke away the hook was forced by the spring in contact with a stud which communicated with the second wire or the earth and set all the bells ringing. With a great flourish of trumpets I explained this to the audience. I said, "Now, gentlemen, when I pull the coupling, "you will see the alarm will at once be raised." The alarm was not raised, and the sceptical general managers, who did not believe then in electricity so much as they do now, laughed me to scorn. I was taken aback. I was not accustomed to address audiences in this hall so much then as I am now, and I passed it away with a very silly excuse—that "perhaps some dust has got on the stud." Of course Mr. Allport, who was then the great general manager, the leader of the railway world, said, "Well, if some dust in the "Institution of Civil Engineers causes this system to fail, what will "it do on the railway line, where dust is in the ascendant?" Of course I felt very chagrined. But, however, the London and South Western Railway were so pleased with it themselves that that did not deter them. The explanation was made, and the explanation was simply this: that one of my assistants—I forget now who it was—found that the audience, before the lecture commenced, were very anxious to try these different things, and they would go and make this alarm go off. To prevent this he took a small bit of paper, wetted it with his saliva, put it on the stud, and never told me a word about it. The result was that when I pulled the coupling it came up against the piece of paper, and of course that stopped the current, and I was done. Mr. Preece.

On another occasion, on the South Western Railway, two or three trains were fitted up; but there was one special train which travelled with all the railway people and all people interested in the matter, who were invited to a great lunch somewhere. The carriages of the train were full, and just as we passed Clapham Junction the alarm sounded, the train was stopped, and we all wanted to know what was the matter. The bells rang, and no one

Mr. Preece. could stop them ringing. I was confused, and everybody was confused. I think Mr. Langdon was there at the time; and we were very much bothered to know what was the matter. Again I was laughed at. However, we did, after a little time, find out what the fault was, and we went on, and the day passed pleasantly. But prejudice against the system was established. What with the No. 1 failure and the No. 2 failure, the committee of railway managers fought shy of electricity. The second trouble rose from this—that in each van I had placed a lineman to see that everything was in working order; and in the front van the lineman—one of the smartest fellows we had—thought that that bell did not ring well enough, so, having a spare battery, he put the second battery on to get all the power he could. You all know, from what Mr. Langdon has said, that the whole essence of this system is a balancing of the E.M.F.'s on the circuit. You have in each of those cross circuits a certain E.M.F.—say 10 or 12 volts—but you must maintain that voltage exactly the same. If you double the number of cells, you all know what will happen: the balance is disturbed, the currents flow, and the bells ring; and again I was beaten. So on that occasion, over 30 years ago, the communication between passengers and guard was not generally accepted, really not because it was not a good thing—because it was—but because these two accidents, simple things in themselves, established a prejudice against electricity. In the early days of electricity that sort of thing militated very much against its progress. It was one of the difficulties we had to encounter. Fortunately, now we know so much more of these things that incidents of that kind do not deter people from looking into matters. We understand them more, and such accidents are not likely to arise. I feel, as I have said before, not much interest personally in this matter now—I have dropped it for 30 years—but it is a source of great gratification to me to find Mr. Langdon reading this paper and bringing up facts that otherwise, perhaps, would be forgotten in the records of the past, unless they were restored to light by writers and men of standing like Mr. Langdon.

rd. Mr. W. LEONARD: I wish, Sir, in the first place to correct one

or two errors that Mr. Langdon has made in his description of the apparatus in use on the South Eastern Railway. He states that Fig. 2 illustrates the arrangement inaugurated by Mr. C. V. Walker. I wish to state that this system was introduced by myself, and has been in use on the engines and in the vans of the South Eastern Railway for many years, and that it altogether differs from, and has entirely replaced, the system invented by Mr. Walker. Further on he states, "But a three-way switch "has to be provided in each van, and the guard has to arrange the "switch in his van according to the position the van occupies in "the train." I very much regret if I did not make clear the working of the switch to Mr. Langdon; it is not indispensable, but is found in practice to be convenient. There are two positions for the guard to deal with—viz., the one which brings the apparatus into work (it is simply turned from an upward position to a downward), and *vice versâ* on leaving his train. What is the object of this? If a train is put back into a siding, and the commutator is pulled by anyone accidentally or mischievously, and there is no one present with a key to restore it, the batteries on that train will be run down, and when it is brought into work the system will be out of order. The instructions are to turn up the switches when the train is out of work. There is also another point. There may be five or six vans on a train, and it is only necessary to have a portion of them connected; the guard sets the switches accordingly. The position 2 (see diagram) enables the circuit to be proved from one van to another. Wherein the complication exists I fail to see. Mr. Langdon evidently does not think a switch is desirable or necessary, but in my experience I have found it of so much service that I still continue to use it. Mr. Leonard.

In 1888 a continuous rod system was brought into use on the South Eastern Railway, which Mr. Langdon describes as having but one indicator to the carriage. But he has not described the other portion, although I am under the impression that I sent him the information. The model of this, which is on the table, has but one contact and one indicator, although there may be six or seven compartments, and each compartment has the means of bringing the communication into operation.

Mr. Leonard.

It is economical, but I must say that it has not quite justified the departure. The officials, after being accustomed to an indicator in each compartment, do not like having one indicator for the whole carriage; if the train is stopped at night, and the person who puts the communication into operation does not signal to the guard, he may have to enter six compartments before he finds out the spot, the delay to the train being thereby increased. All engines on the South Eastern Railway are fitted with battery and bell, both placed on the tender; the latter was formerly placed over the fire-box of the engine, which involved a connection between the engine and tender. Owing to difficulties in maintenance, a change was made about seven years ago, which has been beneficial. My predecessor used an outside brass disc on the carriages, which attracted thieves, and it was found impossible to prevent the discs being stolen while the trains were standing in the sidings. Eventually wood was substituted for the brass. But apart from this the maintenance was difficult, owing to carriage washing and exposure, and they are being superseded. I mention this as I notice inventors are still patenting a similar thing. With regard to the rail return, I have never experienced any difficulty on the trains when they are running, but there certainly has been in stations. When the carriage wheels stand on rusty rails they appear to lack touch with the metal, and for testing purposes some little inconvenience has been felt; but it largely depends on the state of the rails, and very frequently disappears altogether. With regard to the wires used for fitting, I have adopted for many years, instead of gutta-percha, vulcanised india-rubber—the best type of braided electric light wire, No. 18 copper.

Mr. Langdon raises three considerations in his final paragraph. The electrical arrangement of my system accords with the author's views The coupling is not final. That in use (Fig. 6) is a solitary relic of the system which Mr. Walker introduced about 20 years ago. Iron wire was then used, but for some years copper has taken the place of iron; it is very simple and effective in form, and appears to be more commonly used than any other type. If another form of coupling is to be uni-

versally introduced, the combined form of Mr. Hollins's will, in my opinion, be the most acceptable. The traffic department very much objects to additional labour, however small, being imposed on shunters, couplers, &c. Mr. Leonard.

[*Communicated.*]—I would also remind the author that the number of cells must be uniform to interchange systems. Six cells, No. 2 Leclanché, are used on the South Eastern Railway.

Mr. W. H. WINTER: At this late hour of the evening I hardly like to commence my remarks; but, if you will allow me, Sir, I will just make a few. I think we are very much indebted to Mr. Langdon for his statistical information, and his historical *resumé* of the progress of this matter. I am very glad to see he condemns the use of the rails for the "return," and also that he deprecates interference with the all-important vacuum brake to effect this communication. I notice that the systems he describes are confined to those in use on British railways; but no allusion whatever, I am sorry to say, has been made to my late brother's system, which has been in constant use on the Madras Railway, and brought hourly into operation ever since the 1st January, 1879. That system has been submitted to all kinds of tests, and it fulfils one particular requirement of the Board of Trade in that it makes use of an existing coupling. Why Mr. Langdon should dismiss so summarily the use of the side chains as being unsuitable and too difficult in application to merit consideration, I cannot conceive. The side chains are used in India, and they have been submitted to every possible test imaginable. They have been covered with tar; they have been covered with grease; they have been covered with Stockholm tar of the consistency of treacle; the iron hooks have been heated to red heat and plunged in tar which has become a hard varnish; but the communication has never once failed. They have sifted dust and ashes over the tar, and it has been impossible to break the connection. It has earned the most unqualified praise from the traffic manager, the agent and manager, and the locomotive engineer. They run about 2,000,000 train miles per annum on the Madras Railway, where this system is used. It is employed by the rear guards in all cases to start the train; and if Mr. Winter.

Mr. Winter. a train is timed to run *through* a station, unless the engine-driver receives a signal from the rear guard that all is right he stops the train; so that it is in hourly use on all the trains, both goods and passenger, on the Madras and some other railways. In this country the communication is seldom used, and no such test has been applied. The great thing, I think, is that it involves no additional work on the part of the staff. Stress is laid on the importance of no additional coupling being required; but I imagine, from the tenour of the concluding remarks in the paper, that an independent coupling is recommended.

I would further point out that in the system to which I am referring there are no complicated switches—no arrangement of balanced circuits, so liable to accidental disturbance—whilst the cost of applying it is vastly less than that of any other system. It was exhibited at the Calcutta Exhibition of 1883–84, and obtained a gold medal. I have not, for want of time, explained the details of making the connections through the coupling chains, but they are very simple, and I shall be happy to furnish particulars to anyone desirous of further information.

Mr. Wolff. Mr. C. E. Wolff: I received an invitation to attend this meeting and give the views of the locomotive department of the Midland Railway on Mr. Langdon's paper. There is only one point, I think, which calls for attention from me, and that is the locomotive indicator. Upon the Midland we use a whistle for the cord communication, and it seems to me—if it can be avoided—that it is objectionable to introduce a bell and a battery on an engine. Although the whistle would require a little more work to operate it than a bell, and might be stiff, it seems to me that such an arrangement as that shown in Fig. 14, of the compartment indicator, placed on the engine in parallel with the guard's van bell, would meet the requirement. Its resistance would not enter into the question at all, because directly it was released the magnet would be short-circuited, and the guard's van bell would work as if the indicator were not there. I should like to ask Mr. Langdon whether he knows of any case in which such an arrangement has been tried. A spring might be used instead of a weight, and the other end coupled to the cock of the whistle.

Another point upon which very great stress has not been laid—although Mr. Langdon referred to it—is the nature of the handle in the compartment. I think, now that we are getting heating handles and various things, it should be kept as distinctive as possible, like the cord of Mr. Langdon's. I once saw a lady try to stop a train in passing a junction by turning off the hot water, because she thought the train was off the line. The train was not off the line, and there was nothing the matter, but she was very indignant because, as she said, "these things never work." Mr. Wolff.

Professor SILVANUS P. THOMPSON: I should like to bring to the attention of Mr. Langdon a patent specification of ancient date, which contains an exceedingly interesting suggestion with regard to this very subject. It is a specification taken out in 1852 by John Mirand, the inventor of the "trembling" bell. At the end of the patent specification there is a description of a method of communication through a railway train which differs very considerably from the practical systems which have been described to us by Mr Langdon, for it requires three wires to be carried from the head of the train into the guard's van at the end. There is also a train coupling suggested in the final specification. Prof. Thompson.

As it is one of the great functions of an Institution like this to bring together men from different parts of the electrical profession and enable them to settle their ideas in common, I venture to criticise a purely verbal point in Mr Langdon's excellent paper. Surely, if there is one thing that is to be desired in the settling of our ideas, it is that we should use the same terms in the same sense. Well, it is because there are terms used in senses different from their ordinary acceptation in other branches of electrical engineering that I venture to make a very small criticism. I notice on several pages there is a thing called a "commutator." Now I am quite unable to see that it commutes anything whatever. It is simply a short-circuiting switch, a mere make-circuit key like any electric bell button. Why is it called by a name known in every other branch of the electrical industry to mean something which performs a totally different function?

On page 749, something is spoken of as an "open" return.

Prof Thompson. Well, in every other part of the electrical profession, the word "open," as applied to a circuit or return, means that it is disjoined—that there is no circuit. But here it is used as meaning "uninsulated." I wish we might induce our friends who are working in other branches of the industry to use terms in the same significations as those accepted throughout the industry at large. It is bad enough among the medical electricians to have such terms as "galvanisation," "faradisation," and "franklinisation," of which we know nothing whatever at this Institution.

Lastly, may I point out that a discussion on this subject, or even a paper on this subject, however good, would only be possible on this particular hemisphere,—that it would have absolutely no meaning whatever if read in the other and more modern hemisphere of the globe, because there they have reduced railway travelling to a civilised science? There you travel always in carriages that communicate from end to end of the train—cars with communication right through—neither the stuffy compartments nor the awkward corridor carriages we have in this country, but real, proper cars. Moreover, on the other side they employ electricity far more in those cars than we do in our railway "coaches." For when you travel in one of those exceedingly comfortable cars you have electric buttons close to you wherever you are sitting, in order to call the gentleman of colour who is in attendance as conductor at the end of the car.

Mr. Hollins Mr. F. HOLLINS [*communicated*]: Mr. Langdon states that "a train" has been fitted on the Great Eastern Railway with electrical passenger and guard communication. There are, however, two trains fitted, and some 20 engines, and they have been in work several months; and another hundred carriages, and 20 engines, are now being fitted up.

Mr. Langdon lays great stress on the fact that I make use of the iron tube of the automatic brake, and that this is not an insulated return. This is correct so far as the Westinghouse brake couplings are concerned, but the drawing (Fig. 9) in his paper of the vacuum brake couplings shows that both wires come out through the rubber pipe, and do not touch the iron pipe. In fact, they are perfectly insulated from it. They are not in contact

with the spiral of wire inside, and an ordinary iron vacuum coupling case is not in electrical contact with the iron pipes at all. So far, then, even if Mr. Langdon still thinks there is anything in the point (I say there is not), it obviously falls to the ground. The arrangement *was originally* as Mr. Langdon states, but I found that the couplings (the vacuum couplings), when changed, were not unscrewed off at the pipe connections, but that the rubber was unclipped and torn from the pipe. I therefore brought the terminals from the inside to the outside through the *rubber* pipes, as shown in the drawing (and not through the iron pipes), directly to a two-wire (insulated) connection box, as also shown, and as appears in the exhibit of the vacuum brake couplings). In the case of the Westinghouse brake coupling, the iron pipe is made use of as part of the return circuit, so as to take advantage of the simplicity of the connection, when screwing on the pipe (on changing a coupling), to join up one part of the circuit by the necessary action of screwing on the coupling itself. The other insulated connection is also simplicity itself, and is designed on the principle of the bicycle valve. The nut cannot be lost, and the connection cannot be put together wrongly. I invite members carefully to examine both the vacuum and Westinghouse brake couplings electrically fitted, and in the room. Everything has been designed to simplify the changing of these couplings, so that the operation may be done without the assistance of those two men that Mr. Langdon seems also to insist upon, but which is purely imaginary on his part. It is no more necessary to have the two men to change the coupling (electrical and brake), than it is for the bicyclist to take his wheel to the machine shop every time it requires inflating. Mr. Hollins.

Mr Langdon mentions that he in 1885 superimposed an electrical connection upon the vacuum brake coupling, but that it was not long in use—serious objection being raised by the department responsible for the brake. I do not think his own want of success, if he will pardon me saying so, is sufficient to justify his condemnation of other attempts, without strong evidence in support. I may mention that in not one single instance in my arrangement, on the Westinghouse brake, has the

Mr Hollins. electrical connection interfered with the brake, nor has the brake interfered with the electrical connection. Nor do I in the least anticipate any of the evils and troubles which Mr Langdon has indicated.

As regards the two men for dealing with the electrical and brake coupling, my reply is that they are not necessary in my case; but, as Mr Langdon has two independent couplings, he *will* require two independent men to attend to them and change them. In favour of the combined coupling, it should not be forgotten that the brake couplings *must* be connected before a train starts, therefore the electrical connection cannot be forgotten. The connecting of the one ensures the connection of the other. But this is not so with Mr Langdon's independent coupling—the electrical coupling *may* be forgotten.

And just another word about Mr Langdon's call arrangement. If there be only one indicator at the end of the carriage to indicate the one from which the call emanates, how is the compartment to be found? It would seem that the guard must open each door and look in, at the top, for the ancient method of a slack cord. And, if so, will there be sufficient slack to make the indication unquestionable? And, if it has been pulled in the compartment next the indicator, what is there to prevent those in another compartment, further on, pulling up the slack, and therefore removing the compartment indication, first in one direction, and then in another? Again (seeing that a flexible *steel* wire is used), what is to prevent those operating it in one compartment pushing the slack forward into the next, and so convicting their neighbours of their own wickedness? For it follows that if it can be pulled it may also be pushed.

If, however, Mr. Langdon uses a *separate outside indicator and commutator for each compartment* (the same as the South Eastern Maltese-cross indicator arrangement), I should like to ask for information as to its cost—including *the carriage department charges*. And as he bases some of his argument on the desirability of having this arrangement on both sides of the carriage, it would seem to necessitate a rather heavy expenditure. However, I invite information on the point.

There is just one other question. Mr. Langdon tells us that his flexible steel cord passes over the door, and is only to be reached over the fanlight. Now, if it is a fact that this steel cord is about 6 feet from the floor (the doors are about 5 feet 9 inches), and that, being over the seat, it requires more than a 6-foot reach, it would appear that undersized ladies, and children, would not be protected, or, if they want to be protected, that they would have to jump on the seat. There are plenty of men, women, and children, liable to require the communication, who could not possibly reach such a height from the floor, and over the fanlight. Mr. Hollins.

Of course, as Mr. Langdon's paper is, as I take it, an appeal from, at least some of, the findings of the Board of Trade committee, formed upon the evidence of some of the most eminent railway men, and considering the composition of the committee itself, I think I am entitled to put on record the fact that my passenger and guard communication *is the only one in practical use* (so far as I know) which complies with the whole of the conditions laid down by this Board of Trade committee.

Mr. P. V. LUKE [*communicated*]: I should like to say a few words in corroboration of Mr. Winter's remarks about the system of electrical intercommunication in trains employed by his brother, the late Mr. G. K. Winter, on the Madras Railway. Mr. Langdon, in his interesting paper, said that the use of the side chains as electrical connections between vehicles may be at once dismissed, but I can hardly think he meant to be so sweeping in his condemnation. Mr. G. K. Winter has conclusively proved, by actual practice extending over a period of 20 years, that side chains *can* be successfully used for the purpose. Mr. Luke.

I have myself seen the experiments referred to by his brother, made at Arkonam, when the application of grease, tar, and sand to the hooks of coupling chains failed to prevent electrical contact; but his remarks require to be supplemented by a description of the method employed to attain this result. Mr. G. K. Winter made use of two insulated wires running through each vehicle—carriage, waggon, or whatever it might be. These wires are brought out at the buffer-bar close to where the side chains are attached, and each wire in prolongation is laced

Mr. Luke. through the corresponding chain, and the end of the conductor is attached by a screw and soldered to the shank of the coupling hook. The only change required by Mr. Winter's system in the ordinary coupling gear is a slight modification in the shape of the hook, and the facing of it with copper sheet, brazed on. The jaws of the hooks are given more of a cutting edge where they touch each other, and the copper facing, aided by the weight of the chains and the constant shaking, ensures a perfect electrical contact. I may add that the ordinary coupling hooks can be easily so altered, and at little expense.

For the rest, given two insulated conductors running through a train, any number of devices may be employed for calling and attracting attention. Mr. Winter, I believe, preferred the simple arrangement of a relay bell and battery on the engine (or tender), with the current continuously on holding back the tongue of the relay and keeping the local bell circuit open. The circuit can be broken by pressing a button placed conveniently in the carriages. On the Madras Railway the button is protected by glass, which has to be broken before the alarm can be given. When the train has been made up the battery and bell are placed in position, and the gentleman in the vehicle at the end of the train (I dare not call him the "guard," for if Professor Silvanus Thompson demurs to the use of the word "coach" he will surely object to this equally obsolete term) presses his button to test the system: the bell rings, and the engine-driver replies with his whistle. Signals can be made at will from the end of the train to the driver, and if any vehicle is detached or breaks away the bell at once rings.

Mr Langdon. Mr. W. E. Langdon, in reply, said: My object in bringing this subject before the members of this Institution is evident, I think, from the paper itself. It was purely to place before them, and the railway companies generally, the present condition of electric communication between passengers and guards, with the view that, as there is a great probability of its being extended throughout the whole of the railway passenger service, they might have a basis on which to work for the purpose of assimilating it throughout the whole of the British railways.

I am sorry that we have not more railway men here to take part in the discussion. So far as my own company is concerned, the day is not propitious, for it is our committee day, and all our chief officers are at Derby. Mr. Langdon.

Mr. Preece's remarks indicate the importance of a proper balance being maintained. If one battery runs down below another one, of course it has not the same opposing power as the others, and it will form a circuit, or leak, for the flow of the higher potential batteries which are in parallel with it. This strongly emphasises the importance of employing two insulated wires for this means of communication. If you have but one insulated wire, and that wire affords by any means access to the earth—by defective insulation, or by the connections at the end of the coaches in wet weather affording a leakage—then there will be action on the part of the batteries. It may not be sufficient to ring the bells or sound the alarm, but it will tend to destroy the batteries sooner than otherwise would be the case.

I am indebted to Mr. Leonard for his corrections. I placed myself in communication with Mr. Leonard, and I certainly gathered from him that the information given in my paper and in the illustrations on the wall related to the methods which are in use. I do not know that I am quite right in saying they are what Mr. Walker instituted; but I assumed they were what Mr. Walker had instituted, and that they were improved by Mr. Leonard. However, I am very glad that he has set the matter right. When vehicles are standing in sidings they present, no doubt, some temptation to the carriage cleaners and others to interfere with the communication in order to see how it works, and the introduction of a switch—merely a disconnecting switch for the battery—would be of service. Then we have the question of an indicator to each carriage. It is considered that unless there is an indicator to each compartment it is rather a trouble to the guard. The railway companies will consider very much the question of cost, and they will weigh well the question of efficiency, cost, the loss of time, and the trouble to which the guard may be subjected.

Mr. Langdon.

Some remarks were made with reference to the rail return. I can quite understand that, if a rail return is used, where a train is brought to a standstill, or where an engine has been sanding the rails for the purpose of getting a better grip of the rail, there may be some difficulty in passing a current through it. On all accounts the rail return is, I think, undesirable.

Mr. Winter has spoken of the success of a system in operation on the Madras Railway. On that line they make use of the side chains—usually known as the safety chains—for the electrical communication. Those chains are very heavy, and the links hang well together, thereby making, one would suppose, a good contact. I feel somewhat at a loss, however, to understand how the current passes through a connection of that kind which has been well served over with tar, and that well coated with sand. I do not know the system which Mr. Winter adopted in India, but I understood that the London and South Western Railway had, at the request of the Board of Trade, applied this system to a train, and I accordingly communicated with Mr. Goldstone, the electrical engineer to that company, upon the subject, and I find it was applied to what is termed a "short buffer" train; consequently it has been possible to make the electrical connection by the aid of the chains with the end vehicles only. I cannot say what degree of satisfaction has attended the experiment.

I am afraid that Mr. Wolff would find some difficulty in applying the instrument shown in Fig. 14 to a locomotive engine. A locomotive engine is not the most desirable thing on which to instal electrical apparatus. Whatever it may be necessary to place there should be strong, and consist of as few parts as possible. A whistle which may be opened by the electric current and closed by the driver when the current ceases is, I think, the proper thing, and I am pleased to find that in this I have Mr. Wolff's support.

I am very much indebted to Dr. Thompson for having called attention to the very interesting patent of John Mirand, of 1852. I knew nothing whatever about it until Dr. Thompson drew my attention to it prior to entering this room. Then Dr. Thompson very kindly directs my attention to certain terms

which I have used. I have heard the word "commutator" applied for years to instruments such as those to which I have applied it, and I do not know that it is not a good term. Then, again, the question of "open return" is debated. Well, "open" means uncovered, I imagine, and it is of common application to open overhead wires. Then there is the term "coaches" or "cars." When one gets into railway circles one is apt to follow the terms made use of in railway parlance, and there the word "coaches" is in general use, although "cars" is less used. Mr.

[*Communicated.*]—I have been favoured by the Secretary with a sight of some remarks communicated by Mr. F. Hollins. These remarks were evidently drafted for reading at the meeting, but as they were—owing to want of time—not read, I was unable to reply to them when replying to others.

With regard to the insulation of Mr. Hollins's coupling, I think I am right in assuming that one of the wires is connected to the brake coupling. If this is so, then, in wet weather, there will be surface leakage, provided there exists any leakage in the other wire, tending to the destruction of the battery.

To his remarks in relation to my deductions in respect of the responsibility and labour which would be involved in attendance upon a combined brake and electric coupling, I beg to observe that I am quite content to leave the question in the hands of those who are responsible for the efficient operation of the brake. It is clear that, if two men—one for the brake and one for the electric department—are not employed, the responsibility of one must be relegated to the other. Will the electrical department accept responsibility for the brake; or that department responsible for the efficient working of the brake accept responsibility for the electric communication? and would, in that case, the electrical department be content to confide its duty to the other? The removal of two wires from certain connections, and their replacement by others, is evidently an easy matter; but there is always the possibility of the wrong wire being attached, and the electrical communication thereby thrown out of order. With a combined coupling you have to face the fact that a failure

Mr. Langdon.

in the one—whether electrical or brake—necessitates the removal of the other.

It is quite evident that men *may* neglect to couple up the purely electrical coupling; they *may* also forget to couple up the wires of the combined coupling. If, however, it is the duty of the guard to test the electrical communication before starting his train, in the same manner as he now does with the mechanical cord communication, any such omission should be readily discovered. That the automatic brake is an important element in the safe working of traffic no railway man will deny. Is it desirable its responsibilities should be impaired, or that we should risk their becoming so?

The paper clearly points out that, if desired, the system advocated by me admits of one commutator being placed so as to serve two compartments. Economy may suggest the use of one for the entire coach, but it is purely a question of cost and efficiency. At all events, one commutator between two compartments at once disposes of Mr. Hollins's suggestion. Apart from this, however, provision is made for the use of a clutch, or check-piece, if found needful. I would here ask, Is it desirable to view the question from the standpoint which suggests vicious and foolish interference? A penalty is attached to any improper use of the communication. Is not this, and the sense of the travelling public that its provision is purely for their safety, sufficient to guard it from any such undue interference? The system I advocate is the outcome of a desire to provide something which will afford greater protection against outrage than has hitherto been obtainable, and that upon an economical basis. I am quite content to leave it to others to determine if it is out of the reach of short people or children. It affords *four positions* from which the alarm may be raised. Ruffianism does not usually occur in a carriage full of people. If it does, there will be more than one to hand over the delinquent to justice. If the victim is the only other person present, whether a short person or child—if children are ever placed in such a position—they would, in my opinion, find little difficulty, if necessary, in jumping upon the seat. There would be, assuming an assailant occupied one of the four

positions, at all events, three points from which to sound the alarm. With respect to cost, I imagine there can be no doubt that the exceedingly simple method I have advocated will not exceed that of any other system to which reference has been made. It ought to prove the least costly. Mr. Langdon.

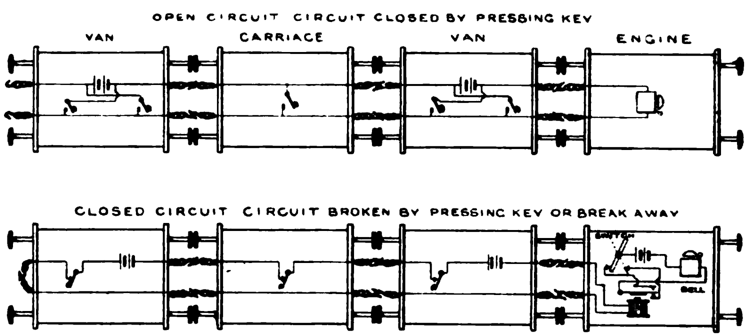
Why the paper should be regarded as an appeal from the Board of Trade report, I am at a loss to see. It is, I believe, simply a fair statement of the present condition of the subject with which it deals. It was necessary that the railway companies should have placed before them the circumstances which are fundamental to a satisfactory issue. The paper points out what are the principles of those electric systems in use, and how they can be brought into line—what are the principles necessary to the establishment of a universal system. It is true the report made certain suggestions, and which I have very slightly commented upon. The conclusions at which I have arrived are my own, inspired by no one. I raise no question on the evidence of the eminent men who appeared before the committee. That evidence, so far as I am aware, has not been published. I am, I think, nevertheless, entitled to some credit for having set forth those points necessary to achieve the main object for which the committee was called together, and that which forms the sum and substance of their report, viz., the establishment of an effectual means of communication capable of universal interchange of stock.

In my remarks in reply to Mr. Winter, I expressed some surprise that an electrical communication of this character should work efficiently through contacts which had been served over with tar, sand, &c. Mr. Winter has been good enough to send me a pamphlet explaining the system of communication adopted by his late brother. In it I find details of certain experiments carried out at Royapoorum Railway Offices, Madras, on the 21st December, 1876, upon which, no doubt, this statement is based. The couplings were first served over with grease; then sand was sifted over this; and then—the grease being wiped off—the coupling hooks were served with Stockholm tar "of the "consistency of treacle." Through each of these the current passed.

Mr. Langdon.

Finally, some hooks—the contact portion of which had been coated with a composition of tar, &c., well dried—were set in position, and the chains set swinging. In this instance the motion broke off the hard coating, and then the current flowed. The solution of the difficulty is now clear. The hooks in question are very heavy: when put together, they, by their weight, excluded—squeezed out—the soft grease, and equally soft tar; while the same agency, combined with motion, broke off the hard glaze.

The late Mr. Winter's system is a two-wire system, but the circuit is an "open" one, closed only when a communication is being made. The communicating wires are carried through the links of the chains, and soldered on to the hook. The hook itself



FIGS. A, B.

requires to be somewhat modified in shape, and to be provided with a facing of copper at the point where the two hooks form contact. If such a system were applied to the British railways, it would involve the remodelling of some 300,000 coupling hooks and chains. It is, perhaps, needless to add that such a mode of coupling is wholly incompatible with a parallel or balanced-current system.

I have read Mr. Luke's remarks with much pleasure, and, in order to make the late Mr. Winter's system clear to all who take an interest in this question, I append drawings (Figs. A and B) showing the two modes adopted by Mr. Winter in joining up his communication. It will be observed that one provides for an "open" circuit, and that on the alarm being raised the signal is

made to the fore part of the train; that where the "closed" circuit arrangement is employed the batteries require to be put in circuit only during the time the train is in work, otherwise they would be needlessly used. The batteries are active throughout the progress of the train. The system is, I think, one which would hardly meet the needs of our crowded lines. Mr. Langdon.

I also attach drawing (Fig. C) showing the coupling as pourtrayed in Mr. Winter's pamphlet.

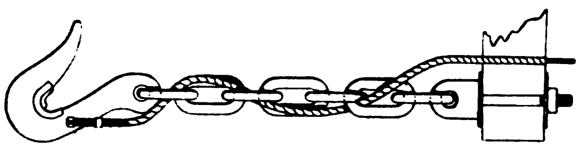


FIG. C.

With respect to Mr. Leonard's communicated remarks, the point he raises is important where guards' vans are introduced into foreign trains, but I imagine this is seldom the case.

The PRESIDENT: I have the pleasure of moving that we give our best thanks to Mr. Langdon for the important communication he has made to us The subject is not one of the largest, but it is one of very great importance, and Mr. Langdon's paper is a valuable contribution to it. The President

The motion was carried unanimously.

The PRESIDENT: The scrutineers have reported that the following candidates are duly elected:—

Members:

Henry Metcalf Hobart.	Mark Parker.
Ethelbert Thomas Ruthven Murray.	Godfrey Bernard Parlett.
	John Grice Statter.

Percy Still.

Associates:

Thomas Gooch Aldrich.
John A. Anson.
Joseph James Charles Bacon.
Dudley Eugene Batty.
Albert Edwin Bennett.
Walford John Benton.
James A. Blackwood.
Frederick John Borland.
Frederick Livingston Bowen.
Godfrey Thomas Bradley.
George Victor Braulik.
Denis Ripley Broadbent.
William Bullard.
William Beckitt Burnie.
Alfred John Edward Catto.
Arthur Blackburn Child.
Joseph Clarke.
Albert Fothergil Cooke.
A. Campbell Cormack.
Charles Coton.
Arthur Curtis.
Arthur Vivian Mervyn D'Arcy.
Alfred Henry Darker.
Frank Ernest De Guerrier.
Montague Charles Dent.
Athol Wilson Dixon.
W. E. D. Duncan.
O. Francis Francis.
William Richard Nash Grimley.
William Henry Hall.
Robert Whitehead Hammond.
Leonard Charles Harvey.
Telford John Sydney Hewson.
Robert William Hughman.
Thomas Hunter.
Alfred Járay.
T. B. Johnson.
Harry Jones.
John McBean.
Charles Frederick McInnes.
Neil McLean.
Herbert Leslie Mills.
George Frederick Alexander Norman.
Charles Oliver.
Alexander Flood Page.
Richard Cameron North Palairet.
Captain John William Parnell.
S. Rignold Pedroza.
William M. D. Pell.
Matthew Penson Plunkett.
William Joseph Polyblank, jun.
Charles Henry Powell, A.R.S.M.
David P. Reid.
William Riley.
Charles John Ford Sevier.
Charles Edward Skelton.
Deane Hervey Slack.
Charles Arthur Slater.
Archer Wellen Smith.
Albert Charles Soutter.
Leo Sunderland.
Frank Suter.
George Edward Taylor.
Frederick Robert Thackrah.
John Todd.
David Brown Walker.
George Weston.
Frederick Charles Havers White.
Albert Edward Wiggins.
N. G. Wigram.
Alfred J. Wilkins.
James Bonelle Willis.
W. Norman Withers.
T. H. Roberts Wray.

Students:

Robert Donald Thain Alexander.
Vero M. Allen.
A. L. Andrews-Speed.
W. H. Arundell.
Frank Camillo Norman Bergh.
H. F. Blake.
Herbert Edward Britton.
James Hally Brown.
J. F. Caine.
John Campbell Callander.
Constantine Calliphronas.
Henry R. Carson.
W. G. Carter.
Ernest Herbert Brodie Clark.
Percy Douglas Collins.
Allen Robert Crane.
Claud Crompton.
Herbert James Densham.
Francis Medforth Denton.
Evelyn Fawssett.
Reginald Forrest.
William Brantingham Giles, jun.
Robert Casper Goldston.
Percy Good.
Charles William Hill.
Francis C. Hounsfield.
Henry John Humphreys.
Thomas Cyril Hunt.
Philip Hunter-Brown.
Alfred John Hurst.
Francis Ernest Lloyd Hurst.
Arthur Priestley Hutchinson.
Thomas Leslie James.
Hammond Levene Everard Kennard.
William Mackintosh.
Frederick John Masters.
Sydney William Melsom.
Rupert Mondel Moberly.
Arthur Nicols Moore.
Donald Smeaton Munro.
Arthur C. Nash.
Ernest R. Nash.
Charles Nunn Nettley.
Charles Ernest Newton.
F. H. Nicholson.
Christopher Allan Phillips.
Arthur Noel Pitcairn.
J. St. Vincent Pletts.
Arthur Milnes Pooley.
William Roberts.
Norman Clive Sawers.
William G. Shee.
William Spencer.
Alfred Langrish Stephens.
John Hartrick Stone.
Piers Sumner.
Herbert Edward Tatham.
George Stamp Taylor.
Hedley Jeffreys Thomson.
Harry Turner Tovey.
Leonhard Vignoles.
Clement E. Vines.
Eric Victor Watson.
Henry Sinclair Watson.
Francis Victor Wimhurst.
Maurice Arthur Wood.

The Three Hundred and Twenty-first Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, Great George Street, Westminster, on Thursday, December 8th, 1898—Mr. J. W. SWAN, F.R.S., President, in the Chair.

The minutes of the Ordinary General Meeting held on November 24th were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council:—

From the class of Associates to that of Members—

Herbert Brandon White.
William Dieselhorst.
Charles Picton Martin.
Herbert Edward Mitchell
Thomas Blackwood Murray.
William Rowan Wilson.

From the class of Students to that of Associates—

G. L. Eynon.
Reginald Wilson Gauntlett.
John Robert Milnes.
H. R. Mott.
H. R. C. Partridge.
C. H. Rosoman.
William George Stuart.
J. F. Wakelin.

Messrs. Fairfax and Harrison were appointed scrutineers of the ballot for new members.

Donations to the Library were announced as having been received since the last meeting from Mr. T. Bashford; the Macmillan Company; the Scientific Publishing Company; Mr. H. Wilde, Honorary Member; Professor S. P. Thompson, Member; and Mr. E. K. Scott, Associate; to all of whom the thanks of the meeting were unanimously accorded.

The PRESIDENT: I have now the pleasure to call upon Dr. Lodge to read his paper.

IMPROVEMENTS IN MAGNETIC SPACE TELEGRAPHY.

By Dr. OLIVER LODGE,* F.R.S., Member.

Part I.—General Principles.

The general principles of electric space telegraphy—or wireless telegraphy, as it seems to wish to be called—are fairly old; and without going into the history of the subject, with which I need not trouble you, since it has been admirably summarised by Professor Silvanus Thompson in his lecture to the Society of Arts, I may recapitulate the three chief methods that have been tried. Dr. Lodge.

1. First and oldest, the earth current or leakage method, wherein a powerful current conveyed to the earth or sea by two well-separated electrodes causes a slight difference in potential between two other well-separated tapping electrodes at a distance, and thereby produces a feeble current through a joining wire, which can be detected telephonically or galvanometrically, or in any other convenient and known way. I am under the impression that this utilisation of earth conduction for the purpose of conveying messages has played a greater part in the past, and is perhaps destined to play a greater part in the future, than most people suspect. When, for instance, as in Mr. Langdon Davies's experience, phonoporic currents have been heard in very distant telephone trunk lines, it is difficult to be sure how much of the effect is due to a true induction from wire to wire, and how much of it is due to conduction—not only conduction through the soil from earth-plate to earth-plate, but conduction also through metals, whether gas pipes or overhead wires or other conductors arranged for other purposes. Whenever an open circuit is employed, with earthed terminals, conduction is liable to assist induction; and this may very likely be regarded as an advantage. It may also be to some extent a disadvantage, owing to its resulting in the mixing of messages; it may, in fact, contribute to ordinary telephonic cross-talk.

* Assisted by Mr. Benjamin Davies.

Dr. Lodge. 2. The second method that has been tried for purposive space telegraphy is the method by magnetic induction between two insulated line wires stretched parallel to one another along opposite coasts. This method, as is well known, has been tried under the auspices of the British Post Office, and is known as Mr. Preece's method. It is, I think I am right in saying, the only system in regular remunerative work; and near Cardiff, between Lavernock Fort and the neighbouring fort on the island of Flat Holm, I have myself seen it at work, and have admired the ingenious system of "call" devised by Mr. Evershed and there employed. I desire to say that when I gave a short preliminary note to the British Association at Bristol this year, on a "call" that I had invented, I was unaware of Mr. Evershed's work in the same direction. He has undoubtedly achieved the result there aimed at, though with the expenditure of a good deal of power; and had I known, I should certainly have referred to it in my communication at Bristol. But after the Bristol meeting Mr. Preece gave me permission to visit Lavernock, and I went in company with Mr. Partridge, the telegraphic superintendent at Cardiff, to whom also I am obliged.

3. In the third system of space telegraphy—that by Hertz waves—the public owes its knowledge and interest likewise to Mr. Preece; for, had Mr. Preece not taken up the subject, very few persons would, in all probability, have heard of Hertz waves and Branly detectors and coherers to this day, notwithstanding the amount of work that had been done on them, and made known to scientific bodies, not only in this country,* but also by Righi, by Bose, and especially by Popoff. As it is, owing to Mr. Preece's great influence and power of lecturing, this third and most recent method, since he took the subject up in 1896, has become the best known of all; though, unfortunately, in such a form that it is generally supposed in unscientific circles

* See, for instance, *Phil. Mag.*, January, 1894; also my little book, published in 1894 by the *Electrician* Co., wherein, in the second edition, a history of the coherer principle is printed as an appendix; with omissions, however, through ignorance, of the excellent work done in actual telegraphy on this method by Professor Popoff in Russia in 1895, and by Captain Jackson in the South of England.

that the discovery was made in Italy. I do not intend to refer Dr. Lodge. to this interesting method again to-night, but before leaving it, it may conduce to clearness if I explain wherein the Hertz-wave method—at which I have also worked, in conjunction with Dr. Alexander Muirhead—differs from the older magnetic inductive method of Mr. Preece, Willoughby Smith, and others.

4. Every alternation in a suitable medium produces waves in one sense, but the intensity of those waves is immensely dependent on the rapidity of the alternation. A rapid pulsation of air, in and out of a hollow vessel like a resonator, may cause an extremely loud sound; a slow pulsation, of the same or even of much greater amplitude, would cause no perceptible effect, beyond an alternate rise and fall of a sensitive barometer.

Again, a reed vibrating slightly but quickly is audible at a distance, while a fan vibrating strongly but slowly is not audible at all. The waves emitted by the one are strong, by the other are exceedingly weak. A magnetic alternation of sufficient rapidity induces an alternating electrostatic field, and is therefore accompanied with emission of energy. A slow magnetic alternation is practically unaccompanied by any electrostatic effects, and so practically all the energy emitted at each pulsation returns again to the source. For a progressive wave must necessarily have half its energy static, and half of it kinetic energy; whereas in ordinary current-induction all the energy is kinetic.

5. Interrupting the explanation for a moment to touch on a practical point: It might seem, from what we have so far said, as if all the advantages lay with the rapid vibration or wave method. But when really large distances are contemplated, or when obstacles intervene, its advantages diminish and ultimately disappear; for slow magnetic pulsations are independent of all obstacles, except actual iron, if they are slow enough, and are only slightly weakened by conductors; whereas rapid pulsations or true waves are stopped by conductors and are greatly affected by obstacles. It is probable, as I have hinted above, that earth-conduction plays a considerable part in some of the coherer signalling which is supposed to occur by means of waves in space; and in all cases it is a possibility to be constantly borne in mind.

Dr. Lodge. The coherer is a detector not only of true electric waves transmitted by free ether, but also of electric jerks transmitted by the earth or by uninsulated conductors such as gas pipes, and the response of a coherer is no conclusive demonstration of the receipt of actual Hertz waves.

6. To detect the radiation of energy, something capable of responding to waves, such as an ear, an eye, or a coherer, is appropriate; to detect the fluctuations of pressure or of magnetic field, something akin to a barometer or magnetometer would seem to be appropriate; but, inasmuch as these instruments only indicate the state of things at their immediate locality, another device can be employed which shall integrate the effect over a considerable area and therefore be much more sensitive. Such a device is afforded in the case of magnetism by Faraday's current-induction. A circuit connected with any electric detector, such as a telephone, may surround a large area and give an induced current depending on the variation in the total lines of force passing through the entire area.

7. In the Lavernock signals, considering them as due to wire induction, the inducing and induced areas are both long narrow strips in a vertical plane, with their long sides horizontal and their faces parallel; each being constituted by the space between a long overhead line on posts and the return earth—or, rather, sea—circuit.

But in some interesting experiments made by Mr. Stevenson near Edinburgh, with the co-operation of the officials of the Post Office, he employed a horizontal coil of wire approximately circular, and by means of a battery and key at one station was able to hear Morse signals in a telephone in another like coil half a mile away. This certainly resulted from pure induction, and it may be considered to represent the closest approach hitherto made to the method I have to bring forward to-night.

Unfortunately, Mr. Stevenson made a slight error in the theory, and perhaps for that reason did not continue the attempt over larger distances. He says that with a given number of ampere-turns the hearing distance increases with the square root of the diameter of one of the coils, or with the simple

diameter of both coils, thus making it necessary to double the size, quadruple the area, of each coil if the distance is to be doubled; and, since he used 1,600 yards of wire in each coil to signal half a mile, he may have considered the cost prohibitive for any really great distances, as, indeed, it is, with mere unaided coil induction. Dr. Lodge.

But, in truth, the law of distance is not the square root of the diameter, but the two-thirds power, *with a given primary current*; and so doubling the circumference of each coil will enable signalling over more than double the distance, if other things can be kept the same.

It is true, however, that for such magnification the thickness of the wire must be magnified likewise, or else more power will be consumed in the enlarged coil; and this consideration, together with others shortly to be stated, does speedily make the cost prohibitive, unless some fresh revolutionary devices are employed.

8. The devices I have to lay before you to-night contain nothing new in principle—*i.e.*, they involve no discovery—but, inasmuch as they make practical over big distances what has been actually successful over small distances, I venture to attach some importance to them, and think them worthy of your attention.

First I have to explain my system of magnetic induction telegraphy, and next the extra sensitive receiving instrument which may be employed with it.

The chief feature of my magnetic system is the outcome of an electric resonance experiment which I described eight years ago (see *Nature*, vol. xli., p. 368), viz., the experiment of the syntonic Leyden jars. The term "resonance" is generally and properly understood as having an acoustic significance, and where no reference to sound is intended I advocate the use of the more general term "syntony" to denote the sympathetic response between two vibrators attuned to the same "note," or more generally to the same frequency of vibration, whether that vibrator be mechanical or electric or magnetic.

Two similar Leyden jars connected to similar circuits are, or may be, thus syntonised, and any electric oscillation in one causes a feebler electric oscillation in the other; the medium of

Dr. Lodge. communication in this case being the alternating magnetic field between the two circuits, the lines of force of the one circuit penetrate the other, and by their fluctuation cause an alternating electro-motive force of feeble intensity. In a distant circuit *not* containing a Leyden jar, and therefore not susceptible of any tuning, because it has no natural frequency of vibration, nothing can be perceived; but when a Leyden jar exists in a circuit, that circuit has at once a natural frequency of vibration which may be syntonised with that of the emitting circuit, and

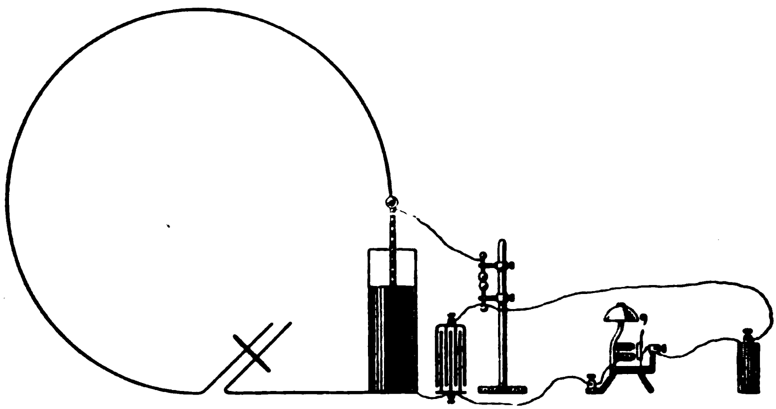


FIG. 1.—Receiving Circuit in the Syntonic Leyden Jar Experiment.—The expanded loop circuit of the jar should be of thick copper wire, thoroughly connected up, and with a slider for tuning by alteration of effective length. The pair of knobs on a glass stem are screwed and clamped into exceedingly light contact; and the battery and bell are shunted out, so far as oscillations are concerned, by an ordinary condenser of large capacity.

in that case the intensity of the reverberating induced electromotive force may rise as high as several thousand volts, and cause a spark over a very perceptible air gap; which spark could either be directly seen, or be detected when extremely minute by its cohesive action on a pair of knobs, almost in contact and in circuit with an electric bell, or a galvanometer.*

Fig. 1 shows this experiment so far as the receiving circuit is concerned. The sending circuit is a similar jar charged by a

* See *Journ. Inst. Elect. Eng.*, 1890, page 352; also "The Work of Hertz, &c.," second edition, page 65.

Wimshurst machine, and discharged round a similar expanded simple circuit through an ordinary half-inch spark gap. After each spark the pair of knobs require a tap to break the cohesion and restore the receiver to sensitiveness; but if the electric bell rests one of its feet on the stand of the knobs, or sometimes even if it stands on the same table as they do, without cotton-wool isolation, its tremor will automatically effect the necessary tapping-back and reduce the ringing to a single stroke at each spark. Dr. Lodge.

9. Now for telegraphic purposes I replace the Leyden jar by a condenser of considerable capacity, and the circuit of the jar becomes a large horizontal coil of wire—it may be of one turn or it may be of more; and the frequency of vibration, instead of being the million or so per second appropriate to Leyden jars, becomes the thousand or even hundred per second appropriate to sound. Hence, instead of using a spark gap or a coherer to detect the disturbance, I use an ordinary telephone as a shunt to the receiving condenser. Further, although the momentary oscillation of a spark constitutes a possible sender, I generally employ some form of alternating or intermitting machine of considerable power as the sending appliance, and carve its fluctuating current into long and short spells, as everyone else working at the subject has done.

I was impressed with the advantages possessed by horizontal coils of considerable area, for big distances, before I was aware of Mr. Stevenson's work. I wish to call the attention of the Institution to those perhaps forgotten experiments of Mr. Charles A. Stevenson, B.Sc, F.R.S.E., communicated to the Royal Society of Edinburgh in March, 1894, as the outcome of his former work; for they appear to have escaped the attention even of Professor S. P. Thompson himself, than whom I know no one better acquainted with what has been done in any department of electrical science, and, indeed, in several other branches of physics.

I have at my house a cable a quarter of a mile long altogether, enclosing an elongated rectangle say 150 × 30 square yards, and with that I can hear all that goes on in the neighbouring telephone wires, and can even answer back with a suitable microphonic transmitter; but, there being, I suppose, no phonopore in

Dr. Lodge. the neighbourhood, I hear no stray musical note; the only musical note I hear is the one belonging to the desired signals when they are being sent to me from an equal length of cable round an equal but squarer contour at the College about two miles away, or 2·5 kilometres in a direct line.

10. I may say that, although the Leyden jar or condenser constitutes a leading feature of my system, I have occasionally, as an experiment, dispensed with it, and tried what could be done with the area-enclosing circuits alone. But under those conditions, with the power I had at my disposal, I was not usually able to hear anything. I was using a small low-frequency dynamo of the Pyke & Harris pattern, and at the sending end had a current of 10 or 12 amperes going round the circuit, sometimes as much as 25 amperes. Under these circumstances, on the ordinary National Telephone Company's telephones the sound could be heard many miles away: away over in Cheshire, towards the Dee, the signals could be clearly heard, while close to the College the subscribers, unfortunately, became rampant; but through empty space the signals only became audible at a distance of two miles when a condenser was used and the circuit properly attuned. I am speaking now of the early trials when no suitable receiving telephone was employed, but only the first to hand. The cable, bought for the purpose of the trial, was described as anti-inductive telephone cable (not exactly the variety required), and it was very badly insulated, having been bought as little more than old materials; but still it might, perhaps, have been expected to do better. Directly the circuits were syntonised, however, there was no difficulty in hearing the note and the signals. I should say that I did not work with the fundamental frequency of the dynamo—128 per second, or thereabouts—but with the third harmonic, which is fairly strong in these Pyke & Harris alternators, of frequency 384. Such a note is pleasanter to listen to than a deep bass, the ear is far more sensitive to it; and, moreover, to attune the circuit to the lower frequency would have entailed a prodigious capacity. As it was I used 28 microfarads, and had to insert the thick wire of a hedgehog transformer in

the circuit too, in order to get the induced circuit frequency as low as 384 with the quarter-mile length of cable once round. Dr. Lodge.

I generally used a couple of Ader telephones in series, each of about 70 ohms resistance, and connected them thus as a shunt to the receiving condenser.

The resistance of the receiving cable itself was only 0·07 ohm (it consisted of 100 No. 18 wires all in parallel), so that if the telephone had been in series with it a very low resistance winding would have to be employed; and in the early trials I did not happen to have such a telephone handy. I did, however, use a Western Electric loud-speaking telephone of 2 ohms resistance occasionally. A transformer, of course, was sometimes used, but it wastes power. Besides, the low resistance of a transformer winding is apt to be rather illusory when its higher resistance secondary is closed.*

11. At the sending end also condensers were employed, in series with dynamo and coil, in order to strengthen by resonance the third harmonic at the expense of the fundamental. The cable there was about 400 yards of well-insulated telephone cable, consisting virtually of 20 strands of separately insulated No. 16 wire, slung on poles round a squarish contour; but, unfortunately, it was again sheathed in lead-foil so as to be nominally "anti-"inductive."

I found it best usually to join these 20 wires in five series of four strands each; and the current in each strand being then, say, 6 amperes, I call the whole current in the cable 24 amperes.

The receiving cable I could also at first employ in separate strands, for its 100 wires were supposed to be insulated with thin

* By this I mean that the real effective resistance of a primary coil surrounded by a closed secondary is well known to be greater than R, its value, when isolated, being equal to

$$R + \frac{p^2 M^2 S}{S^2 + p^2 N^2};$$

and since p N is in a transformer usually much greater than S, and since the windings, n and n' turns, are upon the same core, this becomes approximately

$$R + \left(\frac{n}{n'}\right)^2 S;$$

so that a very appreciable fraction of the resistance of the secondary circuit adds itself to the resistance of the primary. External self-induction in the secondary circuit, like that of a telephone, goes to reduce this addition.

Dr. Lodge. india-rubber. The first time I heard the note I had 19 of these strands in series without a condenser, constituting a coil of 19 turns, connected to an Ader telephone. Since then, however, the insulation has so much broken down that I am constrained to use all the wires in parallel, which for many purposes is not so convenient.

Advantage of Syntony.

12. Now proceed to consider more particularly the elementary theory of magnetic induction telegraphy between two large distant horizontal coils, first without, then with condensers; taking the case, however, of an alternating current, and not the simple make-and-break battery arrangement employed by Mr. Stevenson, which obeys somewhat different laws.

I do not feel sure that all engineers fully appreciate the effect of syntonising or tuning an electric circuit to the frequency of its alternating dynamo. The consequence of introducing a condenser into a circuit fed by an alternator (not as a shunt to the contact-breaker as in an ordinary Ruhmkorff, but in series with both coil and dynamo) was first noticed experimentally by Sir W. Grove in connection with a Ruhmkorff coil (see *Phil. Mag.*, March and May, 1868), and an explanation was promptly given by Clerk Maxwell. The matter at that time was one of interest and novelty; now it is tolerably familiar to electrical engineers; but even now a few words illustrative of the effect of condenser—*i.e.*, capacity—introduction, such as serve to bring the matter home to students, may not be out of place.

The mechanical analogues of the three electrical magnitudes, self-induction, resistance, capacity, are respectively inertia, viscosity, elasticity. Hence, to make a mechanical model of an alternating circuit without condensers, one might think of a more or less massive truck on level rails, pulled and pushed to and fro, say, by an alternating piston. To vary the resistance the brake may be put on and off, or the truck may be immersed in a viscous fluid like treacle; to increase the self-induction the truck may be loaded, *i.e.*, made more massive; to represent the insertion of capacity in the circuit

we must provide the truck with an elastic recoil tending to restore it to its mean position. Thus it may be put between a couple of pair of opposite long-range spring station buffers, or the rails may be curved upwards so that the mean position of the truck is at the lowest point. It has now—what it had not before—a natural periodicity of oscillation, and if disturbed it will swing to and fro, so many times a minute, before returning to rest. Under these conditions it has become much easier to move to and fro. Whereas previously its rapid oscillation could only be maintained by a very forcible alternating thrust, all the energy being dissipated each time, and there being nothing to help the mass back, now the energy is partly stored in the springs, and the truck requires hardly any force to bring it back. Nor does it require much force to push it forward again, for its inertia has caused it to overshoot its mean position and store up some energy ready to aid the next forward movement. So even a boy, if he applies his force at appropriate intervals, will be able to set the truck gradually into a state of violent oscillation, straining the springs to their utmost capacity; being aided to strain the strong springs by the inertia of the truck, and being aided to overcome the inertia of the truck by the elastic recoil of the springs. In maintaining this movement all the energy he has to supply is that which is wasted in friction; and with well-oiled bearings that may be very small. If the friction is considerable, the advantage of syntony is diminished, and the possibility of getting violent oscillation at the expense of small power is removed. If the inertia is small it is easy to get up the swings, but they are no longer violent; hardly more violent than they might be without syntony. Dr. Lodge.

All this is exactly represented in the electric circuit. The analogue of the force of the springs is the voltage or potential difference between the terminals of the condenser; the analogue of the current in the circuit is the velocity of the truck as it rushes past its mean position. Without capacity the oscillations in a circuit of considerable self-induction are feeble; the impedance is too great. Without plenty of self-induction the magnification of voltage at the

Dr. Lodge. right frequency is insignificant. But in a circuit of large self-induction and small resistance the violence of the electric oscillation maintained by small power can be something gigantic; and it would be possible to construct a massive copper coil able to multiply the applied voltage by several thousand, so as to burst any ordinary condenser of the right capacity which may have been rashly introduced into the circuit.

As a matter of fact, I have at different times burst many condensers in this way, notwithstanding that I put several of them in series to increase their strength; strong sparks and dangerous shocks can be taken from the terminals, and to anyone who has not yet seen this magnification of gentle voltage the experiment may be of interest, but it takes a considerable quantity of copper (*cf.* sections 25–29).

Taking up the parable again and pursuing it into detail, we may say that the effect of the resilience of the springs is practically to diminish the impedance or inertia obstruction of the truck, making it easier to move it in an oscillatory manner; but whereas with weak springs the improvement is only slight, with springs which are much too stiff there need be no improvement at all. With a certain elastic resilience in the springs the improvement is a maximum at a certain frequency, but there is *some* improvement over a considerable range on either side of this. When the friction is small the maximum is very sharp, rising to a high peak if plotted as a curve; and the resilience which brings about this maximum is exactly that which confers on the truck a natural period of free oscillation corresponding to the period of the applied alternating force. Under these conditions the truck is as easy to vibrate as a thing with no springs or inertia at all—nothing but friction.

I interpolate this elementary explanation because it has been represented to me that I have not sufficiently illustrated and enforced the advantage of the introduction of a syntonising capacity into both sending and receiving line, which (along with the magnifying telephone and the tuning up of every part) constitutes the essential feature of my system, and its advance (so far as I know) on what has been done in telegraphy before.

The first proposition I wish to maintain is that no unaided simple induction process without syntony can work satisfactorily over really big distances unless an altogether prohibitive amount of wire or an extravagant amount of power is employed. Dr. Lodge.

This proposition I prove by considering the strength of current induced in the distant circuit. Let a and b be the radii of the two coils, considered circular, n and m their turns of wire, and r their distance apart from centre to centre, this last being considerable: then the mutual induction coefficient—*i.e.*, the total lines of force passing through one coil due to unit current in the other—is

$$M = \frac{n \pi a^2 m \pi b^2}{r^3}.$$

The resistance is

$$\frac{2 n \pi a}{\pi c^2} \rho \text{ for one, and } \frac{2 m b}{d^2} \rho \text{ for the other.}$$

The self-induction is

$$4 \pi \mu n^2 a \left(\log \frac{8a}{c} - 2 \right);$$

or, for present approximation—accurate if one coil is like the other magnified—*i.e.*, if they are really "similar coils"—the self-inductions are

$$k n^2 a \text{ and } k m^2 b.$$

Now, if E is the electro-motive amplitude maintained in the primary, the current-amplitude induced in the secondary is

$$x = \frac{p M E}{J_1 J_2},$$

where J_1 and J_2 are the impedances.

For rapid alternations and good stout wire the value of J is not much bigger than p L; hence the most favourable case for the mere pair of circuits without condensers gives

$$\frac{x}{E} = \frac{M}{p L_1 L_2} = \frac{n m \pi^2 a^2 b^2}{p n^2 m^2 k^2 a b r^3},$$

which is, therefore, the "virtual conductance" of the whole system.

The first and most important thing we observe here is that p occurs in the denominator, so that high frequency is actually

Dr. Lodge. disadvantageous—other things being equal, which they usually are not, because high tones are best to listen to.

The next thing to notice is that number of turns of wire occurs in the denominator, so that a single contour is much better than a coil of many turns,—except in so far as number of turns helps to emphasise self-induction at the expense of resistance, and so to bring about the above "most favourable" conditions where resistance can be neglected.

Lastly, we observe that the linear dimensions of each coil occurs only once in the numerator, and that their product is therefore incompetent to cancel the cube of the distance in the denominator, when both wire and distance are magnified in the same proportion, and when both are big. Observe, we are not here taking the case of a *given current* in the primary, but the more practical case of a given E.M.F.

Moreover, we see that extra thickness of wire gives no advantage: if the wire is thin, the induced current will be less than the above; but no increase of thickness can make it greater than the value of x now calculated, viz.:

$$x = \frac{\pi^2 a b \mathrm{E}}{k^2 p m n r^3}.$$

Hence I say that no simple induction system can work over really big distances unless an altogether prohibitive amount of wire or an extravagant amount of power is employed. If for a certain distance a total length of wire, l, is just sufficient, it is best disposed in equal contours of one turn each; and the ratio between the induced secondary current and the applied primary E.M.F., without allowing anything for the impedance of the receiving telephone, is then

$$\frac{x}{\mathrm{E}} = \frac{l^2}{16 k^2 p r^3} \quad \ldots \quad \ldots \quad \ldots \quad (1)$$

To signal double the distance $2\sqrt{2}$ times the length of wire is necessary; to signal 10 times the distance the necessary length of wire is 32-fold, its thickness being increased at the same time so as to keep resistance down.

13. *Introduction of Condensers.*—But now, directly condensers are introduced into both sending and receiving circuits,

and are adjusted until each circuit is attuned to the applied Dr. Lodge.
frequency, the conditions are greatly altered. When the correct note is reached all self-induction is abolished, and the impedance becomes simple resistance: see equation (3) in Part II., below. Hence in this case the induced current is

$$y = \frac{p \text{ M E}}{\text{R}_1 \text{ R}_2},$$

or

$$\frac{y}{\text{E}} = \frac{p\ a\ b\ \pi^2\ c^2\ d^2}{4\ \rho^2\ r^3}.$$

The most important thing to notice here is that p occurs in the numerator, so that high frequency is specially advantageous. This raising of the large number p from denominator to numerator would represent, if everything else remained the same, more than a million-fold advantage over no tuning—the actual advantage is not so great as this, but it is great. The number of turns of wire employed in each coil is now immaterial, instead of being objectionable as in the former case, and this fact is often convenient for tuning. And a great gain results from increasing the thickness of the wire, wherever a big distance is to be attempted.

If the whole bulk of wire employed is W, and if it be distributed equally between the two stations, in a single turn at each, the ratio between the secondary induced current and the applied primary E.M.F., again without allowing for the resistance of the receiving telephone (its "reactance" has been already neutralised if it is connected in series), is in this case

$$\frac{y}{\text{E}} = \frac{p\ \text{W}^2}{64\ \pi^2\ \rho^2\ r^3} \quad \ldots \quad \ldots \quad \ldots \quad (2)$$

Comparing this with (1), which is the corresponding expression without syntony, the chief difference consists in the position of p; but this is a tremendous difference—representing something like a million-fold factor.

14. To take a numerical example in illustration of the two cases. Let the applied E.M.F. be 100 volts, the frequency 400 per second, the total length of wire, say, 2 kilometres, and its thickness 2 centimetres (about 5 tons of copper altogether): then

$k = 125$ about, $p = 2{,}500$, and $\rho = 1{,}600$ sq. cm. per second.

Dr. Lodge.

So at a distance of 100 kilometres the maximum received current possible, by simple induction between wire circuits without condensers, is

$$x = \frac{4 \times 10^{10} \times 10^{10}}{61 \times (125)^2 \times 2{,}500 \times 10^{21}} \text{ C.G.S. units,}$$

or 0·0064 *micro*ampere; which is quite insufficient to affect a telephone. That is the result in the first case; whereas, in the second case, with the same wire circuits, but with the addition of condensers and with both circuits properly attuned, the current might be

$$y = \frac{2{,}500 \times (6 \times 10^6)^2 \times 10^{10}}{640 \times (1{,}600)^2 \times 10^{21}} \text{ C.G.S.}$$

or 0·05 *milli*ampere, which is a fairly strong telephonic current.

Induction *v.* Conduction.

15. As to the question between space induction and earth conduction, and the relative part each plays in any given case, it is a question which can be answered either by experiment or by calculation. In some cases experiment would be absurd, as for instance when either sending or receiving circuit is not earthed anywhere; in other cases, as at Lavernock, it can be answered either way. Now Mr. Gavey tells me that he has tried an experiment on this very point, by taking an insulated wire outstretched between two boats and "earthed" in the sea at either end. Signals being sent from the land, they could be heard out at sea to a certain distance when the wire was above water, but when it was immersed they disappeared at a smaller distance. The argument, of course, is that, regarded as the tapping electrodes of a conductive receiver, the ends of the wire acted similarly both times; but regarded as an inductive receiver responding to magnetic influences, the submerged wire was protected, because, let us say, the direct and return portions of the circuit, when the wire was immersed in the sea, were more or less coincident. And the moral is that induction contributed the greater part of the effect. It is not quite conclusive, however, because induction and conduction may aid each other anyhow,

and, when either is stopped, the residual disturbance may be too faint to hear. A trial with a continuous current might, I think, conclusively measure the part due to conduction. So far as I know, no attempt has been made to dispense with earth conduction altogether, by the use of a return wire along the beach; but calculation shows sufficiently that if this were done signalling would practically cease. It is a mistake to suppose that the negative result with the submerged wire depends on the reflexion of electro-magnetic waves at the surface of water, because true electro-magnetic waves have in this installation nothing to do with the case. The water may and does to some extent act as a conducting screen, but it is sufficient to consider the immersion of the wire as resulting in simply closing up the receiving circuit until it is a long thin rectangle of inappreciable area, so that the direct and return circuits almost coincide. The experiment proves distinctly that induction has something to do with the result, and that it is not wholly due to earth tapping. Let us see if calculation bears this out. Dr. Lodge.

16. Without presuming to quote exact details of an installation on which I am not authorised to report, I state only the facts obvious to inspection of the stations by boat.

The distance between the earth-plates, or length of the base line, I take to be: At Lavernock, 1 mile; at Flat Holm, $\frac{1}{3}$ mile. The average elevation of the line wire I take as 120 feet above the sea in each case; the elevated lengths as 1,200 and 460 yards respectively; and the distance between the stations as $3\frac{1}{2}$ miles.

It may be noticed that in any case, even in this case where the line is straight, the bulk of the impedance is of the "react-"ance" kind—that is, p L is bigger than R—when a thick wire and high frequency are employed.* For, whereas the resistance of the Lavernock line might be something between 1 and 2 ohms, the value of p L is something more like 10 ohms if a note of 400 per second is employed; and in such a case the induced

* I emphasise this because it has been doubted by the Post Office. A method for calculating the self-induction of a straight wire of length l and sectional radius c is given by Lodge and Howard in the *Phil. Mag.*, July, 1889, page 64,

Dr. Lodge. current is almost independent of the frequency, because $\frac{p\,M\,C}{p\,L}$ = constant as regards frequency.*

Hence, with a stronger primary current and lower frequency—say 20 amperes alternating 20 times a second—the effect received would be decidedly stronger; whereas, if *resistance* had been the dominant feature, the effect of the stronger slower current would have been just the same as that of the smaller quicker one. It is chiefly for response at low frequency, I take it, that such thick copper wires have had to be employed; for, at low frequency, resistance does become the dominant feature.

17. Now, treating the sea first as if it were a superficial return circuit, or, rather, treating the case of a return wire along the beach, vertically below the line wire, the mutual induction coefficient between two facing circuits of areas 40 × 1,200 square yards and 40 × 460 square yards respectively, at a distance of $3\frac{1}{2}$ miles, is

$$M = \frac{2 \times 4{,}800 \times 18{,}400}{(6{,}160)^3} \text{ yards},$$

or say 0·008 yard, or 0·73 centimetre.

Accordingly, when a current of frequency 400 and 1 ampere mean strength is sent round either circuit, an E.M.F. is induced in the other of average value,

250 M c.g.s., or 1·8 micro-volts.

and results in $L = 2\,l\left(\log\frac{4\,l}{c} - \frac{3}{4}\right)$. Apply this to the Lavernock line wire: say l = 1 mile, $c = \frac{1}{8}$ inch, we get L = 4 milli-henries; so for a note of 400 per second p L = 10 ohms; and this is the practical impedance of the straight wire,—its resistance being much smaller. The capacity needed to tune the Lavernock line wire alone to a frequency of 400 per second would be about 40 microfarads. It is a great mistake to suppose that in straight wires the self-induction is negligible.

* This is important and interesting. It is very noteworthy how the current given by an alternating dynamo in a circuit of low resistance can be independent of the rate at which the dynamo is run, within very wide limits. A small experimental dynamo with iron in its armature, kindly built for me by C. Parsons & Co., Newcastle-on-Tyne, which is able to be run up to a frequency of 3,000 per second, gives a current through a half-ohm Siemens electro-dynamometer which remains constant within 2 per cent. from 120 per second up to the highest possible; the total resistance of the circuit being 1·21 ohms The range of speed within which this constancy holds is from 240 revolutions to 6,000 revolutions per minute.

And I doubt if this could produce any appreciable effect; unless, indeed, the whole of the receiving circuit were attuned and arranged in the way to be presently described. Dr. Lodge.

But a return circuit along the beach is not employed, and would evidently be very ineffective if it were. A return by the sea gives a more open area, and therefore an inductive advantage, in addition to any conductive contribution which it may furnish.

18. The other extreme case, decidedly more unfair in the opposite direction—so extreme that it is hardly worth consideration except as a warning not to reckon the mutual induction in this way—would be to treat the line wires as if isolated in space —bits of infinite parallel wires—the return current being at infinity. Taking this case for a moment, the mutual induction would be

$$M = \frac{1,200 \times 460}{6,120} \text{ yards},$$

or about 80 metres, which must be far beyond the truth.

Mr. Heaviside, in his "Electrical Papers," vol. ii., page 502, or *Electrician*, 21st July, 1889, gives a correction for the ends of the wires which, for the case of a uniform return throughout space, amounts to a subtraction of 40 metres from the above result; but every constriction or limitation of the return circuit to the space near the wire would go to increase the subtrahend, and so to reduce the above value.

The actual return circuit depends on the distribution of conductivity in the earth, and is therefore indefinite. The fact that the resistance of the circuit varies with the height of the tide shows that the *sea* conveys most of the current.

Earth Conduction.

19. Now, calculating what share earth conduction ought to contribute in the matter, we may either treat the shallow sea as a conducting sheet fairly insulated in its bed, or we may postulate equal conductivity through the whole earth; the latter being probably further from the truth than the former.

Considering the electrodes as approximating in area to spheres

Dr. Lodge. of radius c, at a distance $2a$ apart, the applied difference of potential between them is

$$V = \frac{Q}{\pi k \delta} \log \frac{2a}{c};^*$$

k being the conductivity, and δ the depth of the sea, supposed uniform and small, and Q being the quantity of electricity supplied per second.

The potential at any point distance r from one electrode and r' from the other is

$$U = \frac{Q}{2\pi k \delta} \log \frac{r'}{r};$$

so between two fairly neighbouring points situated symmetrically on either side of a point at a considerable distance r from either electrode,

$$dU = \frac{Q}{\pi k \delta} \cdot \frac{dr}{r}.$$

The distance between these tapping points being $2b$, we have

$$\frac{dr}{b} = \frac{a}{r};$$

therefore the ratio of the tapped voltage to the total applied voltage is

$$\frac{dU}{V} = \frac{\frac{ab}{r^2}}{\log \frac{2a}{c}}.$$

Now, taking c as a metre, $2a$ as a mile, $2b$ as $\frac{1}{3}$ mile, and r as $3\frac{1}{2}$ miles, we find that the tapped E.M.F. is a certain fraction of the difference of potential on the primary electrodes; the fraction being,

$$\frac{\frac{1}{12 \times (3\frac{1}{2})^2}}{\log 1{,}500} = \frac{1}{148 \times 2.3 \times 3.18} = \frac{1}{1{,}080};$$

which represents a very considerable voltage. In that case, for every volt applied to the earth-plates at Lavernock, a millivolt will be tapped at Flat Holm, and *vice versâ*, by straightforward marine conduction.

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*See, for instance, Foster and Lodge, *Phil. Mag.*, June, 1875, p. 453.

20. But this is the most favourable case, with the current limited to a sheet of sea only a few metres thick. An uncertain amount of current would be certainly lost by real earth conduction, and the other extreme case would be to take the whole earth as of uniform conductivity with sea water. This would give us very much too low a result, as the hypothesis of sea conduction alone has given us one too high. Considering the whole earth as conveying the current, half the tapped E.M.F. is Dr. Lodge.

$$U = A\left(\frac{1}{r} - \frac{1}{r'}\right) = A\,\frac{dr}{r^2} = A\,\frac{a\,b}{r^3},$$

while the applied E.M.F. is

$$V = 2\,A\left(\frac{1}{c} - \frac{1}{2\,a}\right) = \frac{2\,A}{c}\ \text{approximately;}$$

so the fraction $\dfrac{d\,U}{V}$ in this case is $\dfrac{a\,b\,c}{r^3}$, a fraction so small as to give an almost imperceptible result—viz., a microvolt tapped at one station for every volt supplied at the other. But this could be easily and cheaply increased by an increase in the size of the earth-plates.

20*a*. In case any difficulty should be felt about the calculation of the mutual induction coefficient M for coils of any shape, it may be well to say that so long as the coils are thoroughly distant from each other there is no trouble at all; the matter is merely one of effective enclosed area. The "magnetic moment" of a primary coil of any plane shape is $m = n\,A\,C$, where A is the area which each of its n turns encloses, and C is its current. The intensity of field which a magnet of this moment causes at a large distance r is, in the axial direction $2\,m\,/\,r^3$, and in the equatorial direction $m\,/\,r^3$.

The secondary coil, of effective area $n'\,A'$, integrates this magnetic field, and displays it in its receiving instrument. So the mutual induction coefficient is $M = n\,n'\,A\,A'\,/\,r^3$ multiplied by either 1 or 2 according as the coils are in the same or parallel planes, but quite independently of shape.

[*End of Part I.*]

Dr. Lodge. *Part II. (Experiments and Calculations).*

21. I will relate briefly a selection from some of the early experiments made on simple wire circuits, at first often without condensers, in order to find what sort of value of mutual induction coefficient (M) was sufficient to give audible signals between a couple of coils of wire lying horizontally in the same plane.

(1.) Two coils of thin wire, each of 350 turns, and 30 cm. (1 foot) diameter, with 5 amperes maintained in one at a frequency of about 100 by an alternating dynamo; a telephone connected to the other coil responded audibly when it was at 6 metres distance. The M in this case is 287 C.G.S., which is large, but no capacity was used to assist.

(2.) Two coils of No. 15 wire, each of 8 turns, on a hoop 92 cm. (1 yard) in diameter, and with 11 amperes maintained in one at a frequency of 120 by means of a make-and-break tuning-fork; a telephone in the other responded at 3½ metres. In this case the M = 67 C.G.S.

(3.) A pair of coils in separate buildings 100 metres distant, each with a condenser, the transmitter being 60 turns of No. 18 wire round a small room nearly 4 metres square, the receiver being one of the above hoops a yard across wound with 8 turns of wire; the hum could be just heard. The M in this case is only 0·6 C.G.S., which is good.

(4.) The next experiment is worth quoting in fuller detail. It was made in 1897, a month or so later than the above called (1), when things had improved. Fig. 2 is a diagram of the connection at the sending end; the dynamo being replaced by a tuning-fork intermitter. (All kind of intermitters have been used in different experiments; one of the most interesting being a monochord plan devised by Dr. Pupin in America (*Am. Jour. Sci.*, vol. xlv., 1893, p. 325), which I have shown at work at a Royal Society soirée, but a description of which is here omitted to save space. Another plan, less successful so far, however, was based on the ideas of Mr. Tesla's most recent form of induction

coil—the one exhibited in this country by Professor S. P. Thompson. But of this also I omit the description at present.) Dr. Lodge.

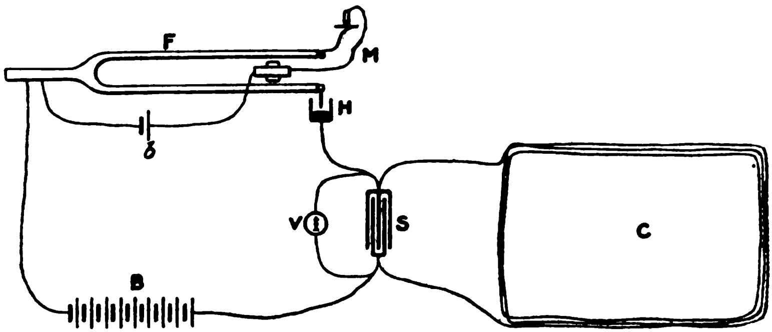

Fig. 2.

F is a Koenig electro-magnetic fork on its massive stand, vibrating 111 complete swings a second.

b is a single storage cell, and M is the magnet and vibrating solid contact to maintain the fork's motion.

H is the mercury cup contact for the main current.

B is a battery of 16 storage cells.

S is a condenser of variable capacity, and C the coil of 60 turns approximately 4 metres square.

V is a voltmeter to indicate the potential difference at the terminals of the condenser.

The self-induction of the coil being calculated roughly as 0·06 henry, the appropriate capacity for tuning, to the fundamental and the different harmonics, would be :—

For the note		111	per second,	S should	be	about	35	mfd.
,,	,,	222	,,	,,	,,	,,	8·6	,,
,,	,,	333	,,	,,	,,	,,		,,
,,	,,	444	,,	,,	,,	,,	2·2	,,

Practically it was found that 2·0 microfarads gave satisfactory tuning, sending up the voltmeter to 98 volts, though the applied E.M.F. was only 32 volts. And under these conditions the note of 440 could be distinctly heard in the small coil in the distant tower; and by introducing a key into the sending circuit, without

Dr Lodge. interfering with the continual vibration of the fork, messages could be sent and read with ease.

The receiving circuit was about 100 metres distant, high up in the College clock tower across the quadrangle, as in above experiment (3); but the small hoop coil was replaced by a square coil of 22 turns of No. 18 wire about 3·8 metres square, whose L was therefore 0·008 secohm. A capacity of 16 microfarads was needed to attune this circuit to the note listened for.

(5.) Instead of any maintained alternator or intermitter of any kind, the make and break, or the reversals, of a signalling key were sometimes used alone to do the whole thing; and when a battery such as 144 very small Planté cells was employed, the induction ticks heard in the secondary at each charge and discharge of the primary condenser sufficed to give clear signals at a moderate distance, on the "sounder" plan, the beginning and end of each long and short signal being marked.

(6.) Another method is to have a much better insulated wire for the primary coil, and to discharge round it a Leyden jar with a short spark gap excited by a Ruhmkorff coil; the spark being made whistling by a moderate glass-plate condenser (see "Modern Views of Electricity," Lecture III., page 435, second edition), the shrill momentary sounds can be heard in a suitable distant telephone and coil.

(7.) A pair of coils nearly 2 miles distant were now arranged, the transmitter being a rough square 100 yards across with 5 turns of four No. 16 wires in parallel, and the receiver being a long rectangle of the same periphery, about 30 yards wide, consisting of 19 turns of No. 18 wire. A dynamo with a multiple armature was employed, which was coupled up so as to give a nominal voltage of 175 volts (240 as measured). Nothing could be heard at the distant station without condensers, although the $M = 10$ C.G.S. This was expected, because of the high impedance and resistance. But when condensers were added at both ends and adjusted for resonance to the first harmonic—about 200 per second—the tone became faintly audible. The transmitting condenser was 10 microfarads, viz., four 5-microfarad condensers, two in parallel and two in series, which, by resonance, ran the

effective voltage up to 1,100 volts; and the receiving condenser was 2 microfarads, shunted by an ordinary 70-ohm Bell telephone. Dr. Lodge.

The self-induction of the sending cable was in this case 0·02 secohm, to which has to be added 0·04 secohm for the dynamo; while the resistances were 4 ohms for cable and 1 ohm for dynamo. So the reactance part of the impedance for the octave frequency was nearly 80 ohms, and the current therefore only about 3 amperes without sending condenser. A condenser magnified it four- or five-fold. The sending condenser can always be replaced by a dynamo of higher voltage; but, on the other hand, given any dynamo, a sending condenser will increase its power, except on circuits whose resistance, and therefore damping, is excessive.

The self-induction of the receiving cable, as connected, was $\frac{1}{3}$ secohm approximately, and its resistance was 133 ohms. Even here, therefore, the impedance was more due to self-induction than to resistance, the value of pL being 410 ohms; and so the condenser was a great advantage. In fact without it nothing could be heard, with the given amount of power expended.

It may be serviceable here to show how quickly to calculate the frequency for any given self-induction and capacity.

Let L be expressed in secohms or henries; let S be expressed in microfarads; then the resonance frequency of the circuit is

$$\frac{160}{\sqrt{(\mathrm{L\,S})}} \text{ complete alternations per second.}$$

So in the above case the syntonised sending frequency comes out,

$$\frac{160}{\sqrt{(0{\cdot}06 \times 10)}} = 208 \text{ per second};$$

the syntonised receiving frequency,

$$\frac{160}{\sqrt{(0{\cdot}32 \times 2)}} = 200;$$

and the fundamental note of the dynamo oscillated about 100.

The damping term of these particular cables, viz., $\frac{\mathrm{R}}{\mathrm{L}}$, is, however, certainly too high, and the wire used for such large circuits ought certainly to be thicker than No. 18 or 16. Coupling up the strands of wire in a telephone cable in different ways does not affect the damping constant, for the R and the L vary similarly; but the power of coupling them up differently is often convenient in enabling tuning to be got with a moderate condenser capacity, and, in general, in giving an adjustment for different notes.

Dr. Lodge. Otherwise, greater number of turns has just the same effect as thicker wire; it is simply *mass of copper* which is required to increase the power of a circuit to send or receive over big distances, and telegraphy over any distance by this method becomes, therefore, chiefly a matter of cost.

Elementary Theory of Attuned Circuits.

22. It may be well here to give the elementary theory of induction between a pair of circuits, each with condensers, especially for the case where the circuits are syntonised to the applied frequency.

Consider a circuit with self-induction and resistance and capacity, driven by an alternator of E.M.F. $= \mathrm{E} \cos p\,t$: the primary current in it is

$$\mathrm{C} = \frac{\mathrm{E} \cos (p\,t - a)}{\sqrt{\left\{ \mathrm{R}^2 + p^2 \left(\mathrm{L} - \frac{1}{p^2 \mathrm{S}}\right)^2 \right\}}},$$

where the lag a is given by

$$\mathrm{R} \tan a = p\left(\mathrm{L} - \frac{1}{p^2\,\mathrm{S}}\right).$$

If there is exact tuning ($p^2\,\mathrm{S}\,\mathrm{L} = 1$) the lag vanishes, and the current becomes

$$\mathrm{C} = \frac{\mathrm{E} \cos p\,t}{\mathrm{R}},$$

its maximum value.

On a distant circuit with mutual coefficient M the induced E.M.F. is $\mathrm{M}\,\dot{\mathrm{C}}$, or, in the above case,

$$\mathrm{M}\,p\,\frac{\mathrm{E}}{\mathrm{R}} \sin p\,t;$$

so the induced current there is

$$x = \frac{\mathrm{M}\,p\,\frac{\mathrm{E}}{\mathrm{R}} \sin (p\,t - \epsilon)}{\sqrt{\left\{ \mathrm{R}'^2 + p^2 \left(\mathrm{L}' - \frac{1}{p^2 \mathrm{S}'}\right)^2 \right\}}};$$

which, if again there be tuning ($p^2\,\mathrm{S}'\,\mathrm{L}' = 1$), becomes

$$x = \frac{\mathrm{M}\,p\,\mathrm{E} \sin p\,t}{\mathrm{R}\,\mathrm{R}'} \qquad \ldots \qquad \ldots \qquad (3)$$

23. Let a cable contain $N = n\,m$ turns of wire, of resistance ρ_1 per unit length, coupled n in series and m in parallel, and be laid round a circuit of radius a, with condensers properly attuned: then, in accordance with equation (3). Dr. Lodge.

$$x = \frac{n\,n'\,\pi^2 \dfrac{a^2\,b^2}{r^3}}{\dfrac{n\,n'}{m\,m'} 2\,\pi\,a\,2\,\pi\,b\,\rho_1\,\rho_1'}\,p\,\mathrm{E}\sin p\,t\,.$$

$$= \frac{m\,m'}{4} \cdot \frac{p}{\rho_1'} \cdot \frac{a\,b}{r^2} \cdot \frac{\mathrm{E}}{\rho_1\,r} \sin p\,t \quad .$$

$$= \frac{a\,b}{4\,r^2} \cdot \frac{p}{\mathrm{R}_1'} \cdot \frac{\mathrm{E}\sin p\,t}{r\,\mathrm{R}_1} \quad \ldots \quad \ldots \quad \ldots \qquad (4)$$

where R_1 is the resistance of unit length of the cable itself as connected.

Here the last factor in (4) is the maximum strength of current which the dynamo could send direct to the distant station if its cable were long enough to reach all the way, and if it were opened out as connected and laid straight, all the parallel turns parallel and all the series turns end to end; and with all the remaining self-induction impedance abolished by a suitable very large condenser.

The actual current received inductively is therefore a definite fraction of this direct line current: the fraction depending on the linear dimensions of each circuit compared with the distance, and on the frequency, and on the effective thickness of the receiving cable.

24. Suppose we tried to signal a distance of 12,000 kilometres (across the earth) with a pair of No. 0 equal cables, each once round an area comparable to England, say a circle 200 kilometres in diameter, using a frequency of 1,000 and an applied voltage of 200; ignoring all hysteresis and other losses (which in condensers are often important), and ignoring also the conductivity of the earth, which may be very troublesome:

$$\frac{\mathrm{E}}{\mathrm{R}_1\,r} = \frac{200}{0{\cdot}322 \times 12{,}000} = \frac{1}{20} \text{ ampere};$$

$$\frac{p}{\mathrm{R}_1'} = \frac{1{,}000 \times 2\,\pi}{3{,}220} = \frac{2\,\pi}{3};$$

$$\frac{a\,b}{4\,r^2} = \frac{100 \times 100}{4 \times (12{,}000)^2}.$$

Dr. Lodge. These are the three factors of equation (4); so the induced telephone current is about 2 microamperes, which is within the limit of audition.

The self-induction of such a cable would be between 2 and 3 henries, its resistance about 200 ohms, and the capacity needed only about $\frac{1}{100}$ microfarad.

This arithmetic is a mere illustration of what would happen if everything were favourable.

Conditions for Distant Signalling and Precise Tuning.

25. Consider further the arrangement of coils adapted to really distant syntonic magnetic space telegraphy with very precise tuning. The mutual induction coefficient depends upon $n\,n'\,a^2\,b^2/r^3$; or for similar equal coils, each of periphery l and n turns,

$$\mathrm{M}\,r^3 = \text{the effect of the coil at a given distance} = n^2\,l^4 \qquad (5)$$

The amplitude of disturbance maintained by an alternator of frequency p and voltage E_0, in a circuit whose natural frequency $p_0 = \frac{1}{\sqrt{(\mathrm{S\ L})}}$, may be written,

$$\frac{\mathrm{E}_0/\mathrm{R}}{\sqrt{\left\{1 + \frac{\mathrm{L}}{\mathrm{S R}^2}\left(\frac{p}{p_0} - \frac{p_0}{p}\right)^2\right\}}},$$

which in case of exact resonance becomes E_0/R. Hence, to make resonance *sharp*, the coefficient $\frac{\mathrm{L}}{\mathrm{S R}^2}$ must be great. If this condition is not satisfied there will be a considerable range of resonance within which some exaltation of voltage is felt, but no decided peak; whereas, if $\mathrm{L/SR}^2$ is great—*i.e.*, if the circuit-time-constant, L/R, is much larger than the condenser-time-constant, S R—then except at a particular frequency the circuit is nearly inactive, but as the correct frequency is approached the activity of the circuit suddenly runs up to a high steep maximum, and condensers are liable to be burst by the immense and rapid magnification of voltage.

Take the case of approximate resonance, so that $p^2\,\mathrm{S\,L} = 1$ nearly, and introduce this value of S: then the coefficient which

has to be big is $\frac{p^2 L^2}{R^2}$; *i.e.*, for a given frequency simply the damping constant or logarithmic decrement, R/L, must be small, or the current-decay time constant, L/R, big, compared with the frequency time constant. Now Dr. Lodge.

$$\frac{L}{R} = \frac{n^2 l \left(\log \frac{3 l}{c} - 2\right)}{\frac{n l}{c^2}} = n c^2 \text{ approximately} \qquad (6)$$

where c is the thickness of the wire, which does not enter much into the numerator.

So, looking at (5) and (6), we see that the two things which we want big are $n^2 l^4$ and $n c^2$; wherefore, in case of magnification of circuit for bigger distances, the thickness of the wire should be magnified as well as its length. Of course for really high frequencies it is no use having a thick wire *solid;* see section 32. It must either be ribbon-shaped, or be stranded, with the strands roughly insulated even if they are not going to be used otherwise than in parallel.

Magnification Ratio.

26. The amount of magnification of voltage caused by resonance is to be calculated thus:—

The impressed E.M.F., or the E.M.F. of the sending dynamo, being E_0, the effective potential difference, V, between the condenser terminals is such that, when there is syntony and when the damping is small,

$$\frac{V}{p L} = \frac{E_0}{R};$$

in other words, the magnification ratio is

$$\frac{V}{E_0} = \frac{p L}{R} = \tan a,$$

and approaches infinity as the lag of current behind E.M.F. approaches $\frac{1}{2} \pi$.

The primary current-amplitude without a condenser would be $\frac{E_0}{\sqrt{(R^2 + p^2 L^2)}}$, but with the syntonising condenser it is $\frac{E_0}{R}$.

Dr. Lodge. 27. But it is to be remembered that all hysteresis losses—whether they be due to an iron core in the coil, however subdivided, or whether they occur by reason of a stratified or other imperfect dielectric in the condenser*—and also all eddy-current losses by reason of induction in neighbouring conductors, whether insufficiently subdivided iron or earth or sea,—all go to increase resistance—*i.e.*, to increase damping—and thereby to diminish the magnification ratio. If the R be measured in the usual way, by steady currents, it is liable to be supposed much smaller than it really is—*i.e.*, smaller than the really effective R for alternating currents—especially for high frequency. Part of the art of magnetic space telegraphy will consist in avoiding or minimising these various sources of loss, all of which show themselves as an increased resistance.

28. Perhaps the neatest way of proving that the magnification ratio equals $p\,L/R$ is as follows:—

Since in free oscillations $\frac{1}{2}\,S\,V^2 = \frac{1}{2}\,L\,C^2$, where V and C are the potential- and current-amplitudes respectively, and where $p^2\,L\,S = 1$, we get

$$C = V\sqrt{\frac{S}{L}} = \frac{V}{p\,L} = \frac{E_0}{R};$$

so the magnified or syntonic voltage, when the damping is small, is

$$V = \frac{p\,L}{R}\,E_0.$$

To maintain these oscillations is the function of the dynamo, and for this purpose it has only to supply the waste power, viz.,

$$\tfrac{1}{2}\,R\,C^2.$$

Hence, in cases where R is large from any cause, considerable power is necessary; but where the R is small, including all the spurious losses referred to in section 27 which go to make up the

* We have had occasion to measure the hysteresis loss in several condensers, using for this purpose a wattmeter method; but the total loss of this kind due to all causes combined is indicated by the actual resonance experiment itself—that is to say, by observing the magnification ratio and comparing it with the theoretical value pL/R or $1/pSR$, and so measuring the effective R. We have thus found some excellent condensers made by the Stanley Manufacturing Company, Pittsburg, U.S.A., which have the smallest hysteresis loss of any so far tested.

effective R or dissipation constant, then violent oscillations can be Dr. Lodge. maintained in a circuit with very little outside power (compare footnote 5, section 12).

The whole matter is analogous to the swinging of a heavy pendulum or bell, whose motion is excited and maintained with little energy if the impulses are properly timed, but which requires more energy when swung in a viscous fluid. The timing of the applied impulses should be the same as if it were free, although the actual time of free swing is a little lengthened by the friction.

For circuits oscillating with extreme rapidity one of the principal sources of spurious resistance, and therefore of damping, is radiation of waves, but with these leisurely circuits there is no radiation that need be taken into account in estimating the waste of power. A church bell stiffly hung would become but little easier to swing if its clapper were removed and its wave-emitting property destroyed.

Best Size and Shape of Coil for Violent Resonance.

29. It is of interest to find the shape of coil adapted to reduce damping due to ordinary resistance to a minimum. Let the coil be of mean radius a, of sectional radius c, and consist of n turns: then the volume of actual copper in it, representing the weight and cost of the coil, ignoring insulation, is

$$W = \frac{\pi^2 c^2}{4n} \times 2\pi n a = \tfrac{1}{2}\pi^3 a c^2;$$

$$L = 4\pi n^2 a\left(\log\frac{8a}{c} - 1\cdot75\right);$$

$$R = \frac{2\pi n a}{\dfrac{\pi^2 c^2}{4n}\kappa} = \frac{8n^2 a}{\pi c^2 \kappa} = \frac{(2\pi n a)^2}{\kappa W}.$$

The damping time-constant, or reciprocal of the logarithmic decrement, is

$$T = \frac{L}{R} = \tfrac{1}{2}\pi^2\kappa c^2\left(\log\frac{16W}{\pi^3 c^3} - 1\cdot75\right) = \frac{\kappa W}{\pi a}\left(\log\frac{8a}{c} - 1\cdot75\right).$$

So T will be a maximum, and the damping a minimum, for

$$\frac{dT}{dc} = 2c\left(\log\frac{16W}{\pi^3 c^3} - 1\cdot75\right) - 3c = 0;$$

Dr. Lodge. or

$$\log \frac{8\,a}{c} = 1{\cdot}75 + 1{\cdot}5 = 3{\cdot}25\,;$$

or

$$\frac{a}{c} = 3{\cdot}22\,;$$

which is, therefore, the condition for maximum T with a given W—*i.e.*, the condition for least damping, with a given bulk of copper.

This maximum $T = T_m = \frac{3}{4}\,\pi^2\,\kappa\,c^2 = \dfrac{3\,\kappa\,W}{2\,\pi\,a} = \dfrac{3\,\kappa}{5{\cdot}5}\,W^{\frac{2}{3}}$

(= 3·4 seconds for a ton of copper).

The E.M.F. which the condenser will have to stand is $p\,T\,E_0$.

The L appropriate to T_m is

$$L_m = 6\,\pi\,n^2\,a = 16{\cdot}5\,n^2\,W^{\frac{1}{3}}$$

(= 1,650 n^2 for a ton of copper).

The mean periphery of the coil is $5{\cdot}5\,W^{\frac{1}{3}}$

(or $5\frac{1}{2}$ metres for a ton of copper).

The thickness of the coil, $2\,c = \dfrac{4\,W^{\frac{1}{3}}}{2{\cdot}345\,\pi}$

(= 53 centimetres for a ton).

Such a thickness of coil may seem preposterous, but in certain confined spaces (*e.g.*, a lighthouse) it is advantageous so to be able to dispose the circuit, and the coil may consist of any amount of turns without detriment. What has to be scrupulously avoided in the neighbourhood of any coil is conducting material; but the reaction effect of such material falls off rapidly with distance—as the inverse 6th power (see Lodge, *Phil. Mag.*, Feb., 1880, p. 142)—and accordingly it is to be hoped that the general conductivity of the earth will not exert a very deleterious effect. Its effect will certainly be in the direction of increasing the resistance, and therefore the damping, of either coil; but, beside this reaction effect, a true partial screening of one coil from the other is to be feared.

The thickness of wire is

$$c\sqrt{\frac{\pi}{n}} = \frac{W^{\frac{1}{3}}}{2{\cdot}07\,\sqrt{n}} \quad \left(= \frac{50}{\sqrt{n}} \text{ for a ton of copper}\right).$$

Thus, to take a numerical example, a coil having the proportions and magnitude now reckoned might consist of 10,000 turns of wire half a centimetre thick (No. $5\frac{1}{2}$); the outside diameter of the

coil would be $2\frac{1}{2}$ metres, and the inside diameter $1\frac{1}{4}$ metres. Such a coil would weigh a ton, and its resistance would be Dr. Lodge.

$$R = \frac{48{,}000\ n^2}{W^{\frac{1}{2}}} = 480\ n^2 \text{ C.G.S.} = 48 \text{ ohms.}$$

Its self-induction would be 165 henries; its time of falling to $\frac{1}{e}$ the current, 3·4 seconds; and its magnification ratio, with a note of 400 per second, would be 8,500.

The condenser to be used with it for this note would be about $\frac{1}{1000}$ microfarad, or 9 metres electrostatic capacity. It might have an air or fluid dielectric to lessen hysteresis, and must be able to stand a very high potential—for instance, nearly a million volts if the applied dynamo E.M.F. is 100 volts. But the coil itself would have notable electrostatic capacity, and a further investigation would be necessary to take it into account.

The current in the wire would be only 2 amperes, and the energy expended therefore insignificant, viz., about 200 watts. In such a case the signalling would be done by the agency of the mass of copper rather than by mechanical power, and the tuning would be excessively sharp. There may be a few places where it is desirable to economise power to this extent, as, for instance, when the dynamo has to be driven by hand, or where space is very cramped, as in a lighthouse; but in most cases it would be far better to open out the coil into a circuit enclosing a large area. This could nearly always be done at *one* of the stations.

A compact coil like the above is not a powerful sender or receiver, in spite of its high magnification ratio. For instance, using a pair of such coils, one as sender and one as receiver,

$$M\ r^3 = (\pi\ n\ a^2)^2 = \tfrac{1}{4}\,\kappa\,R\,W\,a^2 = \frac{10^{18}}{18} \text{ C.G.S.}$$

So the induced current, when properly attuned, is

$$\frac{p\,E\,M}{R^2} = 2{,}500 \cdot \frac{100}{48} \cdot \frac{10^{18}}{18\,r^3 \times 48 \times 10^9} = \frac{6\ 4 \times 10^9}{r^3} \text{ C.G.S.}$$

And the distance at which this current would be a microampere (10^{-7} C.G.S.), or something easily audible, would be

$$r = \sqrt[3]{64 \times 10^{15}} = 4 \text{ kilometres only.}$$

Dr. Lodge.

Simple Condition for Distant Signalling.

30. Having realised that any distance can be reached if enough copper and enough power are employed, it remains to reckon what is the best way of arranging the copper, and how little will serve to enable a given power to signal a given distance.

When nothing else has to be considered, the condition to be satisfied for signalling to a great distance is to make the magnetic moment of either coil as great as possible: its value is

$$n \pi a^2 \mu C = \frac{\mu \kappa W E}{4 \pi n} \quad \text{when syntonised;}$$

so all the copper available, W, of specific conductivity κ, should be disposed in one large single circuit at each station.

Possibility of Using Iron.

31. If iron is used as the core of a sending coil, it should be minutely subdivided, and should, if possible, be hot—as near its critical point as practicable—because then both hysteresis and eddy losses are at their minimum: see a most interesting communication on the behaviour of iron at different temperatures by Mr D. K. Morris in *Proc. Phys. Soc.* for 1896-97, or *Phil. Mag.*, vol. xliv., p. 213.

A transmitter shaped like a chimney or a lighthouse, with a long vertical thin iron strip core surrounded by many turns of wire, would be most efficient if only the damping losses could be got rid of. If iron is used, the sending column could be laid on its side and employed in the end-on position. For the function of the sender is to produce the strongest possible alternating magnetic field at a distance, and this depends simply on magnetic moment, the field being $\frac{m}{r^3}$ at any distance r in an equatorial plane; and this field, though excessively and imperceptibly weak at any point, is then to be detected by the integrating power of the receiving coil over a large area.

So, in general, if a magnet of moment m oscillates $p/2\pi$ times a second, and the receiving circuit is of area A, the number of lines passing through that circuit is A m/r^3, and p times this is

the maximum induced E.M.F. which a telephone or other instrument in the receiving circuit has to detect if it can. Dr. Lodge.

THROTTLING.

32. The thickness of wire permissible without insulated subdivision into strands is regulated by the frequency. Taking Lord Rayleigh's value of the initial throttling, $p^2 l^2 \mu^2/12 R^2$, it comes out $1{\cdot}2\, n^3 a^4 \times 10^{-5}$ C.G.S. for copper.

So, if the throttling is not to result in a greater increase of resistance than 1 per cent., a thickness of wire 1·08 cm., or No. 5/0, may be used for a frequency of 100 per second.

For 500 per second nothing thicker than 0·48 cm., or No. 6, should be used without subdivision.

For 1,000 per second No. 10, or 0·34 cm. thickness, is permissible.

For 1,600, No. 12; and for 2,500 per second the appropriate thickness is 0·216 cm., or No. 14 S.W.G.

If No. 2 wire, or 19/16, is used for 500 per second frequency, the loss due to throttling is 5 per cent.

If No. 0, or 19/12, is used, the loss is 9 per cent.

But if the strands are each cotton-covered before stranding, then there is practically no loss.

32*a*. There remains to consider the improvement and sensitisation of the telegraphic or telephonic appliances which may be used in induction telegraphy at the receiving end; and to these I now proceed.

[*End of Part II.*]

Dr. Lodge.

Part III. (Receiving Appliances).

Detectors.

33. A large number of devices for improving the detector of the fluctuating current at the receiving station were made and tried, and a few of them may be here described.

Tone-Telephone.

Not only was it considered desirable that the two circuits should be attuned to each other and to the sending dynamo or intermitter, but also it was thought well to make the receiving telephone so that it would only or chiefly respond to one particular note.

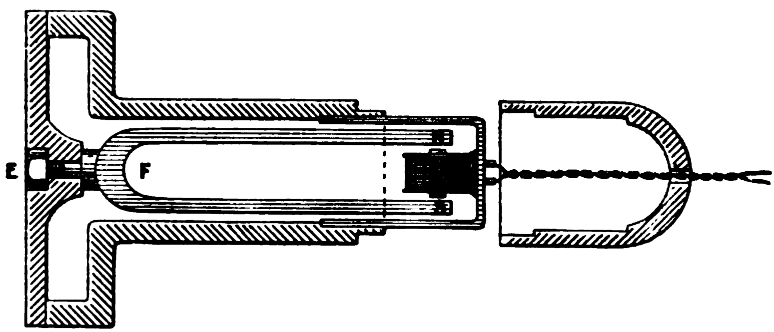

Fig. 3.—A form of Receiving Tone-Telephone, with the receiving coil supported on a brass arch and covered by an ebonite screw cap.

I had found that the diaphragm of a Bell telephone need not be free everywhere except at the edges, but might nearly as well be fixed all over to a thin deal sound-board upon which the ear was directly pressed. Consequently I proposed to use this sound-board idea for a tone-telephone.

One good form is represented in Fig. 3, where F is the tuning-fork, screwed into a sound-board, with its prongs highly magnetised, and with a short iron electro-magnet between its prongs, round which the received alternating currents were to circulate. The whole was mounted in a compact case, in general appearance like a Bell telephone, and the result was very sensitive to the proper note—distinctly more so than an ordinary

telephone when both were in circuit. The ear was laid right upon the wooden end, E, which holds the stem of the fork; and though of course the sound did not rise and decay quite sharply, yet signals could be clearly read, and the damping of the fork might be at any time increased, at the expense of a little sensitiveness, whenever sharper—*i.e.*, quicker—signals were wanted. Such a tone-telephone arranged on a large sound-board, or with a resonant jar, may serve as syntonic "call," accurately discriminating one station from another. Fig. 4 shows an alternative form of it. Dr. Lodge.

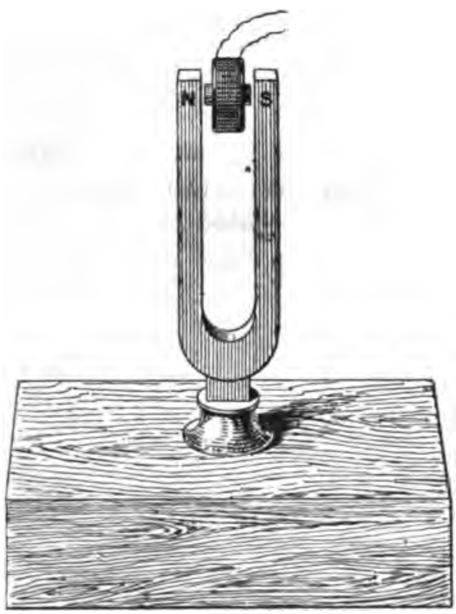

FIG 4.—Another form of Receiving or Transmitting Tone-Telephone, mounted on hollow resonant box, but with support of coil not shown.

Another use for which a magnetised tuning-fork telephone is convenient is to serve as a weak local sender for the purpose of testing whether the receiving circuit is properly adjusted or not. Thus, for instance, suppose signals are expected from a distant station, and are not being received, it may be because the receiving circuit or its apparatus has gone out of tune a little; so its tone can be tested by twitching with the fingers the tuning-

Dr. Lodge. fork of a tone telephone whose local circuit is properly arranged in inductive connection with the main receiving circuit. It gives at each twitch a feeble alternating current which should make the receiving instruments decidedly respond. If they do not, they must be adjusted till they do. Similarly, a local receiving tuning-fork telephone may be used to test the adjustment of the sending circuit. These forks will serve as standards at the different stations, enabling them to keep in unison, and being not affected by anything except *differential* changes in temperature.

Coherer.

Another totally different form of detector was suggested by my old experiment of the syntonic jars (Fig. 1, Part I.), where the air gap was filled by a pair of knobs in near contact, and with a local battery and bell—in other words, with what is now called a coherer circuit. So now the receiving condenser can be shunted by a coherer, and a relay worked by the signals so obtained. But to the sinuous disturbance of an alternator a coherer turns out practically insensitive; it is to electric jerks that it responds. Accordingly, when the sender is of the make-and-break kind a coherer answers; but when it is of the slowly alternating kind it fails.

Coherer Call.

By inserting an interruptor in the receiving current so as to jerk any induced sinuous current that it may find there, the

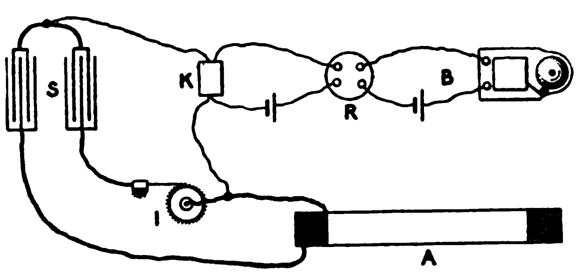

FIG. 5.—Arrangements for Coherer Call.—I is the interruptor or jerker, operating on any sinuous current induced in the receiving circuit, A S; K is the coherer, R the relay, and B the local battery and bell circuit

coherer can be made to work, and one plan of its connections is shown in Fig. 5.

It may not at first be apparent why there should be two condensers in series in this figure, but it must be remembered that the interruptor must not be allowed to affect any portion of the local battery current; else, of course, the coherer will respond when no signals are being sent. Dr. Lodge.

Bolometer.

A bolometric call was also tried and was found to work, but, as it was not specially successful, the details of it will not be given.

Vibrating Coil Telephone.

For the purpose of disturbing a sound-board, and in general of producing a mechanical effect in solids or liquids, as opposed to gases, it was thought probable that an iron diaphragm was not

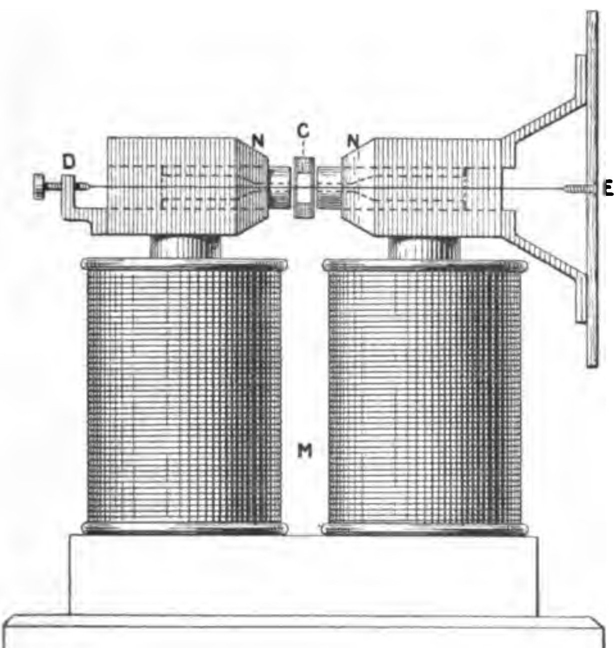

FIG. 6.—Early form of the Vibrating Coil Telephone and Sound-Board.

the best plan, but that the whole receiving coil, free from iron, might be mounted so as to be itself capable of vibration in a strong magnetic field.

Dr. Lodge An early form of the vibrating coil telephone and sound-board is shown in Fig. 6, where M is a large horse-shoe magnet with perforated pole-pieces, one of which carries a sound-board, E, with a stretched wire from its middle passing right through the perforations to a screw tightener, C, and carrying, rigidly attached to its middle, a light fine-wire coil, D, matted together with shellac, and placed between the pole-pieces.

The magnet was magnetised, not in the customary way, but with its adjacent poles of the same sign, so that the lines of force spread out through the coil, and give it the chance of cutting them rapidly if it moves at all axially.

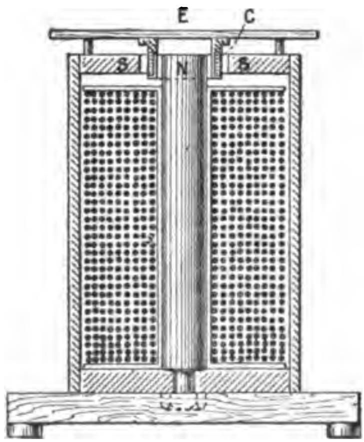

FIG. 7.—Another form of Sound-Board Telephone, with the receiving coil surrounding one pole of the magnet, and surrounded by the other pole. The coil attached direct to sound-board, E.

On passing feeble alternating currents through the coil the sound-board was strongly affected by its taut wire. A subdivided iron core was subsequently placed in the coil, but, whether by reason of the increased mass, or for some other reason, it did not now work so well.

Another form was then made with a magnet specially designed as shown in Fig. 7.

All the iron was well annealed, plenty of field winding put on, and the air gap was an annulus $\frac{3}{8}$ inch square in section and $1\frac{1}{2}$ inch in diameter; no attempt was made to subdivide the

iron, because in this arrangement it is plain that eddy-currents are all to the good. They do not occur in any moving part, so their chief effect is to diminish the impedance of the receiving coil. Dr. Lodge.

The coil is powerfully damped by the strong field, and a certain strength of field is therefore best. A permanent steel magnet may therefore do as well as well as an electro-magnet.

The coil was cemented direct to a wooden disc, and the thicker this disc the higher the note to which the arrangement most powerfully responded. By placing the ear on the wood, the first-made instrument on this plan was quite as sensitive as the best of the usual patterns of telephone—for instance, the Collier or the Ader pattern.

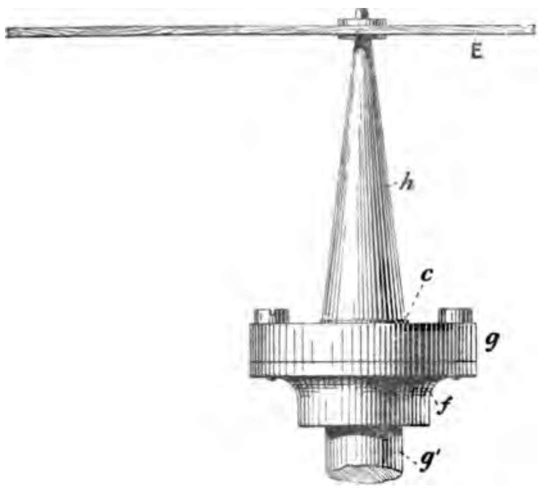

FIG. 8.—Sound-Board Telephone for Acoustic purposes, with vibrating coil, C, attached to large wooden sound-board, E, by light cone, *h*, and immersed in an annular magnetic field, of which the details are shown in several other figures, especially Fig. 15; *g* and *g'* being iron, and *f* brass.

Tambourine membranes and many other plans were tried for holding the coil, but the simple wooden sound-board answered best.

The wooden disc need not be clamped at its edge. It has a circular nodal line, and in three points on this circle it was usually clamped so that it could vibrate freely like a circular sort of harmonicon reed. Thus mounted it was very sonorous, at least

Dr. Lodge. before the coil was put on. The load of the coil had to be taken into account in finding the nodes, and they were accordingly found experimentally. When the plate was unloaded its nodal line was at 0·68 of its radius, which agrees with theory. The loading, however, spoiled all this and greatly increased the damping, so that considered as a syntonic receiver it was not successful. The coils at this time were of copper, and heavy. Now they are of aluminium, and much lighter.

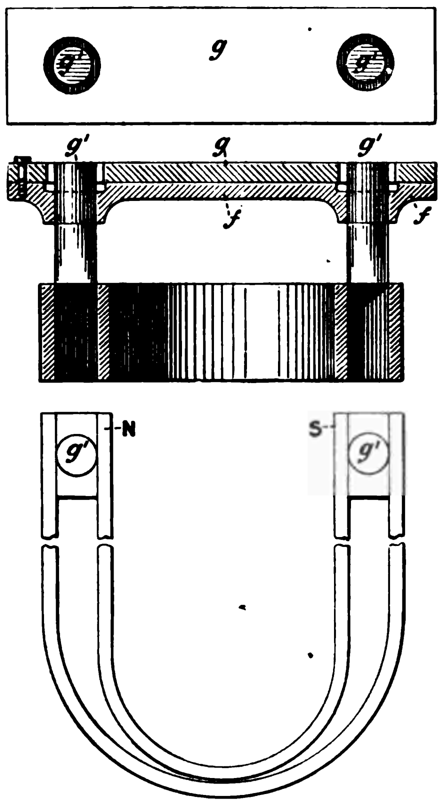

FIG. 9.—A form of Permanent Steel Horse-Shoe Magnet, arranged to provide two annular magnetic spaces; g and g' being soft iron, and f brass.

The present plan of a large sound-board, and a light coil rigidly attached to a point at its middle by either a light wooden tripod or a light stiff cone, was then designed, and has not been improved upon. See Fig. 8. The magnet here intended is a

double-pole horse-shoe magnet, with the keeper, *g*, clamped on either pole by a brass piece, *f*, and reaching from one pole to the other, leaving a couple of annular spaces for vibrating coils to work in. A form of such magnet is shown in Fig. 9. Dr. Lodge.

Magnifying Telephone.

Instead of using the vibrating coil to affect a sound-board direct, it was obvious that it might be used first to affect a microphonic contact in a local battery circuit, and thus cause

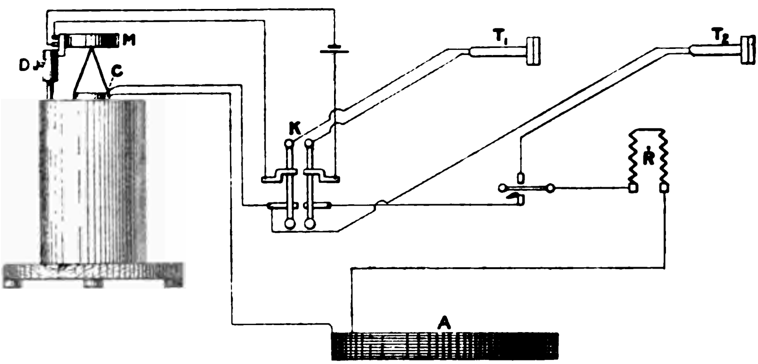

FIG. 10.—Arrangement for experiments in magnification.—A is the experimental receiving circuit, as before, connected to the coil C through a resistance box, R, whereby the disturbance might be readily weakened to any extent. The microphonic transmitter is supported on a stiff pillar and adjustable clamp, D, and communicates with the telephone T_1 when the keys, K, are both up. But when they are both down, the telephone T_1 is affected by the directly received current only, without magnification. The disturbance caused by the insertion of T_1 into this circuit is compensated by the simultaneous switching of the equivalent telephone, T_2, out of it. Or for rough experiments T_2 may be used to indicate the magnitude of the directly received induced currents.

fluctuations in a stronger current than that induced from a distance, and so send on a more vigorous disturbance to another telephone, and thus cause a louder noise. For this purpose, after several microphonic transmitters had been tried, a Berliner was ultimately chosen, chiefly because it could work horizontally, and arranged as in Fig. 10.

Here the ordinary telephones are arranged so that the disturbance can be heard either direct without magnification or after magnification, and thus the received disturbance can be compared

Dr. Lodge. with the magnified disturbance. The telephone T_1 serves for both determinations, according to whether the keys are up or down.

The telephone T_2 was a duplicate of the same make, and was switched into either circuit when T_1 was out of it, so as to keep everything similar. It was found during the experiments that T_2 was unimportant: the resistance and inductance of the rest of the circuit were great in comparison.

In these early trials the tripod merely pressed against the mica disc of the transmitter; but the mica was now removed and the tripod attached to the coil pressed against the carbon plate instead; but this was no improvement. Hitherto the coil was *pressed* up, resting on elastic pellets below, and was under considerable constraint; now, finally, the tripod was attached rigidly to the centre of the carbon plate of the Berliner transmitter, and the coil was allowed to hang freely in the magnetic air gap. This was much the best plan; but a spiral spring pressing upwards is still generally used, to support the weight of the coil, and also to regulate the dominant pitch appropriate to the disc and coil as a vibrating system.

The method of connecting the magnifier to the telephone listened to was also modified, and was frequently as follows:—

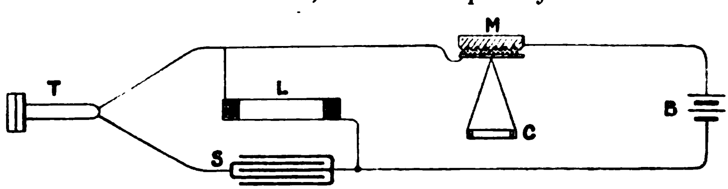

FIG. 11.—Improved method of connecting a microphone electrically with its telephone.

T is any telephone, diagrammatically indicated.

M is the Berliner microphonic transmitter, with receiving coil, C, hanging on a tripod from its carbon plate.

B is a battery of three storage cells; and L is a self-induction coil, without iron, which carries most of the steady current which is necessary to the good action of a microphone, but diverts the jerks and fluctuations mostly through the telephone; even though this be of high resistance.

Sometimes a condenser, S, was inserted in its circuit too, with the object of assisting through the telephone fluctuations of a definite pitch, and of other pitches not too far removed from that. Dr Lodge

A transformer could be used instead of the self-induction shunt, but the advantage of a transformer is more felt on long lines; for a compact apparatus it serves too, but it wastes more power than the L shunt.

The following arrangement was tried as receiver at my house of the signals sent from College, two miles away :—

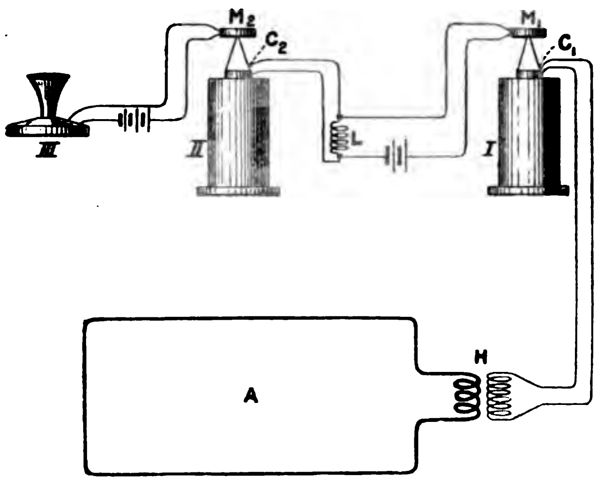

FIG. 12.—Arrangement for two-mile signalling without tuning.

A is the cable once round the garden, connected to the thick winding of a hedgehog transformer, H. Its thin winding is connected to the fine-wire suspended coil of the first magnifying telephone, I.

The currents from its transmitter, M_1, are taken to the fine-wire coil of the next magnifier, II., shunted by the low-resistance self-induction coil, L.

III. is the final telephone of the series, viz., an ordinary Western Electric loud speaker (2 ohms), in series with the transmitter M_2. Under these circumstances the signals could be heard with the ear 2 inches from the funnel of III.

But the hedgehog wasted a lot of power; its volume of iron

Dr. Lodge. being considerable, and the hysteresis and eddy losses at high frequency being very marked.

An improved arrangement consisted in interposing a 20-microfarad condenser (the largest then available, kindly lent by Dr. Alex. Muirhead). This was not enough to tune up the circuit, except when the thin-wire circuit of the hedgehog was unclosed. So the following plan was adopted, and was a decided improvement:—

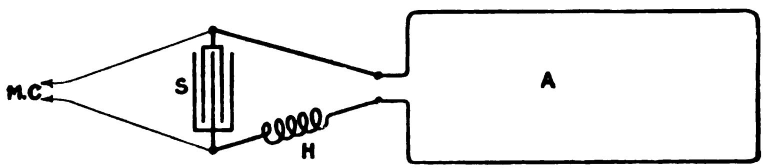

FIG. 13.—Arrangement for two-mile signalling with tuning.

M C is the magnifying arrangement, as before.

H is the thick wire of the hedgehog, inserted merely as extra self-induction.

S is a condenser having a capacity of 20 or 21, or sometimes as much as 24, microfarads (a small plug condenser being added in parallel to vary and improve the tuning).

Thus the note was enhanced by resonance, and the signals became almost loud. (The tuning-reinforcement at the College end happened, however, on this particular occasion to be rather poor.)

This must serve as a sample of a great number of experiments, made with all manner of condensers and different notes; an especial amount of variation being practised at the sending end.

It is to be remarked that fine-wire coils to the telephones are by no means the best for this particular purpose, and afterwards ribbon coils of thin aluminium strip were used instead. Transformers and L shunts could then be dispensed with; but small telephonic transformers are still usually employed, to prevent the constraint exerted on the hanging coil by a steady current passing through it. Small transformers don't waste much, and a

little waste of power matters nothing where there is a local battery. Dr. Lodge.

The Berliner or such-like granular transmitters, though serving well to magnify an already audible current, do not always pick up and magnify an inaudible one, and, moreover, they do not discriminate between intended and accidental vibrations. For this purpose a better and more precise apparatus has to be constructed, viz., the reed or the tuning-fork magnifying telephone with only a single pair of carbon contacts. This is shown in Fig. 14, and its essence is that the coil is hung to a reed or fork or vibrator of definite pitch, and carries also a pellet of carbon which presses lightly upward on another fixed pellet, thereby constituting a single-contact microphone.

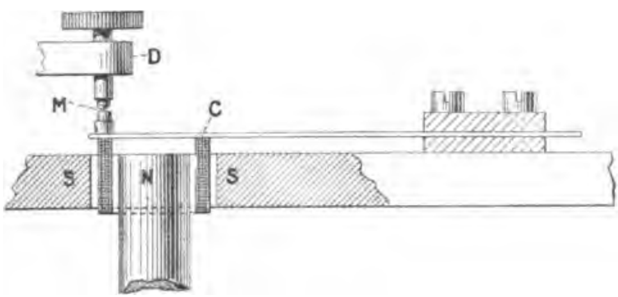

FIG. 14.—Reed Magnifying Telephone, where the vibrating coil, C, is mounted on a tuned spring, and carries half of the microphonic contact, M.

Such an arrangement, as is well known, is excessively sensitive to utterly imperceptible mechanical disturbances, and the minutest fluctuations of current now passing through the hanging coil give ultra-microscopic tremors sufficient to vary the resistance of the light carbon contact perceptibly; the energy of a single-cell local battery being thus brought into action, enough energy is imparted to disturb (through another hanging coil) one of the ordinary granular transmitters, which then passes on a stronger current to the loud speaker, or the "call." Or it may be that two reed magnifiers in series are desirable, when the received current is exceedingly faint.

A still more precisely responding instrument is the tuning-fork magnifying telephone, which responds only to its own note.

Dr. Lodge. One form of it is shown in Fig. 15, where the lettering is the same as before; g, g' being iron pole-pieces, f a brass support, C the coil in the magnetic field attached to a prong of the tuning-fork, F, by a light tripod or cone, h, and carrying half of the microphonic contact, M, which is adjusted by a lock nut thumb-screw working in the rigid independent support, D.

The single-point magnifiers do not carry big currents, and they require delicate, though easy, adjustment; but when properly made and adjusted they work extremely well with pure tones. The multiple-contact microphones work better for articulate

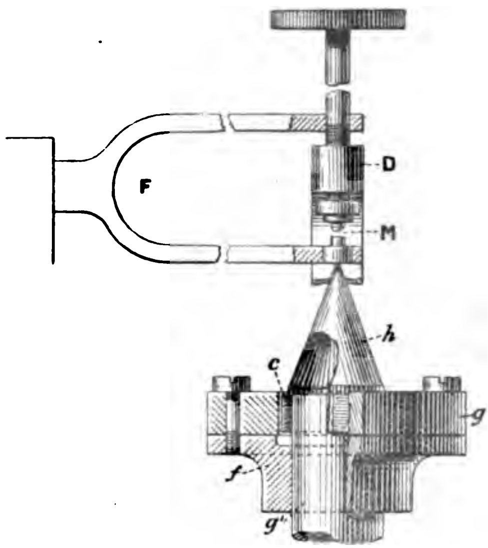

Fig. 15.—Tuning-Fork Magnifying Telephone.

speech, as is well known. I find graphite carbon the best for these single contacts, and I am indebted to Mr. Swan, to Le Carbone, and to Mr. Acheson for several specimens of soft carbon which answer much better than the harder arc lamp variety.

The last telephone of the series has been so far represented as one of ordinary pattern, but it is obvious that the vibrating coil attached to a wooden sound-board may be employed; and, further, that a combination of such sound-board telephones may have an important application to the human voice and the acoustics of buildings—a different subject, into which I will not now further go.

Dr. Lodge.

It is also manifest that when the vibrations are magnified they may be used to work a relay and ring a bell, or actuate a Morse instrument, or anything else; just as in the phonoporic receiver, for instance. I am indebted to Mr. Langdon Davies for the loan of one of his phonoporic receivers.

The bell-ringing arrangement that we at present prefer is a coil telephone of the tuning-fork pattern, so that (if protected from mechanical shaking) it shall only respond to a definite note, and not to casual currents in the line. The light coil hangs to the limb of a large tuning-fork, and on this or on the other limb is a light metallic contact which dances when the limb vibrates, thereby practically breaking a local relay circuit and letting its tongue fly over, so as to close a bell circuit.

A diagram is hardly necessary, but here it is:—

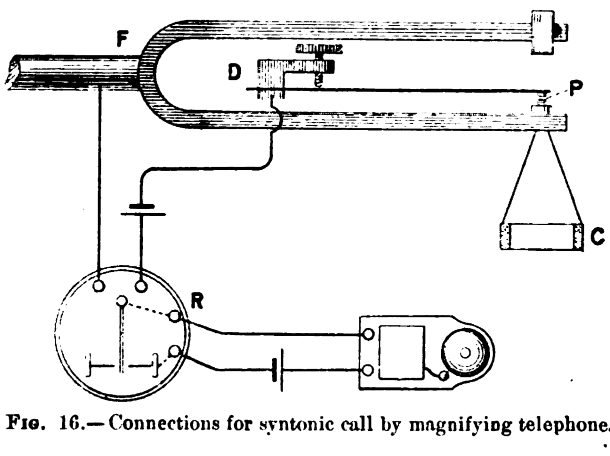

FIG. 16.—Connections for syntonic call by magnifying telephone.

C is the last of the series of the suspended magnifying telephone coils.

F is a tuning-fork on firm stand.

D is also rigidly supported, and carries a thumb-screw for adjustment of spring.

P is a platinum stud contact carried by a very light spring, either straight as shown, or spiral from the other prong; and this forms part of the local polarised relay circuit,

R, which rings a bell in the very ordinary way in any office where calling up is desired.

Dr. Lodge. It may be thought that a light coil is hardly competent to shake appreciably the massive steel limbs of a tuning-fork; but when it is isochronously attuned to the pulsations of the received disturbance, a tuning-fork is wonderfully sensitive (even to aerial vibrations, as is well known in an ordinary resonance experiment); and the fact that it is not sensitive to disturbances of accidental frequency is wholly advantageous.

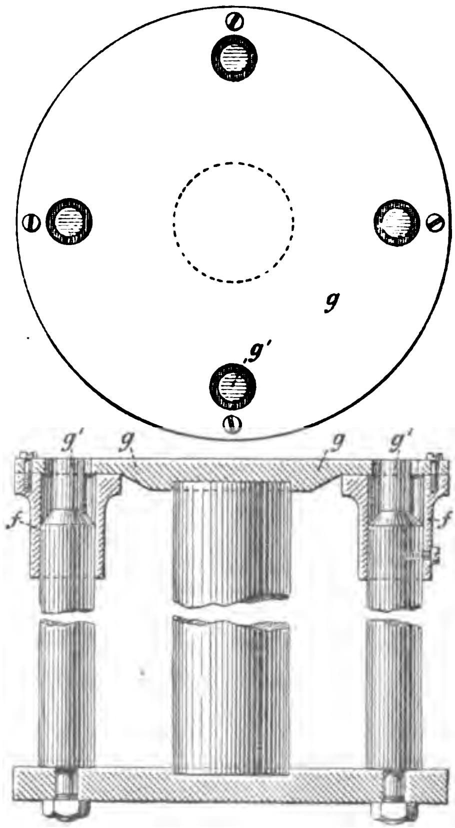

FIG. 17.—Four-Pole Magnet for a compact series of four magnifying telephone coils, each with its own microphone, or the last with a sound-board, or a bell-ringing relay.

The light trembling spring is, however, readily disturbed by any sort of shaking, and if the relay is too delicately adjusted it may give spurious calls due to mechanical tremors. From these it must be guarded.

The essential principle of a series of magnifying telephones is that each vibrating coil shall be connected electrically with one microphone and mechanically with another. By its electrical connection it receives a disturbance, and by its mechanical connection it passes it on. At each microphone fresh energy is introduced into the circuit from a local battery, and hence the energy of the final disturbance, utilised for calling or for loud signalling, bears no relation to the original infinitesimal disturbance, which has virtually only to pull a trigger. Dr. Lodge.

It is not to be supposed that each coil of the magnifying series demands a separate magnet. Fig. 17 shows a four-pole magnet, on which a series of four magnifying telephones may be compactly mounted.

34. Continuous working at the subject for over a year on the part of Mr. Benjamin Davies and myself, with occasional aid from instrument makers, has naturally resulted in a mass of observations and experiments. What is now published is only a selection, but the paper is quite long enough.

For telegraphic communication with lighthouses, lightships, outlying coast stations, and possibly between ship and ship, the method of magnetic induction now elaborated, though it may be less sensational and more bulky than the method by Hertz waves, yet has certain advantages of its own; especially in the absence of capriciousness on the part of the receiving instrument, and in requiring no special skill on the part of the operators.

If there is hereafter any demand for really distant signalling without connecting cables, as, for instance, between scattered islands in the Pacific, it is certain that the resources of science are very far from being exhausted, and that thoroughly known principles can be applied to obtain a practical result.

Mr. W. H. PREECE: I am bound to confess, Sir, that I should like to have deferred the remarks I have to make until we have heard the paper from Mr. Evershed; but I cannot resist this early opportunity to thank, not only in my own name, but in the name of all the members of the Institution present, Professor Oliver Lodge for bringing this subject before us. To me, I am sorry to Mr. Preece.

Mr. Preece. say, it is an old subject, but to-night I am in the very novel position of being on the other side of the table. It is the first time that I have listened to anybody else holding forth on this subject of wireless telegraphy. It is exactly 16 years since I first brought the subject before the British Association; and year after year, from 1882 to the present time, I have never failed to report to the British Association the experiments that have been made during the year. At this late hour I am sure you will excuse my saying anything on my own work. We have had an admirable discourse from Professor Lodge. He has shown us a mode of magnifying sound that is most attractive. He did not illustrate this at Bristol as he has done to-night, but he explained his views and described his plans so clearly and so ably that I felt the time had arrived when certainly we should do something to test his mode of calling on a practical scale. To-morrow morning I am going to have the pleasure of an interview with Professor Lodge, and I think we shall come to some decision to give his system a practical test. I can only emphasise what Professor Lodge himself has said, and that is, that at the present moment there is only one actually practical system of wireless telegraphy in daily use, and that is between Lavernock and Flat Holm. As he has described, it is an absolutely practical system. The messages are sent daily from one fort to the other across 3·3 miles of water. The instruments are worked by the sappers there, and it has been at work now for many months, and during the whole of that time there has not been one single failure. The difficulty that we are trying to surmount—indeed, that we have surmounted—is in the call. I will not say one word on that point. Mr. Evershed will, on the next occasion, read a paper on that subject; and after his paper, perhaps, Sir, you will allow me to speak, and go a little further into the matter. On the present occasion I would simply confine myself to proposing a very sincere vote of thanks to Professor Lodge for bringing such a charming subject before us in such an elegant way.

The President. The PRESIDENT: You have already carried that vote of thanks in a very emphatic manner, without the formality of my putting it. But the vote usually comes at the end of the discussion, and

this paper is one of such importance that it certainly is entitled to very full discussion. The hour is now so far advanced that I think it is doubtful whether it could be completed, unless it were highly inconvenient to Dr. Lodge to attend another meeting on the 22nd. The President.

Professor OLIVER LODGE: Sir, I think the discussions at this Institution are a special feature, since they are so well reported. Engineering societies are the only ones I know that really lay themselves out for discussions, and that is why I have been anxious to bring this subject first before the members of the Institution; and if they do me the honour of discussing it on the 22nd, I shall make a point of trying to come up. Dr. Lodge.

The PRESIDENT: In that case, we will defer the reading of Mr. Evershed's paper to the 22nd. I have to announce that the scrutineers report the following candidates to have been duly elected:—

Members:

Charles Bright.
Valére Alfred Fynn.
Herbert Alfred Humphrey.
William Stewart Robertson.
Albert Henry Unwin.

Associates:

Harry O. F. Bindemann.
James Wm. Garside.
Ernest Vincent Graham.
William H. Grimsdale.
James Hall.
Sydney E. Hall.
John Ernest Hutton.
Walter T. Le Feuvre.
Frederick Vincent Lee Mathias.
Herbert Edward Moul.
David Owen, B.A., B.Sc.
William Benjamin Pinching.
Geoffrey Porter.
William Thom.

Students:

William J. Cooper.
Thomas Cunningham Cunningham.
John Eustace.
Herbert J. Jones.
Lllewellyn R. Lester.
Reginald Pearson Russell.

The Three Hundred and Twenty-third Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday, December 22nd, 1898—Mr. JOSEPH W. SWAN, F.R.S., President, in the Chair.

The minutes of the Ordinary General Meeting held on December 15th, 1898, were read and approved.

The following transfers were announced as having been approved by the Council:—

From the class of Associates to that of Members—

H. Dudley Barlow.	Charles F. Johnston.
Leonard Barlow.	Hubert Russell.

William Edward Sumpner.

From the class of Students to that of Associates—

Horatio H. Bentley.	Frederick Watson Joel.

Norman McLaurence.

A donation to the Library was announced as having been received since the last meeting from Mr. R. K. Gray, to whom the thanks of the meeting were unanimously accorded.

TELEGRAPHY BY MAGNETIC INDUCTION.

By S. EVERSHED, Associate.

Mr. Evershed

Although many practical trials of telegraphy by means of the mutual induction between two distant circuits have been made during the past ten years, this form of wireless telegraphy has of late been rather overshadowed by the more brilliant possibilities of telegraphy by means of Hertzian waves of high frequency, and the low-frequency electro-magnetic system has hardly received the attention it deserves. It is probably not capable of such general application as Hertzian telegraphy, but it has a distinct advantage in employing electro-magnetic waves

of low frequency, the laws of which are simple and well understood, and the necessary apparatus for transmitting and receiving will not be too remote in type from dynamos and motors to enable us to readily understand the principles on which their design and working should be based. Moreover, with one limitation, to which reference will afterwards be made, our knowledge of the laws of magnetic induction is sufficient to enable us to determine precisely what conditions are requisite for communication with a place at any distance. Mr. Evershed.

The author's attention was directed to the subject some years ago in connection with communication between light-vessels and the shore, and his magnetic induction telegraph was, as is pretty generally known, tried between the North Sand Head light-vessel and Dumpton Gap, and proved a total failure, owing to the almost complete absorption of the electro-magnetic waves by the media through which they were required to pass.

During the time these trials were in preparation exhaustive experiments were carried out at Woodfield Works by the author, in conjunction with Mr. Oswald Cox, and the main principles of telegraphy by magnetic induction were investigated, and one or two fundamental formulæ worked out. It will not be inappropriate at the present time, when wireless telegraphy is coming into prominence, to put the conditions then arrived at before the members of the Institution as briefly as possible.

When we desire to convey energy through space from one place to another by electro-magnetic waves, we must provide a primary circuit at one place to emit the induction waves, and a secondary circuit at the other place to receive them. Every time a current is varied in any way in the primary a certain fraction of the energy linked with this circuit will appear in the secondary, to run down there into heat (as $C^2 R$), unless there is a motor or other translational device included in the secondary. By maintaining an alternate current in the primary a continuous supply of energy is available in the secondary, and a suitable indicator in the secondary—that is to say, a motor adapted to convert part of the received electrical energy into energy of motion, either visible or audible—will enable the receipt of the

Mr. Evershed. energy to be detected. Break up the primary current into dots and dashes and you have a working telegraph.

Of course there are other means of indication which are not in any ordinary sense motors. For example, electrolytic effects may give a direct indication of the secondary current, or a coherer may be used to give indirect indication by closing a local circuit. But the principles underlying the application of magnetic induction to wireless telegraphy will be better understood if we assume the indicating device to convert some part of the received energy into motion. It may be motion only capable of bringing two contacts together to close a local circuit, or capable of making audible sound waves in the air, or it may be directly visible; but in any case something is made to move; and the moving body being, presumably, attached to some support, either by pivots or an elastic suspension, cannot be moved without the expenditure of energy to overcome the mechanical friction of the pivots or the molecular friction of the suspension. If sound waves are set up there is a further expenditure of energy.

The secondary current being of course an alternating one, it is probable that the motion imparted to the indicating device will be oscillatory also. The ordinary Bell telephone will occur to everyone as typical of such an indicating device. The friction—resistance to motion—may follow any law with regard to velocity, it may even be constant (independent of velocity), but in every case examined by the author the frictional resistance has proved to be simply proportional to the velocity—at all events at such frequencies as are likely to be used in practice. In what follows it will be assumed that when moving with velocity v the moving body requires a force equal to $r\,v$ to overcome friction, r being a constant for any particular moving system. Calling the amplitude of oscillation a, and if $p = 2\,\pi\,F$, the mean power required to oscillate the moving part will be $= \frac{1}{2}\,r\,a^2\,p^2$, and at its maximum value the power spent in overcoming friction will be $r\,a^2\,p^2$; the oscillation being a simple sine function.

Having got an indicator, the first step towards its application to an induction telegraph must be to determine the value of the coefficient r, and to decide upon a value for the amplitude a

which will give indications of the necessary magnitude. These two data will enable primary and secondary circuits to be designed to give the required secondary power with any given primary power. Mr. Evershed.

THE INDUCTIVE CIRCUITS.

There are two practicable ways of building such circuits. The first consists of running a pole line of sufficient length at each end of, and at right angles to, the imaginary line joining the two points between which communication is to be established. Each pole line being earthed at both ends, the circuits complete themselves through the earth, and form two circuits in parallel vertical planes. This is the system devised by Mr. W. H. Preece. It was in practical use long before any other system of wireless telegraphy was heard of, and, indeed, it is very largely due to Mr. Preece's early and continuous work in this connection that we have any wireless telegraphy to discuss to-day. The efficiency of this system, taking cost of materials as a measure of efficiency, depends on the average depth at which the current travels in returning through the earth from one earth pole to the other. Many experiments have been made by the Post Office authorities to determine the mean depth of the return path, but the author understands that the results do not indicate any definite law governing the relation between length of pole line and depth of return. In these circumstances it becomes impossible to accurately forecast what any proposed pair of lines will do. Moreover, leakage from one circuit to the other complicates matters.

The other method of forming the primary and secondary circuits consists in running a pole line completely round the boundary of a sufficient area of ground at each station. This in effect turns Mr. Preece's two vertical circuits into two horizontal circuits, and there is the immediate loss of one-half the mutual induction between them—an effect only compensated for if the area of the horizontal circuits can be increased. If the areas of the primary and secondary circuits are A_p and A_s respectively, and the distance between them is D (D being large compared with

Mr. Evershed.

the diameters of the circuits), then the total induction threading the secondary,

$$B = \frac{2\ A_p\ A_s\ C}{D^3} \quad \dots \quad \dots \quad (1)$$

when the circuits are in parallel planes normal to the line joining their centres; while in the case of the two circuits lying in the same plane

$$B = \frac{A_p\ A_s\ C}{D^3} \quad \dots \quad \dots \quad \dots \quad (2)$$

Another point to bear in mind when comparing the vertical and horizontal systems is that in the former a pole line is only used over half the whole length of circuit, the remainder being made up by the earth return. In the latter the pole line extends right round the circuit, and is on that account likely to prove more costly.

However, horizontal circuits possess compensating advantages which may easily outweigh their drawbacks, and in the following formulæ for determining the volume of copper in the lines, areas of circuits, and primary power required for signalling over any given distance, the circuits are supposed to be laid in a horizontal plane.

The expressions given in (1) and (2) show that circular circuits will give the maximum effect for a given length of pole line. But this is an ideal form which could hardly be contemplated in practice, and it will therefore be assumed that each circuit forms the boundary of a square area. Since each circuit will have to serve for both transmitting and receiving, it is clear that they must, if possible, be identical in size and shape.

Let

V_p, N_p, R_p be the total volume of wire, number of turns, and resistance of the primary circuit.

V_s, N_s, R_s the corresponding values for the secondary circuit.

a sectional area of line wire.

ρ specific resistance of line wire.

s length of side of square circuit.

D distance from centre of primary to centre of secondary.

Mr. Evershed

C_p value of primary current.
W_p value of power spent in primary.
W_s total electrical power in secondary.
B total magnetic induction threading secondary.
E_s impressed E.M.F. in secondary.
C_s current in secondary.
M mechanical power of indicator or motor in secondary.

All the cyclic quantities are assumed to be simple sine functions, and the letters denoting them stand for their maximum (crest of wave) values.

The whole of the power required in the primary circuit is assumed to be that wasted in heating the wires. To be strictly accurate, the power radiated into space, of which a minute fraction appears in the secondary, should be added; but at the low frequencies suitable for working the whole radiated power is very small compared with $C^2_p R_p$.

To get the greatest possible conversion into mechanical energy in the secondary, E_s must be in step with C_s, and the condition for maximum activity of a motor must be fulfilled. As is well known, this condition requires the back E.M.F. of the motor to be equal to one-half the impressed E.M.F. The highest possible value for the mechanical power of the secondary indicator is, therefore,

$$M = \tfrac{1}{2} E_s C_s.$$

But
$$C_s = \frac{E_s}{2 R_s} \text{; so that } M = \frac{E_s^2}{4 R_s}.$$

Now
$$E_s = p B = \frac{p s^4 N_s N_p C_p}{D^3}\text{; and since } C_p = \sqrt{\frac{W_p}{R_p}},$$

$$R_p = \frac{4 \rho s N_p}{a}, \text{ and } a = \frac{V_p}{4 s N_p}, \text{ we have—}$$

$$E_s^2 = \frac{p^2 s^6 N_s^2 W_p V_p}{16 D^6 \rho} \quad \dots \quad \dots \quad (3)$$

also
$$R_s = \frac{16 \rho s^2 N_s^2}{V_s};$$

so that, finally,

$$M = \frac{E_s^2}{4 R_s} = \frac{p^2 s^4 W_p V_p V_s}{16^2 D^6 \rho^2} \quad \dots \quad (4)$$

Here we have an expression for the power turned into audible, visible, or in any other way discoverable motion by an indicator

Mr. Evershed. considered as a motor—a motor which is only required to overcome its own mechanical friction, and working under the ideal conditions of maximum activity, negligible electrical resistance and self-inductance in the motor coil, impressed E.M.F. and current in step,* and, lastly, *absence of any medium which can absorb* or distort the magnetic waves on their road between the two circuits.

As might have been anticipated, the mechanical power does not depend in any way on the number of turns of wire into which V_p and V_s may be divided. The number of turns in primary and secondary will depend only on the E.M.F. of primary generator, and E.M.F. for which the motor is designed; these are questions entirely distinct from the mechanical work to be done, just as the bearing friction in an ordinary motor is quite independent of the volts and amperes of the machine. Formula (4) also indicates that the least volume of wire will be used if we make $V_p = V_s$; and in that case, if we put V_t for the total volume of wire used in the construction of the two circuits, (4) becomes

$$M = \frac{p^2 s^4 W_p V_t^2}{32^2 D^6 \rho^2} \quad \dots \quad \dots \quad (4^a)$$

At this point it will be useful, by working out an example, to give an idea of the mechanical power which can be delivered at a distance of several miles by the expenditure of a reasonable amount of power in the primary circuit, and by means of a practicable weight of wire.

Suppose we put 1,000 kilogrammes (about 1 ton) of copper wire into circuits 1,000 metres square and 10 kilometres apart. Let the frequency be 100 ∿, and the mean power spent in the primary circuit be 100 watts. That is to say,

$$p = 628,\ s = 10^5 \text{ cm.},\ W_p = 200 \times 10^7 \text{ ergs per second,}$$

$$V_t = \frac{10^6}{8{\cdot}9} = 1{\cdot}1 \times 10^5 \text{ c.c.},\ \rho = 1{\cdot}7 \times 10^3 \text{ C.G.S. units.}$$

$$\text{and } D = 10^6 \text{ cm.}$$

If these values are put into (4^a), we get

$$M = 0{\cdot}34 \text{ ergs per second.}$$

---

* This applies to both the primary and secondary circuits; when the self-induction of the circuits is appreciable, the current must be brought into the same phase ("in step") as the E.M.F. by means of condensers, or other means.

Now, as will presently appear, it is quite easy to obtain indicating instruments which will work with less than one-third of an erg per second; but it must not be forgotten that the value just given for the mechanical power available is under ideal conditions that can only be approached—never reached. Mr. Evershed.

Transmitting Devices.

It is not necessary to discuss in any detail the apparatus required for producing the alternate or interrupted current in the primary circuit. An interrupted current may be derived from batteries by means of a rotary contact wheel driven by clockwork or by an electro-motor. For telegraphing over great distances the primary power must be considerable, and it will probably prove more economical and convenient to have a small alternator driven by an oil or gas engine. No particular difficulty is likely to be met with in designing a compact plant for the purpose.

Receiving Devices.

In every instance, so far as the author is aware, in which communication has been carried on by magnetic induction or by earth leakage, the ordinary Bell telephone has been used as a "sounder." It is well adapted for the purpose, as it enables exceedingly minute currents to be detected. This sensitiveness does not arise so much from the efficiency of the telephone as a current-detector as from the marvellous delicacy of the human ear, which is able to distinguish sound waves produced by motions so exceedingly small as to be absolutely invisible, even when magnified many times. In the course of the investigation already referred to the author made careful measurements of the magnetic and electrical quantities involved in the telephone, but up to the present he has been unable to devise adequate means for measuring what fraction of the electrical power supplied is converted into mechanical power. This conversion can be estimated for any given amplitude of the diaphragm from a knowledge of the change in induction threading the coils for different displacements of the diaphragm; obtaining in this way the back E.M.F. But the difficulty remains of determining the amplitude when an alternate current is flowing in the coils.

Mr. Evershed.

However, in the case of the telephone it is not essential to know the mechanical power in order to ascertain the conditions for working to a given distance. By measuring the alternate current required to produce readable signals, it is possible to deduce with fair accuracy the distance over which Morse code signals can be sent. The author has measured the current flowing in a Post Office pattern Gower-Bell telephone while receiving Morse signals readable by a skilled operator in a quiet room—"readable" implying that the message could be read without difficulty. Two operators were found to agree very closely in their estimate, and it appeared that the current for readable signalling was nearly twice the minimum current which produced an audible sound. The current was determined by connecting the telephone to a circular ring of wire through which a measured induction was alternating. The resistance and self-induction of the telephone were separately determined, and the whole impedance was sufficiently large to enable the current to be estimated without reference to the back E.M.F. of the vibrating diaphragm. The mean of several observations gave for the maximum (crest of wave) value of the readable current,

$$C = \frac{2 \cdot 9}{10^6} \text{ amperes.}$$

A current of one-half the above value makes a just audible sound.

When a telephone of this type is used as receiver on the secondary of an induction telegraph, the only condition to be fulfilled to get the best effect is that the resistance of the secondary circuit should equal that of the telephone. This is only another way of saying that the telephone is a very badly designed motor, with such an enormous combined resistance and inductive drop that the current is quite independent of its back E.M.F. A large component of the current—namely, that required to force the vibrations—is, of course, in step with the diaphragm, and therefore in quadrature with the back E.M.F.; in short, an idle* current in the sense that it does no mechanical work.

* The author regrets to see that Dr. Thompson sanctions the inappropriate phrase "wattless current" for a current in quadrature with the E.M.F. Such

Suppose this particular telephone is connected to the secondary of the two circuits which are given above as an example. To get maximum current we must either wind the telephone to equal the line resistance, or divide V_s into a sufficient number of turns to bring R_s up to the resistance of the telephone. The resulting ampere-turns in the telephone coils is the same in either case, and it will be simpler to choose the latter alternative, although in practice the telephone would, of course, be wound to suit the line. Using the same notation as before, and calling x the impedance of the telephone coils at frequency F, we have first to make

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$$\frac{16\,\rho\,s^2\,N_s^2}{V_s} = x;$$

that is,

$$N_s = \frac{\sqrt{V_s\,x}}{4\,p\sqrt{\rho}}.$$

Now

$$C_s = \frac{E_s}{2\,x} = \frac{p\,s^3\,N_s\sqrt{W_p\,V_p}}{8\,D^3\,x\,\sqrt{\rho}};$$

and, substituting the value just found for N_s, we find

$$C_s = \frac{p\,s^2\,V_s\sqrt{W_p}}{128\,D^3\,\rho\sqrt{x}} \quad \ldots \quad \ldots \quad (5)$$

Working out the value of C_s from the data already given, the frequency being now 400 $\sim$ per second—the rate adopted by the Post Office after many trials at different speeds—and the impedance of the telephone $x = 220$ "ohms" at 400 $\sim$, we find

$$C_s = \frac{12}{10^6} \text{ amperes};$$

that is to say, a little more than four times (4·1) the minimum for readable signals; so that the weight of copper in the lines might be cut down to a quarter of a ton, or the distance increased by multiplying by $\sqrt[3]{4{\cdot}1}$, making the distance between the centres of primary and secondary circuits 16 kilometres (10 miles).

It is clear, therefore, that in the absence of wave-absorption an ordinary telephone without any modification will enable

currents are, alas! only too watt*ful*,—they are never watt*less*. But they are distinctly idle, in that they never do any useful work.

Mr. Evershed.

induction telegraphy to be carried on at considerable distances. In the absence of any measurements of the absorption of electro-magnetic waves by the earth it is, of course, impossible to predict to what distance it will be possible to signal by means of a system working at 400 periods per second, but the author is inclined to think that at this speed absorption will rapidly damp out the waves as the distance increases.

The telephone may easily be improved for signalling purposes. When articulation is not required the pitch of the diaphragm may be lowered with advantage. During the working out of the apparatus for the lightship telegraph Mr. Cox devised several forms of telephone sounders in which the coil was rigidly attached to the diaphragm, and adapted to move in a powerful magnetic field. One or two instruments of this type were made, and worked well. Time, however, did not allow of our bringing any of them to perfection. But in whatever way the telephone may be improved as a motor, the most serious difficulty will always remain. To get an easily audible note it is necessary to work at a fairly high frequency. At 100 periods per second one can just hear a low hum in a telephone, but it is not until the frequency reaches 200 or 300 that dots and dashes become at all distinct. At 400 periods the note is sufficiently piercing to enable an operator to read a message without any serious strain.

Whatever form the telephone as a "sounder" may ultimately take, its suitability for induction telegraphy must depend on the degree of absorption met with in practice. If absorption is serious, audible indications must be abandoned, and an indicator working at a very low frequency will become necessary. What form such an indicator should take for Morse code signalling is by no means clear, but a simple form of vibratory indicator devised by the author in 1892 for use in connection with the light-vessel experiments may be described, as it illustrates very clearly the principles involved in all vibratory indicators of minute alternate currents, and possibly contains the germ of a practicable "speaking" instrument.

The essential parts of the author's indicator are shown in Fig. 1. A rectangular loop of fine wire, *a*, *b*, *c*, *d*, is fixed in an

insulating block in a position which allows its end b–c to move freely in the air gap of a magnet, N S. When the rectangle is traversed by an alternate current it vibrates up and down about its fixed ends a–d, and if the rectangle is tuned so that its free period of vibration is equal to the periodic time of the current, it becomes an exceedingly sensitive alternate-current voltmeter,—the amplitude being strictly proportional to the impressed E.M.F. on the terminals of the rectangle. Mr. Evershed.

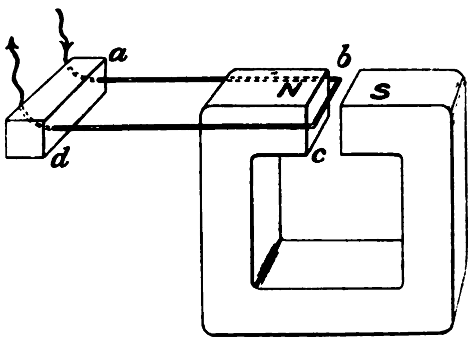

FIG. 1.—Vibrating Rectangle, with Magnet.

By combining the mechanical equation of motion with the equation of the electrical quantities concerned, the following simple equation is arrived at for the amplitude in terms of the molecular and other frictional resistances to motion, the condition of synchronism or resonance being assumed:—

Let

a = amplitude at end b–c.

r = force required at end b–c to overcome frictional resistances at unit velocity.

R = electrical resistance of circuit, including rectangle.

L = self-induction of circuit, including rectangle.

E = impressed E.M.F.

h = H l = field in air gap × length b–c.

Then
$$a = \frac{\mathrm{E}\,h}{p\sqrt{(h^2 + \mathrm{R}\,r)^2 + r^2 p^2 \mathrm{L}^2}} \quad \ldots \quad (6)$$

Also, if i and v are instantaneous values of the current in, and velocity at end of rectangle,

$$h\,i = r\,v.$$

Mr. Evershed.

and the maximum velocity is $p\,a$; so that, if C is the maximum value of current,

$$C = \frac{r\,p\,a}{h}.$$

Also the maximum back E.M.F. $-\ V = p\,a\,h$; hence maximum power is

$$V\,C = r\,a^2\,p^2 \quad \ldots \quad \ldots \quad \ldots \quad (7)$$

By differentiating (6) it is seen that a will have the greatest possible value when the field is adjusted until

$$h^2 = r\sqrt{R^2 \times p^2\,L^2}.$$

Writing R^1 for $\sqrt{R^2 + p^2\,L^2}$, we have for the value of amplitude under most favourable conditions,

$$a = \frac{E}{p\sqrt{2\,r\,(R + R^1)}} \quad \ldots \quad \ldots \quad (8)$$

The condition for maximum amplitude is clearly that for maximum activity also, and accordingly we find that when $L = 0$ and $h^2 = R\,r$, the back E.M.F. is equal to half the impressed E.M.F.

So much for the theory of this simple alternate-current synchronous motor. It is gratifying to add that its actual behaviour is, within the limits of observational error, strictly in accordance with theory.

By adding a fixed contact screw adjacent to the free end of the rectangle a local circuit will be closed whenever the vibrations are sufficient to make the rectangle touch the contact screw. Another and better arrangement is to have two rectangles connected to two separate secondary circuits in such a direction that they oscillate in opposite phase. The two rectangles being made, the poles of a local circuit can be made to act as a relay on coming into contact with each other. Two relays of this type were made by the author's firm in 1896 for use in the light-vessel trials, and a few months ago they were attached to the wireless telegraph erected by the Post Office for communicating between Lavernock and Flat Holm, in the Bristol Channel. They are in daily use as relays for closing a call-bell circuit The primary current is derived from a small alternator with a very heavy fly-wheel on the armature shaft. The machine being

driven by hand to a speed above the synchronising frequency, the exciting current is switched on, and the alternator allowed to come gradually to rest,—passing slowly through the synchronising speed, so that the two rectangles of the distant relay have time to come to their maximum amplitude. Mr. Evershed.

The rectangles in these two relays are made of iridio-platinum wire 3 mils in diameter. They are each 4 cm. in length, 2 cm. in breadth, and their frequency is 16 periods per second. The twin-rectangle relay has one great advantage over the single pattern shown in Fig. 1. When the twin rectangles are properly tuned to unison it is almost impossible to bring them into contact by shaking the instrument. Even when the contact points of the rectangles are separated by only 0·002 inch, it is difficult to make contact by shaking the relay. They may be oscillating over a large angle; appearing to overlap each other, but oscillating in unison and in the same phase, as the rectangles do when shaken, they do not come into contact. These relays were designed for use on board a light-vessel, and entire freedom from accidental contact due to shaking was, of course, essential.

Measurements of the value of the frictional resistances of rectangles made of copper, platinum, and iridio-platinum were made, and showed very little difference between them. The value of r was determined by means of a vibrator like Fig. 1, having its field produced by an electro-magnet, so that h could be varied. The process consisted in observing the amplitude at synchronism for different values of h, and plotting the curve connecting them. There being no self-induction in the circuit, the amplitude follows the law,

$$a = \frac{E\,h}{p\,(h^2 + R\,r)}.$$

The impressed E.M.F. was provided by a secondary circuit of a single turn in which the total induction, B, could be measured. That is to say, $E = p\,B$; so that

$$a = \frac{B\,h}{h^2 + R\,r} \qquad \ldots \qquad \ldots \qquad (9)$$

Having plotted the observed values of a and h, a curve is

Mr. Evershed.

determined by means of equation (9) to fit the observed points as closely as possible. One of the curves is given in Fig. 2. It shows the relation between a and h for a rectangle of iridio-platinum wire 3·3 mils diameter and 2·9 cm. long, having a frequency of 25 periods per second. The curve is drawn from the equation,

$$a = \frac{1{,}770\, h}{h^2 + 25 \times 10^6} \text{ cm.};$$

so that $\quad R\, r = 25 \times 10^6$ C.G.S. units.

Now $\quad R = 4{\cdot}28$ ohms $= 4{\cdot}28 \times 10^9$ C.G.S. units; hence

$$r = \frac{25}{4{\cdot}28 \times 10^3} = \frac{5{\cdot}85}{1{,}000} \text{ dynes.}$$

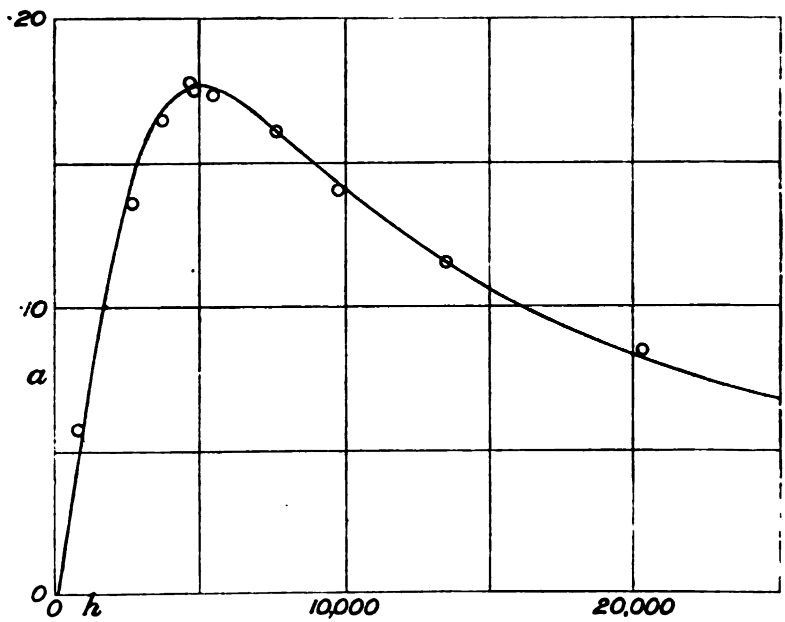

FIG. 2.—Curve connecting a and h for Iridio-Platinum Rectangle; a in centimetrès, h in C.G.S. lines.

In the relays on the Lavernock–Flat Holm telegraphs the greater length of rectangle—4 cm.—results in a lower value for r, namely,

$$r = \frac{4{\cdot}0}{1{,}000} \text{ dynes.}$$

The power required to work a 4-cm. rectangle at any amplitude can now be calculated from the value for r just given, and the known frequency—16 periods per second—for, as we have already seen, the mechanical power expended is Mr. Evershed.

$$M = r\ a^2\ p^2.$$

Let $r = \frac{4}{1,000}$, $a = \frac{1}{10}$ cm., $p = 2\ \pi \times 16 = 100$:

then $M = 0{\cdot}4$ ergs per second ($a = 0{\cdot}1$ cm.);

or, if $a = 0{\cdot}01$ cm.,

$M = 0{\cdot}004$ ergs per second ($a = 0{\cdot}01$ cm.);

and so on.

An amplitude of one-tenth of a millimetre is, however, far more than is necessary under favourable conditions. The author has repeatedly worked a twin-rectangle relay with less than 2 mils between the contacts, and a relay was purposely set up at Woodfield Works with the contacts 2 mils apart to ascertain how often accidental shaking made contact. The relay was tested every day by alternate current to see that it was in working order, but in the course of a three weeks' trial no accidental contact occurred.

Taking 2 mils—say 0·005 cm.—as the limiting value for the clearance between contacts, the mechanical power expended becomes

$M = 0{\cdot}001$ ergs per second ($a = 0{\cdot}005$ cm.).

This must be considered as the limit of sensibility for this particular relay. When the clearance is reduced to less than 2 mils, difficulties arise from the electrostatic attraction between the rectangles drawing them into contact, and when in contact the elastic force of the wires is insufficient to pull them apart again.

It is clear, however, that the energy received in a secondary circuit can be detected by means of a synchronous vibrator of the kind described in which frictional resistance has been reduced to 4×10^{-3} dynes. Returning to equation (4″), and assuming the resistance of the rectangle is negligible compared with the least possible resistance of the secondary circuit, we can now write—

$$M = r\ a^2\ p^2 = \frac{p^2\ s^4\ W_p\ V_r^2}{32^2\ D^6\ \rho^2}.$$

Mr. Evershed.

Hence,
$$a = \frac{s^2\, V_t \sqrt{W_p}}{32\, D^3 \rho \sqrt{r}} \qquad \ldots \qquad \ldots \qquad (10)$$

When, as in the former example, $V_t = 1{\cdot}1 \times 10^5$ c.c., $s = 10^5$ cm., $W_p = 200$ watts, $D = 10$ kilometres, $\rho = 1{\cdot}7 \times 10^{-6}$ ohms, and, in addition, $r = 4 \times 10^{-3}$ dynes, the amplitude will be

$$a = 0{\cdot}014 \text{ cm. (about } 5\tfrac{1}{2} \text{ mils)};$$

so that, if the rectangle is used as a relay, and set to its maximum sensibility, it would be possible to work with a weight of copper amounting to

$$1{,}000 \text{ kilos.} \times \frac{0{\cdot}005}{0{\cdot}014} = 350 \text{ kilos.},$$

or about one-third of a ton.

Or, still using 1,000 kilos., the distance may be increased by multiplying by $\sqrt[3]{\frac{0{\cdot}014}{0{\cdot}005}} = 1{\cdot}42$, bringing up the distance to 14·2 kilometres—a value only a little less than the limiting distance found for readable signals with a telephone as receiver. A very small rate of absorption of waves would, however, tell enormously against the telephone, while the vibrating rectangle, working at so low a frequency as 16 periods per second, would not be appreciably affected.

It is interesting to note that when the telephone and the rectangle are worked under exactly the same conditions as regards primary power and distance and size of circuits, but at frequencies of 400 and 16 periods respectively, the total power available for the telephone is more than six hundred times as great as that available for the rectangle. Yet the rectangle gives a *visible* indication—for by means of a magnifying glass the vibrations are easily visible down to about half a mil.

It has already been remarked that this synchronous vibrator may contain the germ of a "speaking" instrument. It has ample sensibility for practical purposes, and is not likely to be affected by a moderate rate of absorption. It is to be hoped that some means will be discovered for enabling it to be used for indicating Morse code signals, either directly or by employing it as a relay.

Absorption.

Mr. Evershed.

So far as the author is aware, the absorption of electromagnetic waves by the earth—considered as a solid conductor—has never been calculated or measured under the conditions assumed in this paper. What is wanted is a complete series of observations of the absorption at different frequencies and over different distances, and it would be desirable that observations should be made in several different localities of widely differing geological character. The author hopes to be able to carry out at least a part of this investigation in the course of the next few years.

Apart from absorption, it is clear that communication by magnetic induction can easily be established in those cases where the ordinary telegraph line or cable is impracticable. The author has endeavoured to illustrate the fundamental principles involved in the working of induction telegraphs, and if he has in any way cleared the ground for future investigators and thrown a little light on matters relating to the design of appropriate receiving apparatus, the object of this paper will have been fulfilled. In conclusion, the author desires to express his sense of the great obligation he is under to Mr. Preece and Mr. Gavey for their kindness in giving facilities for his investigations, and placing at his disposal the results of the many experiments carried out by them in connection with wireless telegraphy.

ÆTHERIC TELEGRAPHY.

By W. H. Preece, C.B., F.R.S. (Past-President).

I have never submitted to the Institution of Electrical Engineers any record of my long and continuous researches on signalling through space. My reports have been made from year to year to Section A of the British Association. Some of them not having been printed, it is not to be wondered at that all my work is not known, and so much of it is occasionally reproduced by others. Nothing has been patented, and the work done is open to everyone. Mr. Preece.

Mr. Preece. I will confine my remarks in this paper to ætheric telegraphy, and to its electro-magnetic form alone.

In 1884* disturbances—that is, stray currents producing extraneous noises on the telephone—were detected on certain circuits erected over the house-tops in London, produced by currents in telegraph wires buried in iron pipes in the streets. Messages sent on a telegraph wire were actually read on the telephone circuit, though the wires were nowhere in contact. Exhaustive experiments were made to prove that these results were due to induction and not conduction. This was done by making the circuits metallic. They were separated from each other by a space of 80 feet.

Many preliminary laboratory experiments were made with coils of many turns, but in 1885 square coils of insulated single wire, each side being of 440 yards length, were laid horizontally on the ground in the neighbourhood of Newcastle, and conversation by telephone was effected from one to the other through a space of one-quarter of a mile. Disturbances between these squares were appreciable at 3,000 feet, and even at a distance of 10½ miles between parallel lines of telegraph connecting Durham and Darlington. In 1886† experiments were carried out across the Severn for a length of 14 miles between Bristol and Gloucester, the parallel wires being separated by a mean distance of 4·5 miles. Primary currents of 0·449 ampere were rapidly made and broken by mechanical means, producing on the telephone a continuous note which could be conveniently broken up by a Morse key into dots and dashes, as in Cardew's vibrator. Disturbances were observed in the secondary circuit. The circuits were metallic and of many miles in length, that on the Gloucester side being completed through Stroud, and that on the Welsh side through Monmouth. Here we had just reached the limit of disturbance with the means at our disposal. It afforded a convenient measure of the range of audibility. The unexpected fact was shown that, whether the circuits were metallic or earthed at the

* "Induction between Wires and Wires," B.A. Report, Birmingham, 1886; *The Electrician*, vol. xvii., p. 410.

† "On Induction between Wires and Wires," B.A., Manchester, 1887; *The Electrician*, vol. xix., p. 461.

ends of the parallels, the results were the same. This was confirmed Mr. Preece. subsequently by innumerable experiments, especially by some results obtained between Arran and Kintyre in 1894.* All our experience since has shown that a circuit of one single turn of thick copper wire, and of the largest possible diameter, gives the best result, and signals to the greatest distance. Moreover, it will be shown that the greater portion of this circuit may be made up by the earth. In a single turn thus formed we have the full beneficial effect of electro-magnetic as well as of electrostatic induction, we have the advantage of earth conduction, and we reduce capacity and inductance to their minimum. In closed coils of many turns we have only electro-magnetic induction, and we make inductance a maximum. In the autumn of the same year (1886) the vast expanse of sand at Porthcawl, in South Wales, was used to further develop the law determining these effects.† Two horizontal squares of 300 yards per side were placed 300 yards apart, and subsequently one of them was suspended on scaffold poles 15 feet above the other, which was covered with water at high tide. No difference was detected in the strength of signals, whether the space were air or water, or a combination of air and water. Subsequent experiment (1893), however, showed that through 15 feet the effect in air was distinctly better than through water.

The conclusion then arrived at was that the magnetic field extends uninterruptedly through the earth, as it does through the air, and that if the secondary circuit had been in a coal-pit it would have been equally evident. In fact, Mr. Arthur Heaviside succeeded in 1887 in communicating between the surface and the galleries of Broomhill Colliery, 350 feet deep. Subsequent experiments showed that the conclusion then arrived at was not true for water, for, though we have spoken in Dover Harbour telephonically through 36 feet, no practical signals could be detected through 400 feet of water off the Goodwin Sands.

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* "Signalling through Space," B.A., Oxford, 1894; *The Electrician*, vol. xxxiii., p. 460.

† "On Induction between Wires and Wires," B.A., 1897; *loc. cit.*

Mr. Preece. Hence the effect must diminish in water with some high power of the distance.

A new telegraph line of copper wires, running parallel to the Great Western Railway, in 1886 offered convenient opportunities to experiment further. The fact that parallel wires followed the same law as metallic coils facilitated such experiments. Horizontal coils of large diameter lying on the ground are impractical things, and I was glad to get rid of them. It was determined that with two parallel wires of 1 mile length, the primary wire being excited by 1 ampere, the limit of audibility was reached at 1·9016 miles, and that

$$x = 1{\cdot}9016 \sqrt{\frac{C_1 l}{r_2}}$$

gave a fairly accurate empirical formula to determine the maximum distance, x, which should separate any two wires of length l, C_1 being the primary current, and r_2 the resistance of the secondary wire. This was amply confirmed by experiments repeated in the Severn Valley in the same year,* and it established the fact that it was advisable to use copper conductors of the largest gauge that could be obtained.

It was also determined that a very important element of success was the rate at which the currents rise and fall, and that every cause of retardation, such as capacity and self-induction, should be eliminated from the circuits.

A great many experiments were made at Lisvane in Glamorganshire (1887), Loch Ness (1892), the Conway estuary (1893), Frodsham, on the estuary of the Dee (1894), Wimbledon (1894).†

But the most satisfactory results were obtained in the Bristol Channel (1892),‡ between Lavernock and Flat Holm, 3·3 miles apart, between which places messages passed freely. In fact, at the present time this line has been re-erected and made permanent. It is in actual practical daily use, and has never failed ever since it

* "On Induction between Wires and Wires," B.A., 1886; *loc. cit.*

† "Signalling through Space," B.A., 1894; *loc. cit.*

‡ "The Transmission of Electric Signals through Space," Chicago International Electrical Congress, 1893.

was established in March, 1898. The Pyke and Harris alternator, which was used in 1892, has been replaced by dry cells. The frequency has been raised to 400. The rate of rising and falling of the current has been immensely improved. The resistance of the circuit has been made as low as possible. There is no measurable capacity, self-induction is eliminated, and there is no impedance. Hence the signals are simply splendid, and their rate of working dependent only on the skill of the operator. **Mr. Preece.**

The conception of the function of the earth as the completion of the circuit of a single coil has been thoroughly formulated. The earth acts simply as a conductor, and *per se* it is a very poor conductor, deriving its conducting property principally, and often solely, from the moisture it contains. On the other hand,

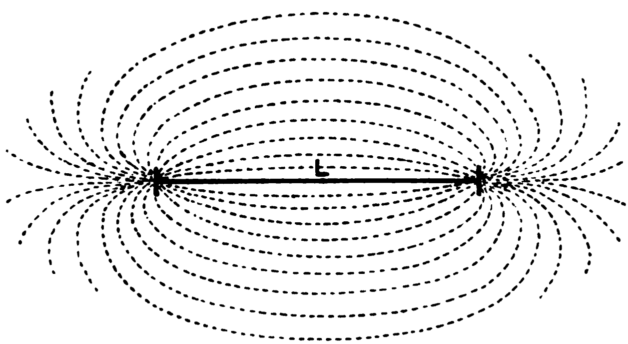


FIG. 1.

the resistance of the "earth" between the two earth-plates of a good circuit is practically nothing. Hence it follows that the mass of earth which forms the return portion of a circuit must be very great indeed, for we know that the resistance of a circuit increases with its specific resistance and length, and diminishes with its sectional area. Now, if the material forming the "earth" portion of the circuit were, like the sea, homogeneous, the current-flow between the earth-plates would follow innumerable but definite stream lines, which, if traced and plotted out, would form a hemispheroid. These lines of current have been traced and measured. A horizontal plan on the surface of the earth is of the form illustrated in Fig. 1, while a

Mr. Preece. vertical section through the earth is of the form shown in Fig. 2.

With earth-plates 1,200 yards apart these currents have been found on the surface at a distance of half a mile behind each plate; and, in a line joining the two, they are evident at a similar distance transversely at right angles to this line.

Now this hemispheroidal mass could be replaced electrically by a resultant conductor (R, Fig. 2) of a definite form, resistance, and position; and, in considering the inductive action between two circuits having earth returns, it is necessary to estimate the position of this imaginary conductor. This was the object of the experiments at Frodsham.

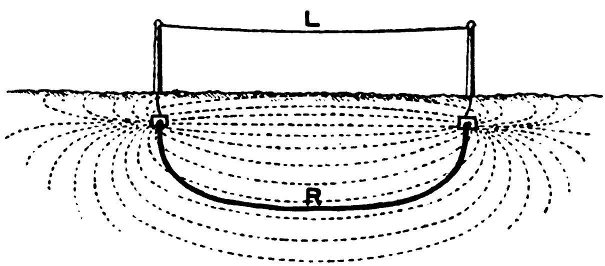


FIG. 2.

If the material of the earth be variable and dry the hemispheroid must become very much deformed and the section very irregular, the lines of current-flow must spread out further, but the principle is the same, and there must be a resultant return. The general result of the experiments at Frodsham indicate that the depth of the resultant earth was 300 feet, while those at Conway are comparable with a depth of 350 feet. In the case of Frodsham the primary coil had a length of 300 feet, while at Conway the length was 1,320 feet. At Loch Ness and between Arran and Kintyre, where the parallel lines varied from 2 to 4 miles, the calculated depth was found to be about 900 feet. The depth of this resultant must, therefore, increase with the distance separating the earth-plates, and this renders it possible to communicate by induction from parallel wires over much longer distances than would otherwise be possible.

Mr. Preece.

In establishing communication by means of induction there are three dispositions of circuit available, viz.: (*a*) single parallel wires to earth at each extremity; (*b*) parallel coils of one or more turns placed vertically; (*c*) coils of one or more turns placed horizontally and in the same plane.

The best practical results are obtained with the first arrangement, more especially if the conformation of the earth admits of the wires being carried to a considerable height above the sea, whilst the earth-plates are at the sea level. By adopting this course the size of the coil is practically enlarged, and even if it be necessary to increase the distance between the parallel wires to effect this object the result is still more beneficial.

Two wires of a definite length were first made up into two coils forming metallic circuits, then uncoiled and joined up as straight lines opposed to each other, with the circuit completed by earth. The inductive effects, and the distance between which they were observable, was very many times greater with the latter than with the former arrangement.

One very interesting fact observed in Loch Ness in 1894 was that there was one particular frequency in the primary circuit that gave a decided maximum effect upon the telephone on the secondary circuit. It was a case of tuning. It was so effective as to secure the transmission of speech across a space of 1½ miles.

Effects of resonance were observed during the experiments in Arran in 1894.* A metallic circuit was formed partly of insulated wire 500 feet above the sea level, and partly of ordinary line wire, the rectangle being 2 miles long and 500 feet high. Wires on neighbouring poles at right angles to the shorter side of the rectangle, although disconnected at both ends, took up the vibrations, and it was possible to read all that was signalled on a telephone placed midway in the disconnected circuit by the "surgings" thus set up.

In March, 1895, the cable connecting Oban with the island of Mull broke down. A gutta-percha-covered wire, 1½ miles long, was laid along the ground from Morven, on the Argyllshire coast,

* "Signalling through Space," B.A., 1894; *loc. cit.*

Mr. Preece. while on Mull the ordinary overhead iron wire connecting Craignure with Aros was used. The mean distance separating the two wires was about 2 miles. There was no difficulty in communicating. Public messages were sent for a week until the cable was repaired.*

Perhaps the most important experiment attempted was that to communicate between England and Ireland. A circuit was made up from Carlisle to Haverfordwest, and another in Ireland from Belfast to Wexford. The whole telegraphic system of the country was stopped from midnight to 2 a.m. one Sunday morning in June, 1895. Attempts were made to signal, but it was impossible to distinguish a signal through the wonderful, incessant, and strange, sounds that filled the telephone and overpowered everything else. No telegraph was at work anywhere. The hum of two or three electric light installations working on the alternate-current system was evident, but there was a weird, strange babel of noises that was mysterious and disappointing. I am strongly of opinion that these sounds were due to disturbances excited by primary electrical effects outside our globe. I have not thought the experiment worth repeating. It can have no practical value, but I had arranged in the event of its success to communicate between England and Europe, and then between Europe and America.

APPENDIX.†

INDUCTION BETWEEN PARALLEL CIRCUITS.

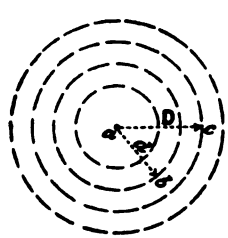


FIG. 1.

If *a* (Fig. 1) be a wire (perpendicular to the plane of the paper) through which a current is flowing, then this wire, acting as a centre, will radiate circles of magnetic force about it, precisely as an insulated wire to which a battery is connected radiates potential through its insulating sheathing. In the case

* "Signalling through Space without Wires," Royal Inst., June, 1897; *The Electrician*, vol. xxxix., p. 216.

† By Mr. H. R. Kempe.

of an insulated wire, as is well known, the electric potential at b is to the electric potential at c as log D is to log d (this follows from the well-known formula—resistance of dielectric between the circles passing through b and $c = k \,.\, \log \frac{D}{d}$, where k is a constant). Precisely in the same way the magnetic potential at b is to the magnetic potential at c as log D is to log d. Mr. Preece.

Let A B (a) (Fig. 2) be an infinitely long wire, and b and c be two wires forming parallel sides of a rectangular circuit, E F G H.

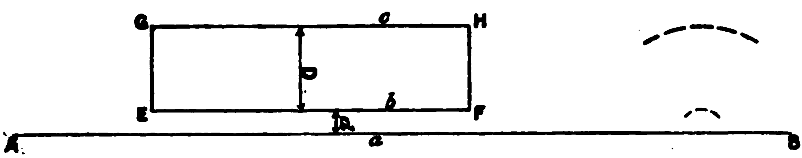


Fig. 2.

Then, if a current be set up in A B, magnetic potentials will be set up at the wires b and c, and the differences of these potentials will cause a momentary current to be set up in the rectangle, so that

$$Q = k \,.\, (\log (D + d) - \log d) = k \,.\, \log \frac{D + d}{d},$$

where Q is the quantity induced, and k a constant.

If l be the length of the sides G H, E F, of the rectangle in centimetres; A the current-strength in amperes in the wire A B; r the total resistance of the rectangle, then

$$Q = \frac{2\,A\,l}{10^3\,r} \,.\, \log_\epsilon \frac{D + d}{d} \text{ micro-coulombs,}$$

$$= 0{\cdot}004606 \frac{A\,l}{r} \log \frac{D + d}{d} \text{ micro-coulombs.}$$

Within certain limits this formula is approximately correct if the wire A B is not infinitely long, but forms one face of a second rectangle similar in form to the induced rectangle E F G H, and set either in the same plane with or at right angles to the first rectangle; but in this case the formula has to be corrected to

Mr. Preece. allow for the influence of the second face of the inducing rectangle, as follows:—

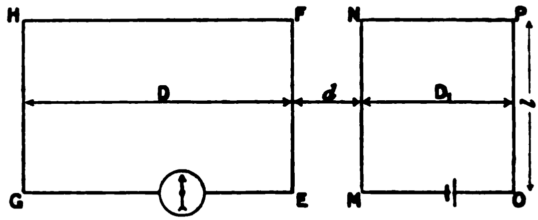


Fig. 3.

In this case the total effect produced by the two faces M N, O P, of the inducing rectangle is

$$Q_1 = k \, . \left\{ \log \frac{D + d}{d} - \log \frac{D_1 + d + D}{D_1 + d} \right\}.$$

If the two rectangles are at right angles to each other, and l is large compared with D_1,

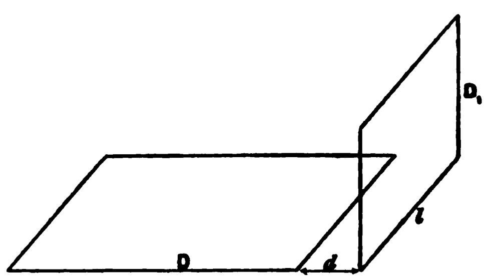


Fig. 4.

then the formula becomes

$$Q_2 = k \left\{ \log \frac{D + d}{d} - \tfrac{1}{2} \log \frac{D_1^2 + (D + d)^2}{D_1^2 + d^2} \right\}.$$

Again, if the two rectangles are parallel and equal, and l is Mr. Preece. large compared with D, then

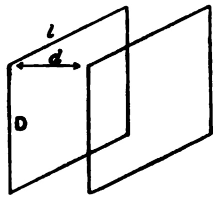


FIG. 5.

$$Q_3 = k \left\{ \log \left(\frac{D^2}{d^2} + 1 \right) \right\}.$$

In this latter formula, if the ratio of D to d is constant, the value of Q remains unaltered. If d is large compared with D, then $\log \left(\frac{D^2}{d^2} + 1 \right)$ varies as $\frac{1}{d^2}$; that is to say, the inductive effect diminishes as the square of the distance.

It must not be omitted to be stated that these formulæ do not take into account the effects on the wires which are at right angles to the primaries; these latter effects oppose the main induced current, so that in practice, under certain conditions, this current diminishes more rapidly with the distance than the foregoing formulæ would indicate. In fact, when the distance between the squares becomes very large, the inductive effect diminishes as the "cube" of the distance; for the squares are practically equivalent to very short magnets, whose effect upon each other, when a long distance apart, as is well known, varies as the cube of the distance.

The decrement of effect as d is increased is small when d is small compared with D, but the rate of decrement gradually increases, passing through the various phases, such as "square root of distance," "directly as distance," "square of the distance," and finally as the "cube of the distance."

Taking D = 100, then the various relative values of Q_3 corresponding to different values of d are as follows (Table A):—

Mr. Preece.

Table A.

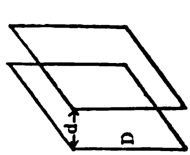
d	1	2	3	4	5	6	7	8	9	10	20	30	40	50	60
Q_3	100	84·9	76·1	69·9	65·1	61·1	57·8	54·9	52·4	50·1	35·8	27·1	21·5	17·5	14·4
d	70	80	90	100	200	300	400	500	600	700	800	900	1,000	1,500	2,000
Q_3	12·1	10·2	8·7	7·5	2·4	1·1	0·66	0·43	0·18	0·15	0·13	0·12	0·11	0·05	0·03

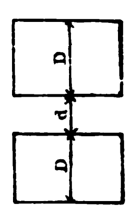
$D = 100.$

Table B.

d	1	2	3	4	5	6	7	8	9	10	20	30	40	50	60
Q_1	100	88·6	72·8	65·8	60·5	56·2	52·6	49·6	46·9	44·6	30·2	20·1	18·2	15·0	12·6
d	70	80	90	100	200	300	400	500	600	700	800	900	1,000	1,500	2,000
Q_1	10·8	9·4	8·3	7·3	3·0	1·6	1·0	0·72	0·53	0·40	0·32	0·26	0·21	0·09	0·05

$D = D_1 = 100.$





If we take case (1), D and D_1 being made equal, then the formula becomes Mr. Preece.

$$Q_1 = k \log \left(\frac{D^2}{d^2 + 2\,D\,d} + 1 \right).$$

In this case also, if d is large compared with D, the inductive effect will diminish as d^2.

Taking $D = 100$, then the various relative values of Q_1 corresponding to different values of d are as given in Table B.

It is interesting to note that a series of experiments made on the Frodsham Marshes bear out the correctness of the theoretical considerations; thus, according to the arrangement shown by Fig. 1, the value of the induced effect in C.G.S. units should be

$$Q_3 = 0{\cdot}004606 \, \frac{A\,l}{r} \, \log \left(\frac{D^2}{d^2} + 1 \right) \text{ micro-coulombs.}$$

In an experiment made the following were the values of the various quantities:—

$l = 300$ feet $= 300 \times 30{\cdot}84 = 9{,}252$ centimetres.
$r = 6{,}800\ \omega$ (galvanometer).
$A = 2{\cdot}5$ amperes.
$D = 20$ feet.
$d = 3$ feet.

Therefore,

$$Q_3 = 0{\cdot}004606 \, \frac{2{\cdot}5 \times 9{,}252}{6{,}800} \, \log \left(\frac{400}{9} + 1 \right) \text{ micro-coulombs.}$$

$$= 0{\cdot}0157 \times 1{\cdot}657 = 0{\cdot}02597.$$

The discharge deflection corresponding to Q_3 was 14 divisions, and the constant of the galvanometer 0·00192; so that the observed value of $Q_3 = 0{\cdot}00192 \times 14 = 0{\cdot}02688$, which closely corresponds with 0·02597, the theoretical value.

In order to verify the correctness of the theory according to arrangement Fig. 3, six experiments were made in which the value of d was varied from 1¼ to 36 inches, the length of D being 1,320 feet, and D_1 being varied from 30 up to 510 feet. The following shows the results:—

Mr. Preece.

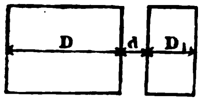


Fig. 3.

D_1 (feet)	510	510	510	510	50	30
d (inches)	1½	3	6	12	36	36
Calculated value of deflection	22·5	20·5	18·6	16·6	8·0	6·7
Observed deflection	22·5	20·0	17·5	15·0	9·0	7·5

D = 1,820 feet.

The agreement between the theoretical and observed values, though not exact, is close enough to prove that the theory is correct; the discrepancies being doubtless due to experimental error, and no allowance having been made for the action of the sides of the rectangles.

In another series of experiments, arranged according to Fig. 5, the following are the results:—

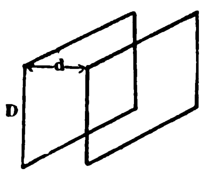


Fig. 5.

D (feet)	20	15	10	5
d (feet)	3	3	3	3
Calculated value of deflection	14·0	12·0	9·2	4·9
Observed deflection	14·0	12·5	9·5	4·5

Here again the agreement between the calculated and observed results is very close.

Besides the foregoing, a number of experiments were also made according to the arrangement shown by Fig. 4, the following being the results:—

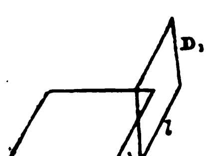


Fig. 4.

D_1 (inches)	240	120	60	240	120
d (inches)	3	3	3	36	36
Calculated value of deflection ...	15·4	13·0	10·6	6·7	4·4
Observed deflection	16·0	13·0	10·5	7·0	5·0

Lastly, a set of experiments were made with the rectangles Mr. Preece placed as shown by Fig. 6.

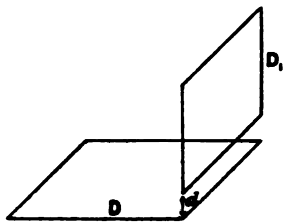


FIG. 6.

In this case the formula is

$$Q_4 = k \,.\, \left\{ \log\left(\frac{D^2}{d^2} + 1\right) - \log\left(\frac{D^2}{(D_1 + d)^2} + 1\right) \right\}$$

The following are the results obtained in a series of eight experiments :—

D_1 (feet)	19	19	9	19	19	4	2	19
d (feet)...	1	1	1	2	4	1	1	9
Calculated value of deflection	9·5	9·5	7·4	7·6	5·8	4·7	2·4	2·5
Observed deflection ...	10·0	9·5	7·5	7·0	5·0	4·5	2·5	2·5

D = 1,320 feet.

Again we have very close agreement between theoretical and observed values.

There can be little doubt, therefore, that the logarithmic law may be taken to be practically proved.

Another most interesting series of experiments were made in order to determine the depth of what may be called the resultant earth, *i.e.*, the imaginary line along which the current flowing between two earth-plates may be assumed to be concentrated.

Mr. Preece.

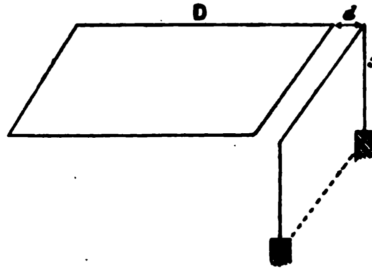


Fig. 7.

If x be the depth of the resultant earth, then, as in case Fig. 2,

$$Q_2 = k\left\{\log\frac{D+d}{d} - \tfrac{1}{2}\log\frac{x^2+(D+d)^2}{x^2+d^2}\right\},$$

$$= k\left\{\log\frac{D+d}{d} - \tfrac{1}{2}\log Z\right\},$$

where $Z = \dfrac{x^2+(D+d)^2}{x^2+d^2}$, or $x = \sqrt{\dfrac{(D+d)^2-d^2 Z}{Z-1}}$.

If we have a second arrangement of rectangles, as follows:—

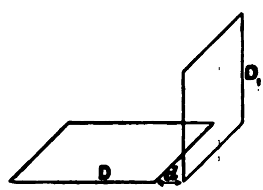


Fig 8.

then

$$Q'_2 = k\left\{\log\frac{D+d}{d} - \tfrac{1}{2}\log\frac{D_1^2+(D+d)^2}{D_1^2+d^2}\right\};$$

so that

$$\frac{Q_2}{Q'_2} = \frac{\log\dfrac{D+d}{d} - \tfrac{1}{2}\log Z}{\log\dfrac{D+d}{d} - \tfrac{1}{2}\log\dfrac{D_1^2+(D+d)^2}{D_1^2+d^2}}.$$

$$\text{or}\log Z = 2\log\frac{D+d}{d} - 2\frac{Q_2}{Q'_2}\left\{\log\frac{D+d}{d} - \tfrac{1}{2}\log\frac{D_1^2+(D+d)^2}{D_1^2+d^2}\right\}$$

The practical application of the foregoing formulæ presents considerable difficulty, owing to the smallness of the induced discharged deflections obtained, even when a considerable primary current is used. An error of half a division in the readings means

a much larger percentage error in the calculated results than the readings error would apparently represent. The calculated results can therefore be only taken as approximate; and, in order to show how large the error may be, the results have been worked out, not only for the recorded deflections, but also for deflections which are half a division above or below the recorded values. Mr. Preece

The following are the calculations:—

$$\frac{D + d}{d} = \frac{1,320 \cdot 25}{0 \cdot 25} = 5,281; \quad D_1 = 5;$$

$$\frac{Q_2}{Q'_2} = \frac{25}{10 \cdot 5};$$

$$\frac{D_1^2 + (D + d)^2}{D_1^2 + d^2} = \frac{5^2 + 1,320 \cdot 25^2}{5^2 + 0 \cdot 25^2};$$

therefore,

$$\log Z = 2 \times 3 \cdot 7227162 - 2 \frac{25}{10 \cdot 5} \{ 3 \cdot 7227162 - 2 \cdot 4211561 \},$$

$$= 7 \cdot 4454324 - 6\ 1979052 = 1 \cdot 2475272;$$

$$= \log \text{ of } 17 \cdot 682;$$

therefore,

$$x = \sqrt{\frac{1,320^2 - 17 \cdot 682 \times 0 \cdot 25^2}{17\ 682 - 1}} = \underline{\underline{323.}}$$

If Q had been 11 instead of 10 5, then

$$x = \underline{\underline{230.}}$$

If Q had been 10 instead of 10·5, then

$$x = \underline{\underline{447.}}$$

Working out the results in a similar manner from other experiments, the following shows the figures obtained:—

$D = 1,320$, $d = 0 \cdot 25$, $Q'_2 = 25$.

D_1.	Deflection (Q_2).			x.		
	Observed.	$+ \frac{1}{2}$	$- \frac{1}{2}$			
5	10·5	11·0	10·0	323	230	447
10	13·0	13·5	12·5	310	236	401
20	16·0	16·5	15·5	239	191	301

Mr. Preece.

$D = 1,320$, $d = 3$, $Q'_s = 15·5$.

(Rectangle horizontal) 3	9·0	9·5	8·5	415	315	574

The great difference which a small change in the deflection makes is quite comprehensible, if we consider that the deflection is compounded of the effect of a primary wire close to a secondary wire, and of a "resultant" primary a long way off from a secondary: this resultant wire might be closer or further away without greatly adding to or subtracting from the effect due to the primary and secondary, which are close together. To determine the value of x with accuracy would necessitate the distance between the primary and secondary faces of the squares approximating to the depth of the resultant earth; but in this case the inductive effect would be comparatively small, and the readings correspondingly small, so that there would be another cause for probable error. The general result of the experiments tends to indicate that the depth of the resultant earth in Frodsham Marsh is about 300 feet.

It may be added that a previous series of experiments made on the Conway Estuary, which at the time were not compared with theoretical values calculated from the logarithmic formula, also fully confirm the correctness of the latter. The following show the comparisons, the depth, x, of the resultant earth being assumed to be 350 yards (Fig. 7):—

Mr. Preece.

1st Experiments.

d (yards)	50	100	200	300	...
Calculated value of deflection	35	22·3	11·4	6·6	...
Observed deflection	35	23·0	11·5	6·5	..

2nd Experiments.

d (yards)	100	200	300	400	800
Calculated value of deflection	16	8·2	4·7	2·9	0·7
Observed deflection	16	8·0	5·25	4·0	2
				Approximate.	

3rd Experiments.

d (yards)	100	200	300	400	800
Calculated value of deflection	17	8·7	5·0	3·1	0·75
Observed deflection	17	8·5	5·5	4·0	1·5
				Approximate.	

D = 440 yards.
Values of *d* approximate only.

Mr. Evershed.

Mr. S. Evershed: A fortnight ago we had, what is, I am sorry to say, the rare, pleasure of listening during the evening to Dr. Lodge's interesting account of his explorations in that sort of fairy-land which he has made his own—the region of the invisible, intangible ether. Dr. Lodge has the happiest manner of getting his own ideas into other people's heads, and I must confess that I find nothing more instructive than an evening spent under those circumstances.

Dr. Lodge has given us in his paper a kind of general sketch of the principles of telegraphy by magnetic induction. To-night

Mr. Evershed. I want you rather to come out of the fairy-land into which he led us, and look at the subject before us from a business point of view.

If wireless telegraphy of any kind ever comes into practical use, it will not be to replace or displace the ordinary wire telegraph, but it will be for use in such cases as are met with fairly frequently where a wire or cable cannot possibly be laid. Therefore it comes down to this: There may be some such places where communication is desirable: how much must people be prepared to pay for it? All electrical engineering problems ultimately come down to questions of cost, and in preparing my paper I endeavoured to deal with the subject from that point of view, rather than attempt any detailed description of apparatus.

In order to arrive at the cost of an induction telegraph we must consider the three items of which every telegraph of the kind will be made up—first of all, the arrangement of the circuits; secondly, the transmitting devices, and the manner in which we are to obtain the primary power, and the amount of power which is feasible under any given conditions; and, thirdly, the receiving devices. I need not tell electrical engineers that the most important thing to know about receiving devices is how much power it takes to work them. There is only a very limited amount of power that can possibly be received in a secondary circuit at a distance, and that amount of power must suffice for working some sort of indicator; that is to say, an indicator must be provided which will respond to the exceedingly minute amount of power available. Now I need not keep you long over the question of the circuits, because this part of the subject is fully dealt with both in Dr. Lodge's paper and in my own paper. I am very glad to see that Dr. Lodge supports the view which I have from the first adopted, namely, that the circuits should be laid in the plane of the earth, with their magnetic axes vertical. The first time I worked out the details of an induction telegraph was in 1892, in connection with communication with light-vessels; and the idea I had was to communicate with all the lightships—and there are numbers of them—round the coast of Kent by means of a single circuit

Mr. Evershed.

enclosing the whole area of the county. I found, as you would naturally expect when you remember that the area of the secondary circuits is limited by the size of the light-vessels, that the cost was prohibitive, unless we could employ an immense amount of primary power. I gave up that scheme, and then devised an arrangement for communicating with the North Sand Head light-vessel by means of a cable on the bottom of the sea. That arrangement, as most of you know, was a total failure, in consequence of the enormous absorption by the necessary armouring on the cable, by the sea, and by the iron of the ship. Of those, the iron of the ship was the most important. I notice that Dr. Lodge rather suggested that the sea would not have much influence, but it did have a very considerable influence. Even at as low a frequency as 16 periods per second the sea caused the absorption of from 30 to 40 per cent. of the induction; but, even supposing the sea had been absent, the iron of the ship was alone sufficient to prevent any signalling at such a frequency as 400 ∿ per second. I was able by means of the vibrating rectangle of my relay to measure the total absorption due to the combined effects of armoured cable, sea water, and iron hull of ship, and it actually amounted to 97 per cent. at 16 ∿ per second.

Now, when we come to telegraphy on land, the matter is rather different, because, so far as we know, the earth at moderate depths is a very poor conductor, and, although there is a great deal of it, the fact that its specific conductivity is very small is in our favour. If it is only low enough, telegraphy can be carried on by means of magnetic induction.

Before we lay circuits, as Dr. Lodge has described, we must endeavour to ascertain what arrangement will give us the best value for our money. Clearly the circuits ought to be circular, but as a rule we shall find that an impossible condition, and we must be content with rectangular circuits, and make them of as large an area as possible. It is also clear that the cost of that circuit will depend upon the weight of copper which you put into it, and the length of the pole line on which it is run. And for that reason you will find in my formula 4^a that I have given

Mr. Evershed. you the maximum power which is available in the receiving circuit, in terms of the frequency, the length of the side of a square circuit (I took a square because you can get something approaching that in practice), and the *power* wasted in the primary.

I notice that Dr. Lodge is hesitating between taking the E.M.F. in the primary circuit, or the primary current, as the essential factor. He is apparently not quite clear which to use in his equations. I met with exactly the same difficulty when I first began working at the subject, and it is only within the last two or three years that I have seen clearly that the cost is pretty sure to depend upon the power. The price of the dynamo does not depend very much on the E.M.F. or current it has to give; it depends upon their product. The price of a battery or dynamo for working an induction telegraph will depend upon the power, and therefore the corresponding factor in my formula is watts. Then we have the volume of copper. I only left in volume because it is a little more convenient for calculating the resistance than if you put in weight. It is quite easy to substitute the weight of copper. Formula 4^a, then, tells us what is the greatest amount of electrical power you can possibly convert into mechanical power to work a receiving instrument when you have adopted every possible means for bringing the current into step with E.M.F., either by means of condensers or other means; and probably the condenser system which Dr. Lodge described to us will prove to be the best for the purpose. It is physically impossible to get more than that power. I have worked out a numerical example in my paper, and, as you see, the amount of mechanical power which is available in the secondary circuit is only 0·34 ergs per second. We are accustomed to dealing with horse-power and watts, but when we have to reckon power in ergs per second it is not easy to realise how small it is, and how exceedingly delicate the receiving instruments must be.

To turn for a moment to the transmitting devices, you can use interrupters (as I believe Mr. Preece has invariably done): but, although they are admirably adapted for giving a readily

audible tone in the telephone, they give trouble from sparking. Mr. Evershed.
Mr. Preece gets rid of sparking to some extent by means of a condenser, but it cannot be entirely eliminated by this means. Another transmitting device is the one which Dr. Lodge has described to us, namely, an alternator fitted with a condenser, in order to bring the current into step with the E.M.F., and so bring "power-factor" up to unity. The use of condensers for that purpose is fairly familiar to those of us who have been working with alternate currents. I do not think I need say any more about transmitting arrangements, because, to my mind, if the difficulty of a suitable indicator can be overcome, there is very little difficulty in providing the proper transmitting apparatus.

Now I come to receivers. Since the function of the receiving instrument is to detect an alternating current, it is probable that some form of vibrator will always be used, like the diaphragm of a telephone, the rectangle which vibrates in my relay, or an apparatus of that kind. The "buzzer" used by the War Office is another instance of a vibratory receiver.

The complete theory of the vibratory relay is given in my paper, and, as you will see by referring to Fig. 2, the rectangle behaves precisely in accordance with theory, the discrepancies being well within the limits of observational errors. In the case of the telephone we are unable at the present moment to measure the amplitude of the diaphragm, and, therefore, unable to measure the mechanical power required to make an audible note. However, it is easy to measure the minimum ampere-turns on the coils necessary to produce an audible note. Mr. Oswald Cox and I made a determination of this kind at Woodfield Works some years ago, and I had the great advantage of having two experts from the Post Office to assist me on that occasion, namely, Mr. Gavey and Mr. Kempe; and we all four made a trial of what we could hear, and, curiously enough, we all agreed. The human ear no doubt varies from time to time, but under the same conditions four average men do not differ very much in what they can hear. I have compared the result which we got with that given by Lord Rayleigh for a similar type of telephone. Lord Rayleigh says that with 1·6 milliampere-turns, alternating

Mr. Evershed. current, in the coil of his telephone there is a readily audible note. He says you can sometimes just hear with about half that amount. Now Mr. Gavey, Mr. Kemp, Mr. Cox, and myself were able to hear 1·4 milliampere-turns; or, rather, Mr. Gavey thought that was about the least amount which was necessary for signalling. But we could all hear a faint note when the ampere-turns were reduced to about 0·7 milliampere turns; so we do not differ appreciably from Lord Rayleigh on that point. At least 1·4 to 1·6 milliampere-turns may be taken to be necessary in order to produce easily audible signals in a Gower-Bell telephone. The Gower-Bell pattern, however, is very imperfect when considered as a means for converting electrical into mechanical energy. As a motor the Gower-Bell is not good. It is like some of the early alternating-current dynamos and motors of the "inductor" type.

At the time the tests I have just described took place, Mr Cox devised a species of "moving-coil" telephone, which has since been re-invented and developed by Dr. Lodge. The idea rose very naturally in Mr. Cox's mind from working with the relay. Seeing the rectangle moving in a magnetic field, it naturally occurred to him that he might attach the coil of the telephone to the diaphragm and allow it to move in a strong magnetic field, and we devised one or two forms of that type. I feel sure the "moving-coil" telephone will ultimately displace the telephones now used, not only for motor work—that is to say, for induction telegraphy—but also for speech. It is clear, from what we all heard of Dr. Lodge's telephones, that they are at least equal to the ordinary patterns as speech instruments, and electrically they are infinitely superior.

But I do not lay very much stress on matters relating to the design of telephones, because I cannot help thinking that we shall have great trouble in working at anything like a frequency of 400 per second over great distances, in consequence of the absorption of induction waves by the earth. I look more towards a system working at very much lower frequency—something below 50 periods, for example. The only low-frequency indicator I have been able to devise is my relay for calling attention, and

Mr. Evershed.

at present it is only available for that purpose—for ringing a bell to call attention. But I cannot help thinking that something on those lines will ultimately be made and adapted to give Morse signals, and will enable us to signal to great distances without any fear of absorption. The two "call" relays which were made for the North Sand Head lightship experiments were arranged to work at 16 periods a second. There is no particular virtue in 16 periods a second, but it happened to be a convenient value. A rectangular wire of a convenient length and material had that frequency, and, it being a very suitable frequency for the purpose, we stuck to it. The equations given in my paper enable one to calculate exactly how much power is required to maintain the vibrations of the rectangle at any amplitude, and I think I have included every electrical and mechanical detail which is necessary in order to clearly understand what a relay of this type will do.

As Dr. Lodge mentions in his paper, those two relays have now been in use for some months at Lavernock and Flat Holm, on Mr. Preece's wireless telegraph there, and I was pleased to hear from Mr. Preece at the last meeting that they are working satisfactorily. Dr. Lodge was kind enough to say in his paper that he admired my system of call, but he then proceeds to say that I have obtained the result at the expenditure of a good deal of power. Well, of course, "a good deal of power" is a little indefinite. I have looked through Dr. Lodge's paper in order to find out what he would consider a moderate amount of power, and I find in his own transmission between the College at Liverpool and his house he is now using, so far as I can make out, from 500 to 900 watts in the primary circuit; so I take it he does not consider that a very considerable expenditure of power. Now on the Lavernock–Flat Holm telegraph we are only using about 120 watts for the call—much less, in fact, than Dr. Lodge uses himself.

Professor AYRTON: Did you say 120 watts?

Mr. S. EVERSHED: Yes, 120 watts. The current is 8 amperes, and the resistance of the circuit is less than 2 ohms. Through Mr. Preece's kindness I am able to show you one of these relays at work. I have only been able to rig up an apparatus which will just show you the manner in which it works. I cannot, of

Mr. Evershed. course, show you its actual operation by means of an alternator three miles away.

[One of the relays in use at Lavernock was placed on the table and connected to a secondary coil. Three means by which the two rectangles could be brought into contact and a bell rung were shown to the meeting. In the first, the continuous current produced by slowly moving a powerful magnet across the secondary coil was of sufficient strength to force the rectangles together without vibration. In the second, a feebly magnetised piece of clock spring was set in vibration close to the coil. The spring having been tuned to vibrate in exact synchronism with the rectangles of the relay, it was shown that an almost invisible vibration of the spring generated an alternate E.M.F. in the coil, and the current flowing through the rectangles set them vibrating and rapidly brought them to a sufficient amplitude to close the bell circuit. The third method was analagous to that in actual use at Lavernock. Exact synchronism was obtained by allowing a small magnet set spinning near the coil to gradually come to rest; thus generating a current of gradually diminishing frequency. The little magnet was mounted on an axle fitted with a small fly-wheel to ensure a sufficiently gradual reduction of speed. It was shown that the relay responded (and rang the bell) when the speed had fallen to that corresponding to the frequency of the relay.]

Dr. Lodge describes in his paper and showed us upon the screen several examples of "call" apparatus based on the same principle as my relay, but he uses tuning-forks as the receiving vibrators. I began with tuning-forks, but very soon gave them up, on account of the large amount of power they need to work them. I have since found that Lord Rayleigh measured the power taken by a small tuning-fork when it was giving a just audible note. He found it required no less than 42 ergs per second to maintain its vibrations. Now the rectangles in the relay which I have shown to-night reach a visible amplitude when the power is only one-thousandth of an erg per second. That shows the enormous difference in the molecular, air, and other frictional resistances opposing the motion of the fork and the vibrating rectangle.

Mr. Evershed.

I should like just here to make a digression, because it so happens that I can give you a little information which will clear up a doubtful point. There have been many questions raised as to whether in Mr. Preece's system of telegraphy the effect is due to induction or due to leakage. Well, it must have occurred to everyone, Why doesn't someone measure the leakage, and so settle the matter once for all?

The truth appears to be that it is by no means easy to measure the leakage when an alternate current or an interrupted current is used. In the spring of this year I went down to Lavernock to assist in setting the call apparatus to work there, and Mr. Gavey and I spent three or four happy days there doing nothing but experimenting. Among other things, we measured the leakage in the secondary when a continuous current was flowing in the primary. We found that when 1 ampere was flowing into the sea through the earth-plates at Flat Holm, we got a potential difference on the terminal earth-plates at Lavernock of 0·4 of a millivolt—quite enough to make the telephone, in use there for signalling, howl. So much for the leakage with a continuous current. Now by means of the relay I was able to measure the leakage with an alternate current at 16 periods a second. It ought, you would think, to have borne the same proportion to the primary current that had been noted with continuous currents. Not at all! It was ever so much less, only amounting to from one-sixth to one-seventh the amount of leakage with continuous currents. That appears to be due to the fact that when we were using the alternator at Flat Holm an alternating potential difference was set up between the Lavernock earth-plates, and the electrolytic effects on the plates being incapable of complete reversal at even so low a frequency as 16 periods per second, prevented the current in the Lavernock line from rising to the value reached with a continuous current. But if the leakage at 16 periods per second is only one-sixth or one-seventh of the value observed with a continuous current, how much less must it be with a frequency of 400 periods per second! It appears to me quite possible that at 400 periods a second the leakage is reduced to a very insignificant amount, and that the

Mr. Evershed. greater part of the current received in the telephone for Morse code signalling at such a high frequency as 400 periods is really due to magnetic induction. The matter should be cleared up by further experiments, and no doubt experiments on the electrolytic effects of earth-plates can be carried out in a laboratory.

The final matter I have to speak about is absorption. Dr. Lodge takes rather an airy view of absorption ; he does not think it will be serious. Well, perhaps the wish is father to the thought. I, on the contrary, having gone through a bitter experience, and having had a beautiful scheme to which I had devoted an immense amount of time rendered completely useless by reason of the enormous absorption, take a very different view of the matter. There is no doubt in my mind that absorption of the waves by the conduction of the earth will play an important part in diminishing the effect at a distance in circuits separated by many miles. It appears to me that the absorption should be measured, and I do not see how we can prophesy or design any induction telegraph whatever until that point has been definitely cleared up. It is no use saying, "Here is an indicator which will work "with only one-thousandth of an erg per second," or whatever it may be. I do not know how much Dr. Lodge's magnifying telephones take, and I should like him to tell us how many ergs per second must be converted into mechanical work in the first of his series of telephones in order to make an audible signal in the final telephone of the series; or, if he cannot tell us the power, tell us the current, or the volts, or something—something to give us a clue to the amount of current or power which is necessary.

But even when we have that information we cannot prophesy what Dr. Lodge's telephones will do on any given circuits, because it may be that with the relatively high frequency he uses the whole of the energy leaving the primary circuit will be absorbed before it reaches the secondary. Therefore it appears to me we really must, in the first place, measure that absorption effect once for all and get to the bottom of it. I am preparing plans for doing it myself. It is rather a difficult job; it is a matter which cannot possibly be undertaken without the co-operation of the Post Office, and that co-operation, I need hardly say, has

been very cordially offered to me by Mr. Preece. With his aid I hope to clear up the existing uncertainty as to absorption by the earth. But it must be cleared up decisively; it must be a definite measurement of absorption. Qualitative results in a question of this kind are absolutely of no use whatever; we must have a quantitive result. We must know exactly what the absorption is, and how it varies with distance, with frequency, and with different underlying geological formations. When that uncertainty has been removed, and we know how much to allow for absorption, we shall be able to predetermine the best form of apparatus for signalling by magnetic induction, with the same confidence that we now feel in designing a plant for power transmission. Mr. Evershed.

Mr. W. H. PREECE: I thought, Sir, that I would save the time of the meeting by putting on record the work that we have done in the Post Office in establishing the so-called "wireless "telegraphy." A paper has been distributed, I hope amongst you all, in which you will find on this particular branch of the subject a reference made to what we have done during the past 16 years. Now I want you to observe that there is one very great difference between the work that we have been doing and the work that has been brought before you by Professor Oliver Lodge and by Mr. Evershed. The work that we have done during the past 16 years has been purely and absolutely experimental. We have started from a very small beginning, and we have pushed on steadily annually. I think, if a record of the work, and of the innumerable experiments made from 1884 to 1892, were in print and were shown to you, you would be simply astonished that so much work has been voluntarily, unrewardedly, and cheerfully done. To save the time of the meeting, I thought that I would illustrate the principle and explain the diagrams as rapidly as I could. The system was not patented, as it might have been. On the diagram on the left-hand side the connections are shown. I may say at once that that apparatus is extremely simple. I have read of the apparatus in use at Lavernock being spoken of as cumbrous. There is no doubt in the earlier experiment in 1892 it was cumbrous; but that term applies no Mr. Preece.

Mr. Preece. longer, for we have replaced the very heavy alternator by 50 ordinary common dry cells. A little motor is shown at A. The motor and the interruptor are on the same spindle. When I put on the switch the current goes through the motor; the motor has interrupted the current at a frequency of 400. *Here* I have a simple Morse key, an ordinary telegraphic key used when a message is transmitted. On the table I have a primary coil, and through *that* coil currents are sent when I depress *this* key. *Here* I have a telephone fixed with a trumpet-shaped mouth, so that I hope the sounds will be heard over the room. Now, when I bring *this* secondary coil over the primary one, the currents induced in the secondary coil as I come down increase. As I take the coil away you will observe how rapidly the effect diminishes. I can make that sound disappear by simply turning the one coil so as to place it at right angles to the other. That simply illustrates the principle. Now *here* we have a rectangle forming the primary coil, and *here* is a movable coil of 50 turns. I brought it close up to the table so as to make use of the same secondary coil. At the other end of the room there is another rectangle. These two coils represent Lavernock and Flat Holm, and the sounds that you will hear by the telephones may be taken as an indication of the sounds heard across these 3½ miles separating those two places, although probably they are twice as strong. Now we will send the signals through that coil, and see if it is possible to make them audible. If anyone at the other end of the room will only listen to one of those telephones, they will tell me whether they hear signals or not.

[*Several members listened at the telephones, and stated that the signals were distinctly heard.*]

They are perfectly audible at that end of the room. Well, here you have an illustration of a simple telegraphic apparatus that has been devised by sheer plodding experiment. It has taken a good many years to reach this present condition. It has been made a real practical telegraph by the calling apparatus that Mr. Evershed has explained to you to-night. It is as practical and as simple and cheap and as effective as any form of

telegraph apparatus that exists. If it is in practical operation only between Lavernock and Flat Holm, it is because nobody has asked for it anywhere else. Professor Lodge, in his paper, mentioned that Mr. Stevenson, in Scotland, had proposed to use this system in the extreme north of the Shetlands, at a place called Muckle Flugga; but, as a matter of fact, no experiment was ever tried there. The experiment that was referred to was an experiment tried near Edinburgh, in a place called Mayfield. The experiment itself was not an experiment of Mr. Stevenson's, but one which was carried out under my instructions by my assistants in Edinburgh, to prove to Mr. Stevenson that his views were wrong. I am not quite sure that they succeeded in doing that. You cannot convince a man against his will. But, at any rate, no connection has yet been made between the Shetlands and Muckle Flugga. I have no more to add, except this—that I do not propose to discuss Professor Oliver Lodge's paper; it contains nothing really new except the call. I said what I wished to say about it the other night. I appreciate it very much, and I think he has brought to bear upon the subject his well-known style, with which we are all so well acquainted. But I venture to say to him that there are a good many statements which require a little modification. I will not point them out to him now, but I am quite sure when I do so by letter there is nobody who will be more ready to correct them than he. Mr. Preece.

Professor J. A. FLEMING: The two communications which have been brought before us by Dr. Lodge and Mr. Evershed raise many interesting points for discussion. Dr. Lodge opens his paper with a rough classification of methods of space telegraphy. The time has hardly yet arrived for an accurate classification of these methods. If we leave out of account some methods in an embryonic condition, such as those of Zickler—who is said to employ ultra-violet light—and neglect those methods which depend upon conduction through soil or water—which are certainly not space telegraphy at all—we may say that at the present time we are undoubtedly in the possession at least of two practical methods for communication through space depending on electrical means. The first, which is our chief subject of discussion now, Prof. Fleming.

Prof. Fleming. may be described, perhaps, as a closed-coil system, and is that devised by Mr. Preece and elaborated by Dr. Lodge. This method consists virtually in the construction of a gigantic air-core alternating-current transformer of which the coils are separated from one another by a distance, perhaps, of many miles. Dr. Lodge ingeniously takes advantage of the principle of resonance to exalt the effect in the secondary circuit. We have for a long time been familiar with a similar effect in the case of large alternating-current transformers used with a condenser in the secondary circuit. We have often discussed here the effects produced when a long concentric cable having a certain electrostatic capacity is switched on to the secondary circuit of a transformer and thereby exalts the electro-motive force in that circuit. In this closed-circuit system of space telegraphy the energy associated with the primary circuit never leaves it in the form of a true wave. We merely use the secondary circuit to detect and measure at a distance the time variation of the magnetic force due to the primary circuit. The other method, which may be called the open-circuit method, to which Dr. Lodge also briefly alludes, has been made known to us chiefly by the interesting experiments of Signor Marconi. This latter experimentalist uses a primary wire, open or circuit, consisting of a straight vertical rod, in which an alternating current is set up by means of an induction coil and spark balls. At a distance he places a similar secondary circuit in which there is a Branly detector, also the two circuits are connected together at one end through the earth. Dr. Lodge suggests that a part of the effect in the secondary circuit in this case may be due to electric jerks, as he says, taking place through the soil, and that "the "coherer in the receiving circuit is a detector of electric jerks "transmitted by the earth or by uninsulated conductors such as "gas pipes." I am not sure that I understand exactly what is meant by an electric jerk. I have tried a good many experiments with Branly detectors, consisting of tubes or boxes partly filled with metallic filings, and it is convenient to call these arrangements discontinuous conductors. A Branly detector of this class is a detector of electro-motive force in the circuit in which it is

placed, and when that electro-motive force reaches a certain critical value the discontinuous material passes suddenly from a condition of almost perfect non-conductivity to a condition of very high metallic conductivity. If a detector of this kind is put in series with a long wire, and if electric force is created in that region, either by an electro-magnetic wave passing through it, or any other way, then the view I take of the matter is that the wire integrates the electric force parallel to itself into electro-motive force. If the wire has a sufficient length, then, since the electro-motive force is the line integral along it of the electric force, a value of electro-motive force in the circuit may be reached which breaks down the non-conductivity of the coherer. Therefore, if the electrical force is small, but if the wire or rod in circuit with the detector is made sufficiently long, we may be able to make evident the presence of the electric force in the region by its action on the discontinuous conductor or Branly tube. Hence, in the case of the open-circuit system, what we virtually do is to integrate the electric force into electro-motive force and detect this last. In the case of the closed-circuit method, what we do is to integrate by the secondary circuit the time variation of the magnetic force due to the primary circuit, and estimate that as electro-motive force by its action on a suitable receiver. Prof. Fleming.

It is essential in both those systems that there should be a certain relative position between the receiving and transmitting circuits. In the case of the open-coil system, it is obviously necessary that this secondary circuit should be parallel to the primary circuit, and unless that is the case no signalling can be carried on over any great distance. It can also be shown that in the open-circuit system a similar condition holds good.

I have before me a box containing a battery, bell, relay, and Branly tube detector, connected up as usual; and on the other side of the room we have an induction coil and a pair of parallel rods, with spark gap, to transmit waves across the room. The Branly detector is inserted in the gap between two other long rods. At present the rods of receiver and transmitter are approximately parallel to one another. The experiment I want to show you is that to cause the bell to ring it is necessary that the rods

Prof. Fleming.

of the receiver apparatus, or secondary circuit, must be parallel to the rods of the transmitter apparatus, or primary circuit. If we turn the receiver rods so as to be at right angles to the transmitter rods, the effect on the receiver is practically nothing, or, at least, is very much diminished. [*Experiment shown.*]

This experiment shows, I think, the necessity for the parallelism of the primary and secondary rods. In the early days of Marconi's experiments, we heard a good deal about tuning these "wings," as the appended rods or wires were often called, which are necessarily connected to the detector; and it was suggested that in this case also a true resonance effect existed. That, however, I believe, is not the case; the true function of the long rods or wires, as used in Marconi's system, is not explained by simply asserting the effect to be due to resonance.

Then, as to the functions of the earth in the open-circuit system of space telegraphy, we are in the presence of some interesting questions. Dr. Lodge seems to suggest conduction through the earth. I think that there is room for much more experimental work before we can speak on that subject with perfect confidence. Mr. Evershed strikes a truer note when he says that much room yet exists for inquiry. I tried some experiments in the last summer of the following kind:—If we connect the transmitter rods of the induction coil with the rods of the receiver apparatus by a wire which may be continuous or may be broken into lengths, when that wire is laid upon the ground, however much it may be earthed, we find that the wave from each spark at the transmitter travels along the wire and affects the detector. It naturally occurred to me to try these experiments with the wire immersed in water. The apparatus was therefore set up by the side of a lake, and a wire laid in the water one-eighth of a mile long; this wire, as before, connected the receiver and transmitter rods. On making sparks at the transmitter, we obtained, however, no effect at the receiver. It has been suggested several times, I believe, that an arrangement of this kind might be utilised for signalling through bare wires laid in water, even through the wire covering of submarine cables, using a Branly detector as relay. This suggestion, I find, has

even found its way into patent specifications; but I am afraid it was put there by enthusiastic patentees, who rushed to the Patent Office first, and the laboratory, if at all, afterwards; because it certainly will not work. There is no possibility of transmitting signals along wires laid in that way through water, because of the exceedingly large absorption of the wave-energy that appears to take place in water. The above experiment confirms, therefore, the experience of others that both the conductivity and the power absorption of ether-wave-energy by water is too great to allow Hertz waves to be transmitted any great distance through it. Prof. Fleming.

There are many other points in connection with the ether-wave system of wireless telegraphy which it would be of great interest to discuss; but, as the subject before us is more particularly at the present time the magnetic induction telegraphy, I will not extend these remarks, but simply conclude by congratulating Dr. Lodge on the success he has achieved in advancing those methods of space telegraphy which the newspaper articles persistently call "wireless telegraphy," though in reality only telegraphy with less wire than we are generally accustomed to use.

General C. E. WEBBER: At this hour of the evening, and with so many wishing to speak, it has occurred to me to remind the Institution of one fact only in the past which I think is deserving of interest. It affects the memory of one of our Past-Presidents (Mr. Preece has spoken of the history of this question); and I should like to remind the meeting of the fact that on the 23rd March, 1882, when Professor Dolbear read his interesting paper in this Institution on what he called the "New Telephone," Mr. Willoughby Smith spoke these words: "I was prepared to "have shown that by placing a flat spiral of fine silk-covered wire "in the centre of this room, the spiral being connected to a suitable "transmitter and battery, that every person present possessing a "telephone would have been able to have heard whatever sound "was influencing the transmitter, although the telephones were "not in any way attached to the circuit." I believe that was the date from which so many of us started (I, in my own small way) General Webber.

General Webber

to investigate the interesting subject which has been brought before you this evening.

Mr. Whitehead.

Mr. C. S. WHITEHEAD: The question has been raised by Dr. Lodge whether certain equations with which I opened a paper bearing on this subject do apply to frequencies so low as 300. As the equations have some importance in connection with this question, I am very glad of the opportunity of a short reply. I understood from Dr. Lodge that it was not to the mode of proof he objected, but to the very first equation with which I opened the paper being applied to the subject of induction telegraphy when the frequency was so low. What I am going to say refers only to this point. It was not so stated in the paper that I wrote, but, as a matter of fact, the equations with which I began were, with a certain assumption, to which I shall refer presently, strictly derived from Maxwell's equations. Hence Dr. Lodge's objection must refer either to the assumption or to Maxwell's equations. I will begin with the assumption. Maxwell, as is well known, supposed that the total current in a conductor is made up of the conduction current and the polarisation current. The assumption I made was that the polarisation current might be neglected. That is a question which is very easily calculated, and the result is, provided the frequency is not comparable with that of light, no serious error can possibly be introduced. The frequency I was considering was only about 300, as I have said. Hence I am confident that no error of any importance can arise on this ground. The next point is, Do Maxwell's equations apply? This is a matter entirely of opinion. I may perhaps be allowed to cite one or two authorities which bear me out in thinking that they do apply. Professor J. J. Thomson, for example, in his book, "Recent Researches," has this sentence: "We shall begin "by writing down the general equations which we shall require "in discussing the transmission of electrical disturbances through "a field in which both insulators and conductors are present." And again, in his "Elements of Electricity and Magnetism:" "We owe to Maxwell a theory, now in its main features "universally accepted, by which we are able to completely "determine the electrical conditions not merely in the conductors,

"but in every part of the field." Dr. Hertz also, in his book, "Electric Waves," in the English translation, says: "Maxwell's "theory is Maxwell's system of equation." Mr. Whitehead.

Professor J. J. Thomson, Mr. Oliver Heaviside, and Dr. Hertz, in many papers on various electrical questions, all start by using Maxwell's equations, without one hint, as far as I remember, that they are not everywhere applicable. Professor J. J. Thomson, after the two sentences I have quoted, starts with Maxwell's equations; and not another sentence is there, or one word of warning, to the effect that these equations are not really true, not merely for this or that space, or for this or that frequency, but they are the general equations we must use in all parts of the field. So far as I can see, if my equations have to go, Maxwell's equations must go with them. It is not the hour, nor am I the man to defend Maxwell's theory; but this, perhaps, I am permitted to say: If Maxwell's equations are not true, where are the equations we must use instead of them?

Sir HENRY MANCE: I should like to make one suggestion of a practical nature, which, although simple, may be of service to those who are engaged in working out the problem of wireless telegraphy. As I understand, the method proposed to indicate the signals is to cause the distant receiving apparatus to respond to the intermittent transmission of magnetic waves for the long or short periods required by the Morse alphabet; this being so, it will be obvious that, as in all telegraphic apparatus, there is a certain amount of inertia to be overcome, and I think that greater sensitiveness would be obtained if the receiver is caused to give out a steady continuous note, in which the slightest alteration would easily be detected by any telegraphist. Better to explain my meaning, I might mention that many years ago I had to deal with a land line on which communications regularly failed every night, until oil insulators were introduced. It was found that long after signals had ceased by ordinary Morse, and when no current whatever could be detected on the galvanometer, there was still sufficient arriving to affect the humming tone set up by a make-and-break electro-magnet belonging to an electric bell which happened to be in the local circuit, but from which Sir Henry Mance.

Sir Henry Mance. the bell portion had been removed, thus allowing the spring to vibrate freely when the relay circuit was closed, which it happened to be at that time.

I suggest that, instead of endeavouring to produce an intermittent action on the receiver, which in its normal position is at rest, it would be better to have the receiver in constant action—that is to say, vibrating at a high rate, so as to produce a certain note—and then make the signals by acting on the receiver in some way so as slightly to alter the tone emitted.

Mr. Sennett. Mr. A. R. SENNETT: There certainly can be no doubt as to the amount of interest Dr. Lodge's paper has aroused in the minds of all of us who have heard it. We do not know what the future may have in store for induction telegraphy, but one thing we know—that a necessity exists for connecting our lightships with the mainland, either by its means or otherwise. Dr. Lodge has referred to this; and it seems that Mr. Evershed was led to study the question of wireless telegraphy, in a measure, from considering the necessities in this relation. I was therefore very much disappointed to hear him throw a very considerable damper upon that on the score of expense. But, Sir, if there is anything that the English people have reason to be proud of, and are proud of, it is their maritime supremacy; therefore I think we ought not to be daunted by electrical troubles, and certainly not deterred in our efforts by financial difficulties: we ought to go ahead in our experiments until the problem has been solved, and the matter has become a *fait accompli*.

I was very much impressed and surprised by the figure that Dr. Lodge gave us as to the absorption taking place in sea water. Mr. Evershed said he understood Dr. Lodge to say that this was only small. I understood him to say it was no less than 70 or 75 per cent. This being so, we must direct our efforts accordingly. I remember, some years ago, Mr. Wimshurst explaining to me his system of connecting lightships. I thought it very ingenious and practical, and it would certainly overcome the whole of the difficulties arising from absorption. The principal difficulty hitherto experienced has been that of cable abrasion. I will sketch this simple device. What Mr. Wimshurst suggested was

simply to put an induction shackle, or swivel, into the cable; this consisted of two disc-shaped chambers. D represents a disc. In that disc is a coil of wire which can be efficiently and entirely insulated—wholly embedded, in fact, in some dielectric, as, for example, a suitable resinous body. E is the other chamber, or half, of the swivel, embedded in which is a corresponding coil—primary or secondary coil, whichever you like to consider it. *Here* is the chain going down. I cannot myself imagine why that should not act, and I certainly think the Trinity House authorities should try such a simple contrivance. Mr. Sennett.

I cannot help thinking that this invention, in conjunction with suitable and sensitive instruments, should solve this problem of communication with lightships, progress in regard to which has not, I venture to suggest, up to the present reflected greatly upon the inventive ability of our country.

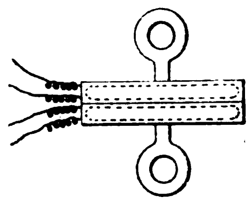


FIG. A.

I was very much interested in Dr. Lodge's paper, because his system essentially turns upon isochronism; and I was particularly impressed because he made the almost startling statement that the sensibility of his system was increased a *million-fold* by the proper tuning of the condensers. I happened to be present at the reading of the paper by Mr. Stevenson in Edinburgh in 1892, to which Professor Lodge has referred. Mr. Stevenson certainly described to us what he actually did—the wires and circuits he used, and everything else in his experiments at the Isle of Skye. I cannot quite assimilate that with Mr. Preece's remarks.

Mr. PREECE: It was near Edinburgh—Murrayfield.

Mr. SENNETT: I ventured to impress upon Section G of the British Association at this meeting my views as to the great advantage to be derived from isochronism, and I said that with regard to the connection of lightships it seemed to me that it was not necessary to employ electricity at all for the actual transmission. Having regard to the enhanced velocity of sound in water, its constancy, and the homogeneity of the medium, I advocated a system consisting essentially of the transmission of mechanical

Mr. Bennett. vibration directly through the water, in conjunction with delicate and sympathetic receiving apparatus, so devised that it could be tuned in accurate unison with the periodicity of the mechanical vibrations thus transmitted.

Such experiments are rather expensive for outsiders to make, but I did what I could in this direction, and was very pleased with the results, and even more strongly impressed with the great value of synchronism and accurate tuning.

I will sketch the instruments I made and used. No electrical impulses passed through the water at all, although electricity was used in the receivers. Mechanical, instead of electrical, waves were used; these may be produced, for example, by means of an electrically driven siren emitting a certain note in the water. A bell, indeed, may be used, for it is simply surprising how far one can hear even slight noises produced under water; and I think the day may come when we shall be able to communicate in this manner from ship to ship in motion at great distances; and I think that such transmission will prove so efficient that, apart from its utility in increasing the sensitiveness of the system, the tuning will be found a necessity for isolating the particular and predetermined note employed.

For the receiver I took a membranous diaphragm similar to those used by Professor Thompson in his telephone. This I put in the mouth of a Helmholtz resonator. On the back of this diaphragm I put a Hughes microphone. This microphone had its battery in the ordinary way, but included in the circuit was an induction coil. The secondary coil of this induction coil was connected to a telephone, and this, again, was mounted in front of a diaphragm similar to the first. I found that it was advisable to put the microphone diaphragm at an angle of about 60°, for this had a kind of damping action on the jumper, as we may call it. My diaphragms were not fitted as in ordinary telephones, but they were mounted more after the fashion of an ordinary kettle-drum. D D represents the diaphragm in the microphone, R the resonator, B the local battery, C the induction coil. The vibratory period of the resonance cavity was known, and could be adjusted to the greatest possible nicety. The fundamental note

of this tympanum—as I prefer to call it—was tuned to the resonance of *this* chamber. Mr. Sennett.

It seemed to me to be very essential that a relay, or relays, should be made use of; indeed, seeing that no current or electrical influence was sent through the water at all, the receiver itself—or, rather, that portion of it working in conjunction with its own battery—may be looked upon as a relay.

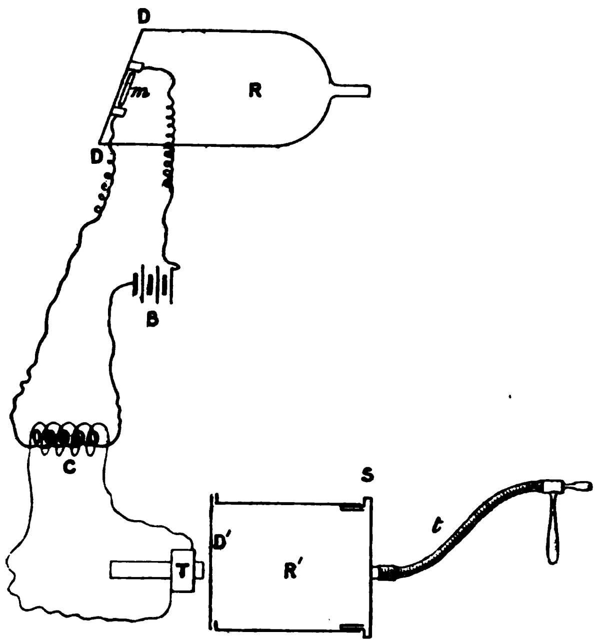


Fig. B.

The receiver proper was very similar. The tympanum, D′, was very accurately tuned to the chamber R′, and was made to form its base instead of covering its mouth. For this chamber I employed an ordinary Helmholtz resonator, which is provided with a slide (at *s*). By pushing this slide in or out the combination could be tuned to the greatest possible sensitivity, with the result that a feeble current of isochronous periodicity circulating in T was made to produce an appreciable sound in the tube *t*. I used a

Mr Sennett. conveniently shaped ear-piece of glass, and I consider that for shipboard use the receivers should be duplicated, and such an ear-piece used in each ear. I feel convinced that, if one were to put on a lightship an arrangement of this kind, and transmit from the nearest harbour—it might be 20 or 30 miles away—the vibrations from a big bell or a powerful siren, one would hear distinctly, and be able to signal by the Morse code. Such a system would lend itself to use between ships in motion; and possibly, by fitting the diaphragm, or an acoustic lens, in a revolving frame, one could, in a fog, by revolving the frame over a divided quadrant, find out the exact direction from which this sound emanated—a matter of great importance.

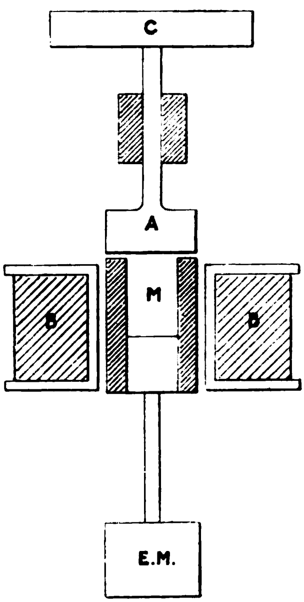


Fig. C.

In common with everybody else—as I should imagine, but speaking for myself—in making these experiments I got very tired and worried over the employment of tuning-forks, and the system with which I am now experimenting is simply this: I do away with all vibrating parts, and use instead a revolving commutator divided up into the requisite number of contact pieces. This is mounted on a vertical spindle, and the bottom of the spindle carries a round disc of soft iron, which forms the armature of an annular electro-magnet, M. This magnet is mounted in its turn on a spindle, and is driven at an approximately uniform rate, but at a speed, say, 50 per cent. faster than it is desired that the commutator shall revolve. The commutator is held back by one of Professor Hughes's very ingenious governors. On account of the armature travelling more quickly than the governor, its brake is always applied to a certain amount, and the instant the commutator drum has a tendency to accelerate in the least the

brake goes on tighter, and an uniform velocity is thus obtained, and at the same time all the difficulties attendant upon tuning-forks, reeds, vibrators, mercury cups, and other paraphernalia are obviated. Mr. Sennett.

There are other matters I should like to have said a word upon, but at this late hour I will only just refer to one. Professor Lodge touched upon, and demonstrated to us with his remarkably loud-speaking relay micro-telephones, the characteristic sound which it is well known that every telephone possesses, and which detracts so much from the efficiency of the instruments. I think we might appropriately call this the telephonic "nose-timbre." If we want to know why this sound exists, and want to be put in the way of eliminating it, we have only to consider the construc- of the tympanum of the human ear.

I should like here to point out the difference in the way such instruments should be treated, according to whether it is desired to aggrandise a sound—say, for example, the fundamental note of a musical tone—or to obliterate other and confusing sounds,—in other words, whether it is wished that the instrument should have a powerful and sympathetic fundamental note of its own, or be capable of receiving with almost equal facility a number of sounds of varying pitch.

Our ears are specially constructed for the latter purpose. First of all, in the human ear the diaphragm is oval, not circular; secondly, it is not a "free" diaphragm, for there is glued, as it were, to the back of the tympanum a bone. What is the effect of this? It is to prevent the diaphragm having a pronounced intrinsic fundamental note, for it cannot possess more than two radii of the same length, instead of its being equi-radial as is an ordinary telephone diaphragm. The consequence is, it practically has no fundamental note. Conversely, if the ear could speak, it would naturally have no note of its own—it would possess no characteristic *klangfarbe*.

On the other hand, when you desire to reinforce a given tone, you must deviate from Nature's receiver, and call to your aid all the effects of resonance you possibly can; consequently, I place my diaphragm actually in a resonance cavity, and find its sensibility to be thereby enormously increased.

Mr. Sennett. Therefore I consider I have found that by adopting something of the construction of the human ear one is able to take away very largely from the characteristic sound with which we are familiar in telephones and phonographs.

In conclusion, it would be redundant in me to eulogise Mr. Preece for the amount of energy he has put into the subject. May I ask him to extend this hospitality in labour—I sincerely trust he will—to the system Dr. Lodge has developed? and I am inclined to think that, if Dr. Lodge's system were employed, and the utmost attention given to the tuning of the receiving instruments, such as I have described, whatever kind of prophecy we might indulge in as to the future of induction telegraphy, we should be excused any reasonable degree of rashness. I have for a number of years been convinced of the necessity of efficient tuning and the enormous value of isochronous working, and the great sensibility of Mr. Evershed's reed-call is but an exemplification of this.

Prof. Ayrton. Professor W. E. AYRTON: The only point I wish to refer to at the moment is in connection with the remarks made by Mr. Whitehead. Possibly those present may not have appreciated the importance of the investigation to which he referred. Some three years ago Mr. Evershed brought to my notice the fact that he had made such an extraordinary sensitive relay call as he has shown us to-night, although this is the first time I have ever seen it in its complete form. He stated that for the purposes of his experiment she was able to reduce *to scale* the sizes of the coils that he proposed to place at the bottom of the sea and round the lightship, but what he was not able to do was to settle the area and thickness of a copper sheet that ought to be inserted between the primary and the secondary coils so as to correctly imitate the action of the sea; and he naturally added that until he could ascertain that fact he was unable to carry out the complete experiments which would enable him to predict what would happen when a trial was made to ring a bell on the lightship by the action of a coil placed at the bottom of the sea.

I, therefore, entered into a correspondence with Mr. Oliver

Heaviside on the subject. Mr. Heaviside expressed much interest in the problem, and was good enough to write his views to me. Without going fully into the matter, he said that he anticipated that the absorption of the sea would be small, and that the experiment of signalling inductively through it was well worth trying. Mr. Evershed has told you what the result was; and he has mentioned that there were three causes why the result was a failure, any one of which alone he considers would have been fatal—viz., the sheathing of the cable, the absorption by the sea, and that by the iron of the lightship. Now two of those causes we can dismiss, because we can remove them. As Dr. Lodge pointed out the other night, it is possible to employ a wooden lightship. Also, you can imagine that no iron nor steel sheathing were used with the cable. But what we cannot do is to float a lightship away from the land without having sea under it. The question appeared to me, therefore, to be of great interest; and Mr. Whitehead, who was then one of my students, or had just left me—I forget which—was good enough to attack this problem mathematically at my suggestion. He brought me his results and showed them to me before his paper was read before the Physical Society; so that I am responsible, at any rate for the method used in attacking the problem, although not, perhaps, for the accuracy of all his numerical calculations. Now, if that calculation is wrong *in principle*, I should be extremely glad if Dr. Lodge would point out why it is wrong. In other words, if the Maxwellian equations do not hold in the space between two such coils as Mr. Preece has been using in this room to-night, I should like to know why they do not, and, secondly, what equations do hold. Dr. Lodge told you last time he was here that the discussion which followed the reading of Mr. Whitehead's paper was very briefly reported in the *Proceedings of the Physical Society*, so that it was impossible to say what the discussion really had been. Now this discussion, as far as I remember, consisted not only of remarks made by various people, but particularly of a written communication from Mr. Oliver Heaviside. And in this communication, as far as I remember, he objected neither to

Prof. Ayrton. the mathematics nor to the fundamental equations. Was not that so, Mr. Whitehead?

Mr. WHITEHEAD: Yes.

Professor AYRTON: Mr. Oliver Heaviside did not object at all to the equations?

Mr. WHITEHEAD: No.

Professor AYRTON: And the only point he objected to was the value Mr. Whitehead had employed for the conductivity of sea water. There was, however, no doubt about that value, since it was experimentally measured for the purpose. Mr. Oliver Heaviside kindly suggested another method of attacking the problem, but that is another matter. Up to the time, therefore, when Dr. Lodge gave us his most interesting paper, a fortnight ago, I was under the impression that the equations used by Mr. Whitehead were correct. Now it is all-important to ascertain whether they are, or are not, correct. Is it a fact, or is it not a fact, that in 20 metres of sea water, with a frequency of 300 or 400, whatever it was Mr. Whitehead assumed, there is an absorption of something like 79 per cent.—that the field, in fact, is diminished by 79 per cent. in consequence of the conductivity and permeability of the sea water? For, if that be the case, then trying to work Mr. Evershed's call *through* considerable thicknesses of sea water seems to be nearly hopeless. The only other point I want to put to Dr. Lodge is to emphasise the question Mr. Evershed has asked, viz., What is the actual amount of power that must be received by the coil attached to Dr. Lodge's first telephone of his train of telephones in order that the last one may act as a call by giving out a loud sound? Mr. Evershed tells us—and I have no doubt what he tells us is absolutely correct—that if he gives to his instrument 1-1,000th of an erg per second—he did not tell us what that was in watts, but it is something extremely small——

Mr. EVERSHED: Ten thousand million relays can be worked by one watt.

Professor AYRTON: Ten thousand million of his relays can be worked by one watt, or about six hundred thousand million of his relays by the power taken by that glow lamp on the President's

table! Will Dr. Lodge, in his reply, therefore, kindly tell us what is the minimum power that must reach the first of his train of telephones? since then we shall be in a position to judge which of the two systems of call is the more sensitive. Prof. Ayrton.

As, doubtless, many others desire to speak this evening, Dr. Lodge will, I feel sure, excuse me if I do not take up time by enlarging on the pleasure and delight we have all experienced in listening to his paper, and on our expression of thanks for his kindness in giving it to us.

Dr. OLIVER LODGE: I am much obliged for the opportunity of saying a few words at this stage, to remove some misapprehensions. Mr. Evershed's paper was an important communication. He has worked at this subject for a long time, but he appears to keep his work dark, and I had not known of it when I spoke at the British Association meeting at Bristol this autumn. I am very glad that he has now brought it forward, and told us what he has done. I did not quite understand his reference to a fairyland, but he rather spoke at first as if I had suggested that space telegraphy would replace wire telegraphy. I never intended to convey that suggestion. I have always said, when asked, that wherever you can lay a wire, you had better do so; because you generally want to talk to one particular person, and there is nothing like having a wire straight from sender to receiver. There are cases, perhaps—such as newspaper intelligence—where you want to shout it all over the country simultaneously, for which space telegraphy may be suitable; but perhaps Mr. Preece would hardly like us to do that by magnetic induction telegraphy, because it would be liable to disturb the telephones—although by the way, I am not sure that he cares whether they are disturbed or not, except the trunk lines. With regard to the power required for the Evershed "call," I stated (quite truly) that his call takes a good deal of power at the sending end to work it. To this Mr. Evershed retorts, very fairly, that my signalling, as reported in my paper, takes a good deal more. I admit that, so far as I have hitherto had it working at Liverpool, it has taken more power at the sending end to work a telephone at the distant station than with the much longer base Dr. Lodge.

Dr. Lodge. wire at Lavernock he has had to apply in order to work his call at Flat Holm, or *vice versâ*. I do not admit that I needed, or got, more power at the receiving end. Moreover, much less sending power is now sufficient; and with the receiving apparatus which I showed you last time I feel convinced, from the result of my experiments in the laboratory, that the power that is used at Lavernock for giving the audible telephone signals—which is quite a small power, and is quite insufficient and unsuitable to work Mr. Evershed's call*—will be amply sufficient and suitable to work the call that I propose, by aid of the magnifying arrangement which you saw at work last time. I should like to be able to answer the question at once, how many ergs are required to work the first of my magnifying telephones, but I have not got the data to hand at the moment. I will take care, however, to be able to answer that next time. It is evidently a question that demands an answer, but I must say that certainly the power will be exceedingly minute. It is very much less than is wanted to make an audible sound in an ordinary telephone, for instance, and that is pretty small. I prove this by using a pair of induction coils such as happen to be here: one can get a true zero or a graduated effect by tipping them more or less at right angles to each other. I supply a gentle current to one of them from an alternator whose field magnets are not excited, and I connect the other coil either with the first telephone of the magnifying series, or with an ordinary Bell or Ader or Collier telephone. Suppose then that I arrange this ordinary telephone so that I can hear it about as loud as they have it at Lavernock: I can then tip the secondary coil up and up so that the sound shall get less and less, until you cease to hear it altogether. I then tip it up a little more, but not up to zero, and I switch it on to the series of magnifying telephones. In the last of the series I can still hear the signals easily, even loudly, humming out into

* *Note added* 31*st December*, 1898.—Near the end of Mr. Evershed's paper, he speaks of the power available for a telephone at high frequency being greater than that for his call at low frequency; but this is a mistake, if it refers to the actual Lavernock conditions: the self-induction part of the impedance is several times as great as the simple resistance at high frequency.

the room. If I continue to tilt the coil, it continues to act on the magnifying telephone more and more feebly until I get it actually into the neutral position: then the sound suddenly stops; but give one of the coils the least tilt and it is heard again. Dr. Lodge.

I would also say, concerning the speed of signalling with a tuning-fork arrangement, that, though I was afraid it might be rather slow—I was afraid that a tuning-fork receiver would take a little time to work up its swings—yet I find that, using 300 or 400 alternations a second it takes appreciably no time to work up its swings, and however fast the key is worked the series of magnifying telephones respond and the signals come out quite clear and sharp. You cannot tap the dots quickly enough to blend them indistinguishably into one another, so long as they are at all reasonably strong. If they are too weak this is what happens: When the signals are purposely made altogether inaudible, you hear in the magnifying telephone nothing but the usual growl of the microphone; but when you tilt the secondary coil down sufficiently you begin to hear the musical notes of the signals coming; as you tip it up again the signals begin to fail, merging into the growl, and there just comes a point when you cannot detect the signals. You can hear the noise is there, but you cannot signal properly. I do not know that one can really give the exact number of ergs needed for a signal, because it varies considerably with the conditions of the adjustment of the telephone, but in any case it must be extraordinarily small. I propose to modify the telephone from what I showed last time here, by making the upper prong carry the screw, and the other prong carry the other carbon; *i.e.*, the top carbon of the microphone is to be carried by one prong, and the bottom carbon by the other prong. When the fork vibrates properly the microphone will be put into action, but any slight mechanical jarring of the fork as a whole will not be so likely to affect it; thus to some extent imitating an ingenious feature of Mr. Evershed's two rectangles, which are admirably arranged so that a mere mechanical disturbance throws both rectangles up and down together, while the proper electrical disturbance you want to hear vibrates them in opposite phase and so brings them into

Dr Lodge momentary contact. Mr. Evershed's rectangles are thrown into a state of very visible motion, but in my call I do not want any perceptible motion: I only want motion enough to work a microphone—excessively smaller than anything visible, or than anything needed to work any possible kind of discontinuous relay.*

There are some things I should like to have said with reference to Professor Fleming's interesting remarks with regard to the other, the Hertzian, method of space telegraphy, but I think I will not speak of them now. Perhaps they hardly belong to the present subject, which I take to be limited to the magnetic system. I would just say, however, that his remarks about the inadequacy and uselessness of a partially earthed wire I do not wholly agree with. Dr. Muirhead and I have made experiments on an earthed wire, sending what I call an electrical jerk, or, if he prefers it, a very sudden rise of potential, through a wire not insulated, or very imperfectly insulated, or through gas pipes, and I have been surprised at what a considerable distance a coherer at the other end can feel some trace of effect. If the wire is very clean, as possibly it was in Dr. Fleming's interesting experiments in Regent's Park, no doubt a perceptible disturbance does not go far; but an ordinary coat of oxide insulates to some extent. What one means by a bare wire is a wire roughly and imperfectly insulated—not insulated with gutta-percha, anyway—and with such a wire, I believe, by the method of jerks and a coherer, a good deal more can be done than his remarks would lead you to think.

Before I forget it, I wanted to thank Mr. Niblett last time for lending us the batteries which were used for the experiments. Mr. Niblett was good enough to send over to this hall a number of his convenient portable secondary batteries, and I quite omitted to thank him for doing so.

Professor Ayrton says you cannot have a lightship except on the sea, which is true; but you need not put the coil that you are going to use for signalling to that lightship at the bottom of the sea. That is by no means the best place to put the coil. You can

* *Note added later.*—Nevertheless, the motion is sufficient to give an appropriate back E.M.F., as shall be shown in a later reply (page 930).

keep it in the dry, and thereby do away a good deal with the absorptive effects of sea water; though, of course, if the ship be made of iron, or of any other metal, it will screen itself more than any sea water can, even though the receiving cable be outside the ship. Dr. Lodge.

Now we come to Mr. Whitehead, and I am particularly anxious to remove an impression into which Mr. Whitehead seems to have fallen. I only read his Physical Society paper* in the train coming down last time, and it reminded me of a question —which I fancy had been half raised in my mind before—about the calculation of the opacity of a conductor. I am afraid I may have expressed myself hastily, as if I desired to say that his equations were wrong; I did not speak guardedly enough. Mr. Whitehead is a mathematician, and you had far better take his equations and his results as correct until somebody substitutes something else for them; but I do find that this calculation is made by different people in what seems to me different ways, and, if I may be pardoned for indicating the kind of thing I mean, I will write down the equations which Maxwell uses where he shows us how to tackle this problem of electro-magnetic waves passing through a conducting medium.

[*Here an abbreviation of " Maxwell," vol. ii., section* 798, *was written on the board.*]

That is not quite the same as Mr. Whitehead's calculation, nor is it quite the same as what Professor J. J. Thomson uses for a partially similar case. My feeling was that this theory of Maxwell's could not possibly apply to all frequencies. The case treated by Maxwell is a slab with a source of light on one side and a detector on the other, and he is attacking the problem of the transmission of light through a partial conductor, and calculating the absorption of energy to be expected. He makes it depend on the ratio of p, the frequency constant, to q, the wave-length constant; and p/q equals the velocity of light. Now this velocity is nearly the same for all frequencies; so at once you get the logarithmic decrement of the

* *Proc. Phys. Soc.*, 1896-97, or *Phil. Mag.*, August, 1897.

Dr. Lodge. disturbance the same for all frequencies. Hence that slab will be equally opaque whether those vibrations are the rapid vibrations of light or the slower vibrations of Hertz waves, or any other electro-magnetic vibrations. But we know experimentally, as well as by common sense, that a conductor is not equally opaque to all these waves, and that long waves can go through where short waves are absorbed; because, if a conductor wipes out a certain amount of energy at each swing, then, if there are three pulsations in its thickness, there will be a certain percentage wiped out If there are 30 pulsations in its thickness, there will be a great deal more wiped out; and if there is only half a pulsation, then there will be but a small fraction of amplitude destroyed. The opacity must therefore depend on the wave-length as compared with the thickness. Now Mr. Whitehead, instead of getting Maxwell's result, gets something much more reasonable: he finds that the opacity depends on the square root of the frequency; treating the matter as straightforward diffusion. But then Professor J. J. Thomson, who ten years ago treated of a case very like this—viz., a circular oscillator on one side of a liquid layer and a circular resonator detector on the other side, where, however, the circuits are comparable in size to the wave-length —gets a thing like this:—

[*Quotation from "Proc. Roy. Soc.," vol.* 45, *p.* 268.]

Thus you see that, whereas Mr. Whitehead gets an opacity proportional to the square root of the frequency, and Maxwell gets something independent of the frequency, Professor J. J. Thomson, for a case almost identical with the problem before us, though with far more rapid vibrations, gets something apparently proportional to the frequency; hence I do not feel that we have got to the bottom of this problem; though I by no means maintain or suggest that these investigations differ irreconcilably, or that there may not be a way to reconcile them all.*

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* *Note added 31st December*, 1898.—In the discussion on Mr. Whitehead's paper at the Physical Society, as reported in their *Proceedings*, appears a brief statement from Mr. Oliver Heaviside, which represents all that was published of a "note" of three or four pages sent by him to the Physical Society, a copy of

Our present problem of two coils on dry land is, however, not the same as any of those hitherto treated. It is no question of a conducting slab interposed between the two coils, but of a conducting mass in their neighbourhood. The problem is not one of opacity, but is more like the problem of a Hughes induction balance disturbed by the neighbourhood of a conductor. Last week I wrote to Professor J. J. Thomson suggesting to him this problem—not the same problem that Mr. Whitehead attacked, with one coil buried in the sea, where I admit the absorption may be very considerable. (Indeed, in every case I think the conductivity of the earth may give us trouble—I said so in the paper. I regard the matter as one of importance, but one that can certainly be solved by mathematicians.) The problem I put was this: One horizontal coil is elevated on posts at a small height above the earth at one latitude; and another horizontal coil, also at a certain height above the earth treated as a badly conducting sphere, is at another latitude: find what is the damping power of the earth's conductivity on the mutual induction between coil and coil. Just as I came into the room, I received a letter from Professor J. J. Thomson to say that he had worked it out, and giving in general form the solution; it is rather complicated, and he has not yet reduced it to arithmetic. Dr. Lodge.

Professor AYRTON: This problem or the other one?

Dr. LODGE: This problem.

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which he has to-day kindly sent me. It contains a complete solution of the problem—at least, when the wave-length is large compared with the source—including both Maxwell's and Mr. Whitehead's as special cases. Its essence will be found in Mr. Heaviside's "Electrical Papers," vol. ii., page 422; see also "Electro-magnetic Theory," page 452. There are two cases—one where inductive capacity dominates, the other where conductivity dominates. On looking at Maxwell again, I see that he quite realises the two cases, and treats them separately; though, as it seems to me, he applies the wrong one to gold leaf, evidently thinking that from the high frequency of light it would be the one applicable. Mr. Heaviside treats the two cases together—*i.e.*, for all frequencies and all conductivities—in a general manner. His paper is dated November, 1887; so all this was done before the era of Hertz.

I propose to communicate a note to the Physical Society on the matter, because I reckon that the hitherto extremely discordant experiments on the opacity of gold leaf are more nearly accordant with the more general theory, and may possibly lead to interesting results or suggestions concerning the specific inductive capacity of gold.

Professor AYRTON: And he does not know what it will come to?

Dr. Lodge. Dr. LODGE: No; he has no figures as yet: everything is in series and spherical harmonics, and even the form of the solution may depend on what conductivity you assume for the earth; but by postponing the discussion he will no doubt have finished it, and perhaps he will communicate it either directly or through myself.

I thank you, gentlemen, for letting me say what I have had to say so far.

On the motion of Mr. W. H. PREECE, the discussion was adjourned to Thursday, January 12th.

The Three Hundred and Twenty-fourth Ordinary General Meeting was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday, January 12th, 1899—Prof. SILVANUS THOMPSON, D.Sc., F.R.S., Vice-President, in the Chair.

The CHAIRMAN: Members of the Institution will be very sorry to hear of the indisposition of our President. He is absolutely forbidden by the doctor's orders to come out this tempestuous evening, and I am told that he will be obliged to seek a warmer climate for two or three weeks. We all regret to hear that this is so, and I presume the Institution would desire to express its sympathy with him at this meeting.

The minutes of the Ordinary General Meeting held on December 22nd were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

A donation to the Library was announced as having been received since the last meeting from Mr. W. Perren Maycock, to whom the thanks of the meeting were duly accorded.

The CHAIRMAN: The next business is the presentation of the premiums for the session 1897–98. It is a matter of extreme regret to me that the President himself is not able in person to present these premiums to those to whom the Council has awarded them.

The SECRETARY read the list of awards, as follows:—

The "Institution Premium," value £25, to Mr. H. F. Parshall, Member.

The "Paris Electrical Exhibition Premium," value £10, to Mr. R. Hammond, Member.

An "Extra Premium," value £5, to Mr. Leonard Andrews, Member.

Premium for "Original Communication," value £10, to Mr. H. N. Allen.

"Senior Students' Premium," value £10, to Mr. J. M. Donaldson.

"Second Students' Premium," value £5, to Mr. M. Solomon.

"Third Students' Premium," value £5, to Mr. E. E. Tasker.

And two "Salomons Scholarships" of £50 each to Mr. Tom Rolls Renfree, of King's College, and Mr. H. J. Tomlinson, of University College.

The premiums were presented by the Chairman.

The CHAIRMAN: Before I call upon Dr. Oliver Lodge to open the continued discussion, I believe Mr. Evershed has a remark or two to make by way of explanation.

Mr. Evershed.

Mr. EVERSHED: I am obliged to you for giving me the opportunity of correcting a slip of the tongue during my remarks at the last meeting. Professor Ayrton has pointed out to me that, after describing a design for a circuit for an induction telegraph, in which a line was to be laid round the county of Kent in order to communicate with a number of lightships, I said that I was appalled by the enormous size of the condenser, and that this size was consequent upon the large self-induction of the circuit. What I intended to say, and what, of course, should be obvious, is that, in consequence of the very low frequency at which the apparatus was intended to work, the size of the condenser was necessarily large. Of course, at any given frequency, the larger the self-induction the smaller the capacity of the condenser—that is sufficiently obvious.

Dr. Lodge.

Dr. OLIVER LODGE: I had not quite finished what I had to say last time, and since then I have so much more to say that I could take up all this evening if I thought it would be appreciated, and if the audience were not anxious to get on to "wiring rules." I am sorry to take up much of your time, but I shall be as brief as I can. I do not apologise on the ground of the subject, however: I think the subject is eminently one well worthy of your attention, and that three evenings is not too much time to give to it.

Mr. Evershed has corrected one very small slip, but there is

another little slip that I expected him to correct. I did not mention it to him, but I mentioned it to Professor Ayrton, and I do not know whether Professor Ayrton passed it on. I understood him to say, towards the end of his paper, that his relay at Lavernock would receive 600 times less power than was received by the telephone, because of the difference in frequency, if the sending current-strength were made the same—meaning that the power received varied as the square of the frequency. But of course that is ignoring the self-induction of the line. In one of the paragraphs of my paper (sec. 16) I have pointed out—what is no doubt well known—that the current given by an alternating dynamo can be almost independent of the speed in a circuit of low resistance. The circuit at Lavernock, as I understand, is of low resistance, and, although the line is straight, the greater part of the impedance is of the self-induction character; that is to say, the p L term is much greater than the resistance of the line wire, at least when the frequency is high—*e.g.*, at a frequency of 400. Hence, at the frequency of 400, or anywhere thereabouts, up to any number of thousands down to 100 perhaps, the p is almost cancelled, and the current is almost independent of the frequency. Hence you will find the power practically independent of the frequency too,

Dr. Lodge.

$$\mathrm{E\,C}\cos a = p\,\mathrm{N}\,.\,\frac{p\,\mathrm{N}}{p\,\mathrm{L}}\cdot\frac{\mathrm{R}}{p\,\mathrm{L}}\text{ approximately,}$$

until you come to low frequencies. At 16 per second the R certainly does constitute the denominator; so it does, of course, when a condenser is used to neutralise L, but I take it he was referring to actual, not ideal, conditions.

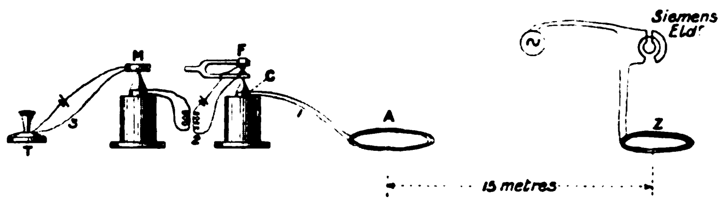
So, also, I expect he will probably say, in reply to my mention of the comparatively large power required to work the relay at Lavernock, that the actual circumstances there are rather difficult: they are not like the circumstances in the laboratory. You are interfered with by earth currents, and by a number of spurious disturbances, and hence the relay must be set not in a delicate fashion. Therefore it is not in so sensitive a condition as when he showed it here, or as I was able to show with my receiver here. That is quite true.

Dr. Lodge. The churning of the dynamo at the sending end in order to get up enough current to work his relay is, in practice, laborious; and in my opinion the plan of calling by running down the gamut of all frequencies, with the aim of getting the right one on the way, though useful perhaps in war time for the purpose of disconcerting the enemy's instruments, is not a plan that will be found permanently commendable in ordinary work. At the same time I cordially admit that Mr. Evershed's relay is in daily work earning its living, whereas mine has not yet had that opportunity. Doubtless under the disturbed circumstances of an actual line with earth connection it is not, and will not be, possible to get any receiving instrument into a condition of the most refined delicacy, lest it yield to accidental causes and furnish spurious effects.

Now coming to the power question: I only partially agree with Mr. Evershed in laying so much stress upon power and energy considerations, to the exclusion of current and E.M.F. If one knows the E.M F. available for a given circuit, one knows the current; and when current and E.M.F. are known the power is known, because for a given circuit the lag is definite for each frequency; but by skipping direct to "power" one ignores some useful factors, a knowledge of which may affect the design of a suitable circuit. Again, the statement that a certain signalling instrument can be operated by the hundred-millionth part of a watt, or that a watt can work a hundred million instruments, is calculated, perhaps, to strike those into whose daily practice kilowatts and Board of Trade units largely enter; but, after all, of most detectors of current the same sort of statement could be made. It would be easy to treat a sensitive galvanometer, for instance, as a motor, if it were worth while, and to state the ridiculously small energy needed to produce a visible deflection. So, also, a telephone can be treated as a motor, and its already surprising sensitiveness made still more obtrusive. Nevertheless, there are certainly some advantages and some useful information to be gained by adopting Mr. Evershed's mode of treatment.

Professor Ayrton asked, last time, what power will work one of my magnifying telephones when a tuning-fork is employed

as an essential part of the receiver for the purpose of eliminating accidental disturbances and causing it to respond only to those which are of the right pitch; so I have had a special determination made, not aiming at anything excessive, but just asking Mr. Davies to make certain measurements with the instrument in its average ordinary condition, so as to give me the data I require. He reports as follows* :— Dr. Lodge.



Receiving circuit, a coil, A, of 25 turns, 85 centimetres diameter, in circuit with the little aluminium ribbon coil, *c*, of the tuning-fork magnifying telephone, this coil having 36 turns, 1¼ inches diameter. Total resistance of circuit, 0·9 ohm; self-induction, 0·005 henry; resistance of aluminium coil with flexible connections alone, 0·43 ohm.

The fork magnifier, F, was connected through a transformer with a Berliner magnifier, M, and that connected direct to an ordinary "Western Electric" loud-speaking telephone of 2 ohms resistance. This latter telephone, T, could be switched either into circuit 1 or circuit 2 or circuit 3, at pleasure.

Sending circuit, a coil, Z, of 50 turns, 84 centimetres in diameter, placed with its centre 15 metres (50 feet) away from the receiving coil (both coils lying flat on the floor of different rooms on the same level), and connected in series with an alternator working with residual magnetism only, a resistance box, and a Siemens electro-dynamometer. All leading wires well twisted

* The account which follows is one which was sent in to the Secretary before the meeting, in case I was not able to be present, and is not verbally what I said; but it is the same in essence, with a few additions which I omitted to refer to for lack of time, and with a numerical slip in the mutual induction coefficient. M, corrected.—O. J. L.

Dr. Lodge. together; the frequency adjusted to 384 per second, corresponding to that of the loaded fork.

Under these conditions, if a current 0·42 ampere circulated in the sending circuit, the telephone, T, just responded, even when in circuit 1; in circuit 2 the signals were amply strong, and in circuit 3 unnecessarily loud.

On reducing the current in the sending circuit to 0·1 ampere, the telephone, T, gave no response in circuit 1, in circuit 2 it gave usable signals, and in circuit 3 they were comfortably loud.

By taking pains, and having the telephone, T, in a quiet room, away from the hum of the alternator and other noises, the sending current could have been decidedly reduced without rendering the signals inaudible in circuit 3; but no effort was made in this direction, the data now obtained being sufficient.

Calculating the mutual inductance of the two circuits for this case from the data given, it comes out 11·6 centimetres. [At the meeting I said 47 centimetres, having omitted a divisor 4.]

The primary current is 0·1 ampere, or 0·01 C.G.S.; and the frequency is 384 per second, or $p = 2{,}414$.

So the E.M.F. induced in the secondary circuit has an amplitude p M C = 281 C.G.S. = 2·8 microvolts.

The impedance of the receiving circuit is p L = 12 ohms, so the current amplitude is a trifle less than $\frac{1}{4}$ microampere.

Ignoring the lag and taking the maximum possible power, it is thus found to be about 0·63 micro-microwatts for the whole receiving circuit, and of this only a fraction (say about a half) is consumed in the actual coil of the magnifying telephone.

But taking the lag into account the power is really much less, for $\tan a = \frac{p\,L}{R} = \frac{12}{0{\cdot}9}$ approximately, or say 12 since there may be a trifle of throttling; so $a = 85°$, and $\cos a = 0{\cdot}08$.

Wherefore the power needed by the *whole receiving circuit* is a twenty-millionth of a millionth of a watt; and this must last for about the tenth part of a second in order to give a signal. To give a call—*i.e.*, to ring a bell—it must last longer—a whole second or two, for instance; but in no case can the power be considered excessive.

If a condenser is inserted in the receiving circuit to abolish the lag, the induced current rises in strength, and with it the loudness of the sound, enabling the *sending* current to be still further diminished. But the received current here measured is the current needed to make the magnifier decidedly respond; and it amounts to 9 microampere-turns. Though, as said above, with care this could be reduced. As it stands it is about 120 times as sensitive as Mr. Evershed's estimate of the ampere-turns required for an ordinary telephone, and that is about what one might expect. Dr. Lodge.

If we prefer to reckon the watts as R C^2, taking into account only the energy dissipated, we get for the energy expended in the above coil *c* per second $0{\cdot}43 \times 0{\cdot}055 \times 10^{-12}$ = one-quarter of the ten-billionth of a watt; agreeing very well with the above estimate of half the ten-billionth of a watt for the entire receiving circuit.

Wherefore the energy needed to pull, as it were, the microphonic trigger and stimulate the series of magnifiers into the production of an audible signal is less than the ten-millionth of an erg—*i.e.*, less than the thousandth part of a milligramme dropping the thousandth part of a millimetre.

Observe that in Mr. Evershed's relay there is a visible motion of the rectangle. The wire coil moves in a magnetic field, and you get a back E.M.F.; but *here* there is no perceptible movement. It is simply required to vary the pressure of the carbons in the microphone. There is nothing like a perceptible motion; it is something almost molecular, and I suppose there is no back E.M.F. to speak of. [See footnote below, however.] All that the signalling current has to do is, as it were, to pull a trigger, and thereby throw fresh energy into the circuit from the local batteries, and the resulting energy in the final telephone bears no necessary relation whatever with that of the current in the circuit which pulls the trigger or stimulates the whole into activity.

The existence of any appreciable back E.M.F. due to the motion of the receiving instrument will reduce the current as now estimated, and therefore reduce the power actually available below the present calculated power. But of course this back

Dr. Lodge. E.M.F., if data could be got for it, would give the actually useful net power, and so the efficiency of the telephone considered as a motor.* But I take it that what we want is not the minimum or net power—not the power on the brake, so to speak—but that it is the gross power supplied to the receiving circuit that we ought to estimate.

Although the current here detected by the arrangement is as great as $\frac{1}{4}$ microampere, yet that is a small current for a coil of only 36 turns, and less than half an ohm resistance, to detect; and by substituting a fine-wire coil the perceivable current must necessarily be very minute. I have no doubt that the magnifying telephone must be the most sensitive detector of alternating currents of low frequency yet made; though the eye and the

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* *Note added 14th January.*—Taking a hint from Mr. Evershed, it may be worth while to make a rough estimate of the back E.M.F. probable or possible in a magnifying telephone with imperceptible excursion for its coil. Hitherto, I had treated this E.M.F. as negligibly small, being satisfied that whatever value it had would only go to *improve* my estimate of the smallness of the total available gross power. But, on considering the matter, I see that the back E.M.F. is likely to bear a very fair proportion to the applied E.M.F., and may even come up to the most advantageous "half."

A coil of 36 turns, $1\frac{1}{4}$ inches diameter, vibrating across the lines of force a distance $2x$ in a field of induction density B, at a frequency 384, will generate an E.M.F. whose root-mean-square value is

$$\epsilon = 2\pi n r x p \mathrm{B}\, 1/\sqrt{2} = 252 \times 2{,}414\, \mathrm{B}\, x.$$

The field intensity in the narrow air gap of one of my magnifying telephones, when moderately excited, is usually a little more than 10,000 C.G.S.; so a reasonable value for the back E.M.F. is

$$\epsilon = 60 \times 10^{8}\, x \text{ C.G.S.}$$
$$= 60\, x \text{ volts.}$$

The *applied* E.M.F. in the receiving circuit (for the production of a faintly audible note after one magnification) was found to be three microvolts = 281 C.G.S. (see above); so the movement x needed to give a back E.M.F. comparable to half this magnitude is $\frac{1}{2} \times 10^{-7}$ centimetre—*i.e.*, half the thousandth of a micrometre, or half a millionth of a millimetre (the so-called $\mu\mu$); a quantity closely approaching molecular magnitude, and a very reasonable sort of quantity for the amplitude of the faintest perceptible microphonic disturbance. I find that the actual induction density employed in the trial was 12,000 C.G.S., and so the actual microphonic excursion used for the weak signals was probably not very different from the estimate just made.

As a check, I will take Lord Rayleigh's calculation of the energy of a tuning-fork with a given amplitude, and see how that agrees. The energy is $\frac{1}{4}\, m p^2 x^2$, where m is the mass of either prong; for a 512 fork loaded to 384 let us say $m = 60$ grammes: then the energy at half a milli-micrometre amplitude is $15 \times (1{,}200)^2 \times 10^{-14} = 2 \times 10^{-7}$ erg, which is of the right order of magnitude.

photographic plate probably surpass it when the frequency rises to hundreds or thousands of billions per second. Dr. Lodge.

Lord Rayleigh estimates that an acoustic amplitude of aerial disturbance of 10^{-7} centimetre is certainly audible (at a frequency of 2,700), and possibly 10^{-8} centimetre. The energy per unit volume of such a disturbance is $\frac{1}{2} e (2 \pi a/\lambda)^2$, or—what is the same thing—$\frac{1}{2} \rho p^2 a^2$; and, since the elasticity of air is 1·4 atmospheres, and the velocity of sound in it is 33,000 centimetres per second, the power received per unit area is

$$33{,}000 \times 0{\cdot}7 \times 10^6 \times \frac{40 \times 10^{-15}}{144}$$

$$= \frac{33 \times 7}{36} \times 10^{-6} \text{ C.G.S.}$$

$$= 6 \text{ micro-ergs per second on a square centimetre.}$$

Regarding this as the order of magnitude of the power needed to affect the ear, the power required by the eye appears to be not very different (see Rayleigh's "Sound," art. 384, footnote). Assuming that the eye needs the same minimum power as the ear, and taking the aperture of the pupils of both eyes in the dark to be not far short of a square centimetre, and the energy per unit volume of light to be $4 \pi \mu C^2$—where C is the average strength of displacement current existing in the beam—the power received by the eyes per second would be $4 \pi \mu v C^2$. But $\mu v = 30$ ohms; so, expressing the above postulated minimum power (6 micro-ergs per second) in watts and the current in amperes, we get $12\frac{1}{2} \times 30\ C^2 = 6 \times 10^{-13}$, or $C = \frac{1}{25}$ microampere. This would mean that the eye can detect an alternating current of appropriate frequency whose strength is about the twenty-fifth, or, since 6 micro-ergs is probably an over-estimate, say the hundredth, of a microampere. It would therefore seem as if a magnifying telephone could be made as electrically sensitive as the eye.

Returning to telegraphy: I have no objection to taking Mr. Evershed's expression (4^a), page 855, for the power received at a distant station under the most ideal conditions, and seeing what distance it suggests as possible with a given power and weight of copper.

Dr. Lodge. Take a ton of copper, distributed in a couple of squares 1 kilometre in the side, and apply 1 kilowatt at the sending station, with frequency 400: then the distance at which it can affect a magnifying telephone under the most favourable ideal circumstances is given by

$$D^3 = \frac{p\, s^2\, V}{32\, \rho} \cdot \sqrt{\frac{W_p}{W_s}} = \frac{2{,}500 \times 10^{10} \times 10^6}{32 \times 1{,}600} \cdot \sqrt{\frac{10^3}{10^{-13}}} = \frac{10^{24}}{205};$$

wherefore

$$D = \frac{10^8}{6} = 160 \text{ kilometres, or say } 100 \text{ miles.}$$

The most potent factor towards increasing this distance is size of circuit: increasing the linear dimensions of both circuits increases the distance in about the same proportion (the two-thirds power), whereas increasing the horse-power applied to the sending circuit is but of little use. Nor does the advantage of increasing the sensitiveness of the detector make itself rapidly felt; bulk of copper and size of circuit are more effectual. This simple mode of calculation, however, will by no means bear pushing to extremes, and the whole subject is gone into more thoroughly in my paper. It is quite true, however, and convenient, that there is no necessity for the expenditure of much power at the sending end, if all the other arrangements are good.

Now a few words only about opacity calculations—that is to say, the damping by the intervening layer of an electrolyte such as sea-water. I have gone into that pretty fully since last time, and I think I have got to the bottom of it, and realised the sort of reconciliation that there is between Clerk Maxwell's calculation of opacity and Mr. Whitehead's calculation. They turn out both right, but both refer to different cases. They are both extremes, and if either is wrong in its application, it appears to be Maxwell's. Maxwell applies to gold leaf a theory which is not, in my judgment, quite applicable to gold leaf; and he gets a transparency for gold leaf vastly less than experiment gives. But Mr. Heaviside has given a more general solution, which includes the whole of these two extremes; and this, when one comes to work it out, reconciles the whole thing and makes a satisfactory calculation. I propose to say something about that at a meeting

of the Physical Society—perhaps the anniversary meeting, on the 10th February—and so I need not say any more about it now. But I would just like to ask Mr. Evershed whether, as a matter of fact, in his experiments on the Goodwin Sands, the ship was of iron, and whether the cable was inside or close to the sides of the ship, because, of course, that would screen a great deal more than the sea-water. Further, with regard to the submerged cable—the cable at the bottom of the sea—was that sheathed in iron? because, if so, no further explanation is needed of the failure of the experiment: the sea-water would short-circuit the iron sheath and constitute it a closed secondary close to the primary. Dr. Lodge.

With regard to the absorbing action of liquid conductors in general, I did make a few experiments myself on the effect of the neighbourhood of conductors, some six months ago; putting near the primary coil a sheet of copper, which, of course, produced a great weakening, as one might expect; and a sheet of tin-foil, which produced very little; and a pan of dilute acid, which produced no appreciable effect. Hence I thought it likely that liquid conductors, being of so high resistance, need not be very much taken into account. I did not try immersing the coil in acid, for I was not then thinking about a submerged cable and an intervening stratum; I was thinking only of the effect of earth or sea on cables laid on or near its surface. The experiment was not an exhaustive one, and the result was by no means conclusive, but it indicated that no very serious or fatal disturbance due to the earth's conductivity was to be expected at any ordinary distance.* And my two-mile experiment between College and house is in the same direction; for most certainly it is a case of pure induction—there are no earth connections.

I mentioned last time that I had requested Professor J. J. Thomson to attack this subject, viz.: a primary coil supported horizontally on poles at one part of the earth, and a secondary coil

* *Note added January 14th.*—On this Mr. Evershed makes the interesting remark that when the inducing circuits are varied in size the conductivity of the screening medium should be varied too, in order to make the test applicable on a different scale. This is perfectly true, and it is a point I had overlooked. The damping will depend partly on the ratio of absolute size to conductivity.

Dr. Lodge. similarly supported at another part of the earth's surface, to see what effect on the mutually inductive influence of those two coils the conductivity of the earth would have; and I said I had just got the beginning of a solution from him at the last meeting, though it was at a stage very much in the air, full of spherical harmonics, and difficult to apply as it stood. In any case the calculation is complicated. Yesterday I had another note from him to say that he had been working at the problem further, and he came to the conclusion that, if the whole earth conducted anything at all as well as sea-water,—if the whole body of the earth conducted as well as that, —if that amount of conductivity existed at considerable depths in the earth,—then you might almost as well have the earth a perfect conductor. Now, if the earth were a perfect conductor, naturally the magnetic force at its surface would be wholly tangential, and there would be no vertical magnetic force for a horizontal secondary coil to feel. Hence it appears that the telegraph will not work with horizontal coils if the earth is as good a conductor as sea-water. One may look at it from an elementary point of view. Taking the earth as a perfect conductor, then we need only consider a perfectly conducting shell, and I suppose one may say that a coil on that shell will be imaged by a coil beneath, and that the current induced in the coil beneath will be in opposite phase to that in a horizontal coil above; and hence at a distance the vertical magnetism will be weak. Now this is very deadly. Supposing it gets borne out by examination and criticism—and I think it is likely—this is very deadly to horizontal circuits. I do not see Mr. Preece here to-night. I am sorry not to see him here, because I think he will be glad to hear that I am coming round to the notion of vertical coils. I have been coming round gradually to the notion that vertical circuits may, after all, be better than what I at first started with—horizontal circuits; and this result of J. J. Thomson is a tremendous argument in favour of vertical coils, for he has also calculated what the effect of vertical coils would be. If you set up coils in planes perpendicular to the earth, then the conductivity of the earth, so far from hindering, actually helps. That also one may see if one takes an extreme case—takes the earth as a perfect conductor.

At least, I suggest it. Think of the surface of the earth as a horizontal mirror, and then think of a vertical coil; let a current be started in the coil in one direction, and attend now to the imaged coil. The starting currents in adjacent parts will be inverse. Or one may say that, whereas the lines of force in the horizontal coil pass in the same direction through the image, the lines of force of a vertical coil come out and pass in an opposite direction through the image. The consequence is that the coil and its image are in the same phase—both coils are pulsating together—with the result that they increase each other's action, so that at a small distance the effect of a vertical coil is doubled by reason of perfect conduction in the layer of earth below. A semicircular conducting arch would be virtually completed by earth reflexion, and the lines of force grazing the surface would be reflected or continue tangential. I do not suppose the effect will be so simple as that, with the actual earth, but at any rate it it appears to represent a great advantage for vertical coils. Dr. Lodge.

One word more, however, about those horizontal coils. The damping effect, as calculated, appears to be about the same at all distances. Whatever effect the earth's conductivity has, appears at first sight, on the theory, to be independent of distance. But that can hardly be the case, because if the coils are near together they do work. The chief question is, What will happen when they are far apart? If the damping were really the same at all distances, then it would not be so deadly, and a measurement of what the damping actually is, would give us information about the conductivity of the deep parts of the earth, and be of geological interest. But it appears that when the coils are close together there is a certain series which need not be convergent, and that the effect of coils close together, within very small angular distance, must be separately examined; and that has not yet been done.

I would just like to say why I had been specially thinking that vertical coils would be, in some other respects, good. They would be especially good for ship-signalling. It has always been a difficulty how to put a horizontal coil on board a metal ship, so near a conducting mass, without its being altogether screened.

Dr. Lodge. Mr. Preece, I think, urges that the right way to put a circuit on a ship is from the bowsprit to the top of the foremast, along the tops of the other masts, and down to the stern, taking the wire across the masts like *that*, and then, I suppose, connecting to the body of the ship or using an earth-plate. I agree, and I think earth-plates are often good things, because you get the advantage of earth-tapping as well as of induction; and indeed I think very often earth-tapping is the greater power. But why should it not be? The earth-tapping method is quite a good method; its chief trouble is that it introduces spurious earth currents. Suppose now that a vertical cable is made part of the rigging of a ship; the disadvantage of a ship arranged like that is that its receiving power or signalling power will depend upon its aspect. It can signal broadside on, and it can signal with less intensity end on, but it cannot signal in every azimuth. That is a disadvantage from some points of view, but it becomes an advantage from other points of view. People often say what a great thing it would be in a fog if you could signal to a ship, or a ship could signal to a lighthouse; but picture to yourself a fog. A ship with a horizontal coil comes, and she hears the signal of a lighthouse, and is able to speak to the lighthouse and ask, "What are you?" and the lighthouse responds. Then the ship says, "Where are you?" and the lighthouse says, "Well, you can "look at your chart to find where I am." Then the ship says, "Well, where am *I?*" and the lighthouse says, "I don't know: I "cannot see you; but you sound pretty near, and you had better "take care." That is about all it can get; and if its apparatus happens to be in rather feeble order, it will think it is far off when it is not. It cannot even tell the direction whence the signal comes, because for horizontal coils every azimuth is the same. Hence the ship can only make a very rough estimate of distance, and cannot be told where she is, so the signalling apparatus does not help her very much. Now contrast this with the case of a vertical coil. The ship can swing round. Supposing she is in a fog, and hesitating where to go: when she hears the noise, she swings round, and finds the influence gradually increasing till she is in a certain aspect, or else gradually diminishing

and ceasing when she is in another aspect; from these observations Dr. Lodge. she can make an estimate of the direction of the signalling station. She may have a difficulty in knowing whether it is towards the bow or the stern, and that may be awkward. But I think you can make it do more than that. Sir Henry Mance last time suggested signalling by two notes; he meant, I believe, instead of dots and dashes, a right and left signal, as it were, between two bells—two notes. I do not know that that would exactly suit the inductive method, as far as I see at present; but, at any rate, a modification of it would suit two different stations. Supposing a ship is wishing to enter a harbour (diagram). On one side of the harbour is a fixed station, signalling magnetically a certain note – we will say the note C—while on the other side of the mouth of the harbour is another signal station, sending another note—we will say note E; and outside is the ship in a fog, with apparatus attuned to both these notes. She will hear the chord of a major third if she is pointing straight for coming in. If she were to turn round a bit now, she would hear one increase and the other decrease; and if she got exactly conjugate to one station, that note would cease to be audible, and the other note be heard alone. Very well, then she can take her bearings by the compass and say, "That station bears so and so;" next, she can turn back again till she gets silence on the other note, and so take her bearings on the other station. Having done this, she could, I suppose, now lay off on the chart the bearing from one station and the bearing from the other station, and, producing them till they intersect, find out exactly where she is. This suggestion needs a good deal of developing before it is practical; but it may contain a germ.

I am obliged to Mr. Preece for the offer of the powerful aid of the Post Office service in connection with further experiments, for I feel that by thus working in conjunction with the Government something good can be accomplished, and most of the outlying island and lighthouse stations be gradually welded into permanent communication with the adjacent coast. I believe that much more than this can be accomplished, and that it will be attempted before long, either by America or by ourselves. It now only

Dr. Lodge. requires a little friendly co-operation and absence of rivalry to put the matter on a sound basis, and to begin to work towards a rapid extension of communication with coast stations, and, I hope, also possibly with scattered and at present isolated parts of the world.

Mr. Granville. Mr. W. P. GRANVILLE: In attempting to discuss Dr. Lodge's most important and valuable paper, I propose to emphasise the practical side of the problem, because it is well for us as electrical engineers to have some idea of the difficulties that will probably arise in the practical application of wireless telegraphy. It is clear that, if wireless telegraphy is to come into general use, it will largely be limited to those cases where ordinary telegraphic communication is not practicable; and, according to one's own experience, such exceptional cases present exceptional difficulties, which will in many instances completely bar any such system yet described. But before referring to this phase of the subject I should like briefly to refer to the work of one of our Past-Presidents; his name was mentioned at our last meeting by Major-General Webber—I refer to the late Mr. Willoughby Smith. Some of you may remember, on the occasion of his Inaugural Address in 1883, that he described a large number of experiments relating to magnetic induction; and, fortunately, I am able to show here one of the original diagrams by the aid of which, and by actual experiment, Mr. Willoughby Smith demonstrated many of the laws of magnetic induction, and at the same time measured the absorption when plates of various metals were interposed between the sending and receiving coils: the results of these measurements are recorded in the Journals of this Institute. In Fig. A the inducing coil is shown on the left, and the receiving coil on the right, and a screen of metal is shown symmetrically placed between the two coils. Since our last meeting opportunity has been found to determine more accurately, by means of a chronograph, the effect upon the induced current caused by the interposition of metal plates as shown in Fig. A; and Fig. B shows the results obtained, and clearly indicates the detrimental effect produced by the near presence of conducting bodies. If time had permitted, I should

like to have referred to this at greater length, but I will simply Mr. Granville.
mention that Mr. Willoughby Smith on that occasion suggested the

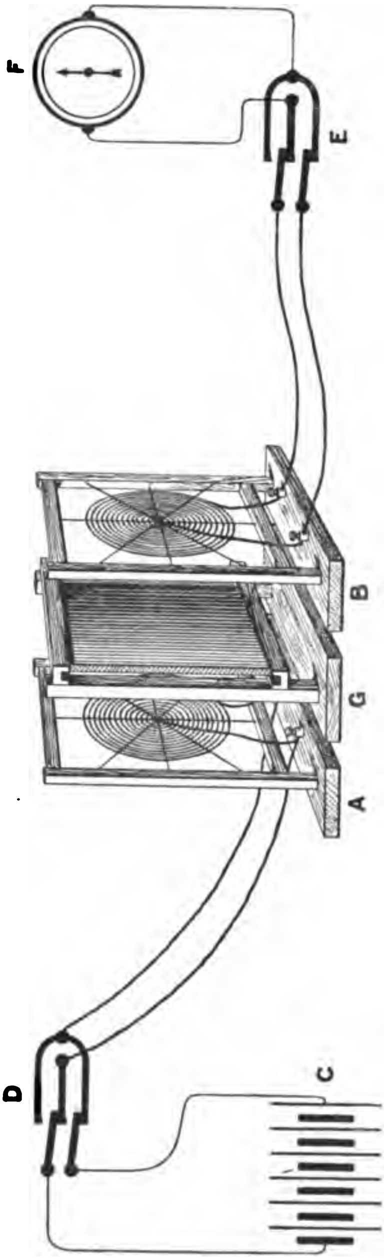


FIG. A.

Mr. Granville.

use of magnetic induction for telegraphing with trains in motion, and for other purposes. The idea, however, was not very well received at the meeting, but times have changed since then. During the last ten years or so, Mr. Willoughby Statham Smith and I have had the task before us of devising some practical system of telegraphic communication which should be suitable for those positions to which ordinary methods are inapplicable, such as outlying rock lighthouses; and we have made a large number of experiments in the laboratory, and also at the Needles lighthouse, in Alum Bay. The results of these experiments were

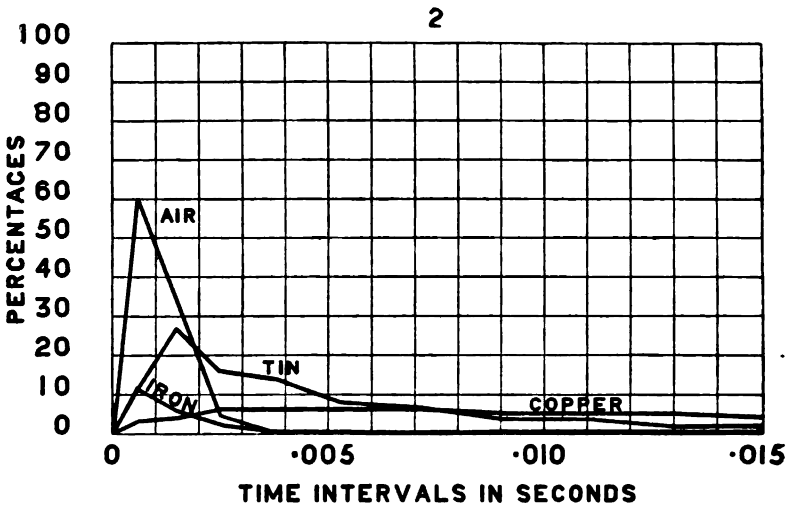


Fig. B.

such that we devised a system that should be applicable to outlying rock lighthouses, and on the recommendation of the Royal Commission on Lighthouse Communication, and by direction of the Post Office, we practically applied the system to the Fastnet lighthouse. Now that lighthouse affords so excellent an object-lesson—it being one of the cases where wireless telegraphy, if it is to be of use, must be able to adapt itself—that I do not think you will consider it waste of time if I ask for a photograph to be thrown on the screen, so that you may better realise the difficulties which have to be met with in those exceptional cases

Mr. Granville.

which are so peculiarly suitable for its application. [*Slide.*] The photograph was taken on an ordinary August day two years ago. The rock is always surrounded with a belt of foam, and no landing can be made on the rock, except by means of a jib 58 feet long—not at all a pleasant proceeding. Now here is a case where the Government desired to effect communication telegraphically, but, as had been proved by very costly experiments, it was impossible to maintain a continuous cable, the cable being repeatedly broken in the immediate vicinity of the rock. This, therefore, is a case where some system of wireless telegraphy is absolutely necessary, but neither of the systems yet described would answer here. Dr. Lodge advises us to eschew iron, and to avoid all conducting masses. But the tower and all the buildings are built of boiler plate, and that which is not of iron is of bronze. In fact, the rock itself is the only bit of non-conducting, and therefore non-absorbing, substance for miles around. It is very clear in a case of this sort—and this is a typical case—that it is absolutely impracticable to employ here such a method. Now we hear in regard to the method used—and successfully used—at Lavernock, that a certain base is required, of perhaps half a mile, a quarter of a mile, or a mile in length; and that base must bear some proportion to the distance to be bridged. But where can you get any such base on the rock? You could barely get a base of 20 yards, so that method utterly fails. Then we come to the case suggested by Mr. Evershed, of a coil which would be submerged round the rock. Well, where would the coil be after the first summer's breeze, let alone after a winter gale? Why, probably thrown up, entangled, on the rock. A few years ago, during a severe gale, the glass of the lantern, 150 feet above sea level, was smashed in; and at the top of the rock, 80 feet above the sea level, the men dare not, during a winter's gale, leave the shelter of the hut for a moment, for, as they said—and I can well believe it—they would be swept off like flies. This is a practical point, and, therefore, one I am glad to bring to the notice of the Institution; and, I repeat, if wireless telegraphy is to be of use, it must be of use for these exceptional cases.

Mr. Granville.

I will ask for the next photograph, showing how we surmounted the difficulty. [*Slide*] There you see a plan of the harbour of Crook Haven, 8 miles from the Fastnet Rock, the latter being drawn out of scale for the sake of clearness. If you suppose Mr. Preece's base line on Lavernock rotated so that one end is brought out to the near vicinity of the rock, then you have an approximation to our own method—it is wireless telegraphy reduced to its least common measure; and of course there is no virtue in increasing a difficulty: our object as electrical engineers is to find the simplest means of overcoming a difficulty, and here only that part of the path is wireless where it is impossible to maintain a wire, and thus, instead of needing a dynamo for signalling, we are readily able to signal with a few Leclanché cells. The end of the cable in the immediate vicinity of the rock, just outside the belt of foam, was connected to a mushroom anchor weighing 5 or 6 cwts., and submerged in 12 fathoms of water; so that by sea leakage we are enabled to pick up by the two rock "earths" a small fraction of the sending current, and now the lightkeepers exchange daily messages with the shore. The rock is as hard as granite, and we took about three months to cut a chase down the side of the rock, and embedded one of the earth lines therein; but, unfortunately, its lower portion was washed out three weeks afterwards. Not to be defeated, however, we then used a diamond drill, and bored a hole 40 feet down through the rock, so as to emerge far below the water level. On one side the drill refused to come out, because the rock sloped a little, but a small fissure at the bottom of the hole gives some contact with the sea. We hope to get permission to put in a small charge of gunpowder so as to enlarge the fissure, or else we may have to drill again. In the meantime, however, signals are exchanged daily, and I merely mention the matter as an illustration of the difficulties that will have to be contended with in wireless telegraphy, if applied where it is needed, and not simply in selected places.

I do not propose to enter very fully into the discussion, but I should like, with some little diffidence, to refer to a statement in Dr. Lodge's paper where it is said that the whole matter

whether wireless telegraphy is assisted most by induction or conduction might be settled by experiment, but that if either coil were not earthed an experiment would be absurd. Now is that necessarily so? That is to say, if the transmitting coil has no "earth" connection, is it absurd to suppose that another circuit with ends "earthed" could possibly pick up the current by conduction? Surely wherever the lines of force cut a conductor a current is induced, whether the conductor be the receiving coil or a portion of the earth's surface; and that current in the earth might be picked up, I take it, by any earthed circuit in the neighbourhood—for instance, the telephone lines in Cheshire, as referred to in the paper. Mr. Willoughby S. Smith and I made an experiment during last week on a canal barge; and the results led us to think that this is the case—that conduction, even where the receiving coil is not earthed, may largely assist. This may explain why Dr. Lodge was only able to signal a distance of two miles "in free space," and yet in Cheshire they were able to hear the signals on the telephonic system. Mr. Preece, in his paper, mentioned an experiment tried with a certain length of wire,—the wire first being suspended in a straight line on poles and ends earthed, and then compared as a sending circuit with a similar length of wire made up into a coil; and the paper points out that very much better results were obtained by the former method. Yes; and is it not probable for the same reason as indicated above? All our own experiments—and we have made a very large number—tend to show that that would be the result, because of the great benefit obtained by earth leakage, or, rather, conduction. And from the results of our own experience we have come to the conclusion that, after all, whether we are using the Hertzian wave system or the coil system, or whether we are using the system used at Lavernock, Mother Earth does affect the results much more than the ether of free space. And it is a fortunate thing that she does, because the energy is thereby confined in a comparatively thin layer of conducting earth instead of being dissipated in space. Mr. Granville.

In conclusion, I should like to say that this matter has been brought up from a practical point of view—not with the slightest

Mr. Granville. idea of diminishing or depreciating work that has been so well done and described by Dr. Lodge, but rather to present the practical side of the problem as derived from actual experience.

Captain Brett. Captain W. P. BRETT, R.E.: After the last speaker, I do not think I need offer an excuse for introducing the practical side of the question. Personally, I regret very much that Dr. Lodge has apparently forsaken the experiments with Hertzian waves; because I think myself, if it comes to a case of the survival of the fittest, that it is the Hertzian wave method that will survive. Apparently many here are not acquainted with what has been done in connection with the Hertzian wave method, and, as I have had rather exceptional opportunities of watching its progress, it may interest you to know. In the first case Mr. Evershed pointed out that the electro-magnetic methods certainly possess the considerable advantage that calculation with them is easier; on the other hand, the wave method possesses the advantage that experiment is very much easier. When you come to consider one detail alone—viz., to try the effect of increasing distance—with electro-magnetic methods you have to transfer your tons of copper to these different distances. Compare with this the act of transporting a vertical wire, which may be effected with almost as much facility as towing a toy balloon. I think that this is borne out more or less by the indebtedness which every experimenter with electro-magnetic methods (and one may add, too, with Hertzian wave methods) owes to Mr. Preece; because, without having a large private estate at your disposal, or the facilities of the Post Office authorities, I do not think it is very feasible for people to carry out experiments on anything like a large scale. I myself am glad to have the opportunity of acknowledging my own indebtedness to Mr. Preece for his ungrudging assistance. I do not think that the statement made in Dr. Lodge's paper that Mr. Preece's system is the only one in actual regular work can be quite accepted; because Mr. Marconi has for nearly 12 months been exchanging messages daily between Alum Bay, Isle of Wight, and Bournemouth, a distance of 17 miles. On the 8th December last I had the pleasure of exchanging many messages between "Sandbanks," near Poole, and Alum Bay, a distance of 18 miles:

I want to emphasise that distance, 18 miles, and ask you to consider it in the light of the calculations which you find in the papers, and also to note that it is nearly six times as far as the existing installation between Lavernock and Flat Holm. The excellence and certainty of the messages sent, I assure you, leave little to be desired. It may also interest this meeting to know that since the date of the last meeting the difficult problem of communication between a lightship and shore has been practically and satisfactorily solved. Certain details have appeared in the public Press, which are, however, wanting in accuracy. Since last Christmas Eve the East Goodwin lightship has been in uninterrupted communication with South Foreland Lighthouse, a distance of over 12 miles. I had yesterday the pleasure, by the kindness of Mr. Marconi, of seeing the working of the installation. I have here some of the tape records of the messages received at South Foreland in my presence. Yesterday was a practical test in every way. The weather was exceedingly stormy; the Channel traffic was stopped. In some of the messages there was internal evidence of the state of the weather. The motion of the lightship was too much for the assistant on board. However, a very important practical point was brought out—that whenever the assistant was *hors de combat*, one of the seamen on board, who had a certain acquaintance with the Morse code, maintained the communication sufficiently well. I have here two messages sent by one of the seamen after a week's practice. It has been stated that iron masts and wire stays would interfere with messages being received on board. The East Goodwin lightship has iron masts and it has wire stays; they apparently make no difference whatever. That will be interesting to the last speaker, because I presume that even with boiler-plate lighthouses there need be no interference. Other interesting points are that the alternators which are used at the South Foreland lighthouse do not affect the system, even when the vertical wire is brought within *one inch* of the alternator circuit; and also that telephones in the same building are quite unaffected. That is an important point for many practical reasons. I am personally, of course, mainly interested in the application of the system for military

Captain Brett.

Captain Brett.

purposes, and any system that interferes with telephonic communication is out of the question for coast defence, where telephones are used to a large and increasing extent. Also there is a fatal want of mobility about these electro-magnetic systems, whereas the vertical wire coherer system possesses a very fair amount of mobility indeed. Previous speakers have laid some stress on speed of signalling. Although it is a consideration, I think it is a very secondary one. Space telegraphy is not going to rival telegraphy with wires at all, but it will afford means of telegraphic communication where previously it was impossible, and people who thus acquire facilities of communication are not going to grumble because their messages are sent at 10 words a minute instead of 20. There is one other point, in reference to the suggestion of Dr. Lodge that "earth conduction plays a "considerable part in some of the coherer signalling which is "supposed to occur by means of the waves in space." Mr. Marconi made an experiment yesterday, at my request, bearing on that point. The experiment was this: While a message was being received (that is to say, from East Goodwin lightship, 12 miles away), the earth connection of the receiver was completely removed, with the result that the message was not interrupted in the slightest degree. If any change at all was perceptible, it was that the signals were rather more powerful than before. In this case, if the signals were not due to Hertzian waves, what were they due to? I think the experiment is fairly conclusive. I am bound to say it surprised Mr. Marconi himself. The earth connection does not appear to be a necessity of the receiver. It *is* a necessity of the transmitter, and I fancy that the reason is, that it assists in conferring on the spark discharge that sharp, snapping character which indicates "oscillation." Although I confess to be a believer more in the possibilities of the Hertzian wave telegraphy, I admire as much as anybody the extraordinary beautiful apparatus which Mr. Evershed and Dr. Lodge have shown us here; but there is no question that, without detracting in any way from Mr. Marconi's share of the credit, the success of the Hertzian wave method is in no small measure due to the early labours of Dr Lodge in this field.

Mr. DANE SINCLAIR: This is a question which is highly interesting and instructive, and we have all enjoyed it very much. But there are two points I should like to draw attention to. The first is that the word "earth," or "ground," has been referred to as a common quantity—that is to say, as if the earth behaved in different places on its surface in a constant way. From a very long experience of using the earth in a practical way I find that it varies very much indeed. What is an infinite earth—as telegraphic men call it—in one place, is practically or nearly not earth at all in another place. I am quite sure that our scientific friends will excuse me if I point this out to them. I feel sure that the bottom of this subject will never be reached until it is fully realised that the earth in some places is an excellent conductor, and in other places nearly, or very nearly, an insulator. I think in practice it will be found, speaking broadly, that where you are about the level of the sea, with a sandy soil, it is almost impossible to get anything like a good earth. Where you get into a clay soil it is very rare not to get good earth. Another point that may be borne in mind is this—that to get an earth for two points that are distant 50, 60, or 100 miles is an easy matter; but to get an earth for a point that is only distant 20 yards, or half a mile, or a mile, is quite a different matter altogether. That I know myself from very long practice, and I think almost all telephone men would bear that point out with me. Mr. Sinclair.

There is just one question that I should like to ask Dr Lodge, as it is of some importance, and of very considerable interest to me; and that is, what I would call his stepping up of the telephone receiver from the first to the second and from the second to the third circuit, getting there a larger effect than he got in the first. I frankly admit I have not studied it very much, but if that process could be reversed, and you could get the stepping-up motion from the reverse way, then we should have a relay in telephonic work that would have an immense importance. Away from wireless telegraphy, there might be something taught to us here that would be of much practical importance. In experiments that I have made with it, you have

Mr. Sinclair. always to remember that, while we could step up, the other man from the other end has to speak backwards; and if you step his communication down as much as you step the first man's up, the final result is that you have lost something and gained nothing. But perhaps Dr. Lodge has considered it from this point of view. I have listened with great pleasure to both papers, and I cannot sit down without saying one word of admiration for that enormously sensitive relay which Mr. Evershed has shown us, although for me it is difficult to see how it could be very much more sensitive than an ordinary telephone receiver, which seems to respond to infinitely small currents wherever they are. There is one point that I think might be more strongly in the minds of experimenters on this question. If Dr. Lodge is working in the vicinity of Liverpool with his coils, causing great influences in the air around them, he will of course be affecting conductors that run all the way up to London; and if another man is listening in London for that effect, there are two ways in which he may get it—one by conduction through the earth, and another a way that he may not be thinking about at all, namely, by conductors passing through the air in Cheshire and going through the other counties and on arriving in London retransmitting it into the circuit of the listener. I can assure members that in practice it is not so easy to get free of all these elements, and to say with any degree of certainty that the result obtained is what it appears to be. Of course this discussion is a cheerful one, and I for one welcome it very much into this Society.

Mr. Brown. Mr. A. C. Brown: I should like to say a few words with regard to a very important part of the discussion which has been brought out both in Dr. Lodge's paper and Mr. Evershed's—that is, the frequency which it is best to use for transmitting by induction, and, possibly, earth conduction, in an ordinary wireless telegraph. I think that right through this discussion it has been well borne out that a low frequency is best for transmitting the energy, but that a high frequency is better for affecting the most sensitive receivers which we have—the telephone and the coherer. Up to the present I may say that

the frequency used has been usually the one best adapted to operate the receiver, without reference to the point as to whether or not it is the best frequency for transmitting the energy across the space. But it does not seem to have occurred to anyone that the advantages of both these frequencies may be combined: that is to say, that it may be possible to transmit the energy at the rate which is best adapted for allowing the energy to pass the space, in whatever way it passes—that is, a very low rate—and still retain the advantage of having the receiver affected by the frequency best adapted to operate that: in other words, to split up the received current. I was specifically, but no doubt unconsciously, referred to last time by Professor Fleming in regard to my bare wire cable patent, and I may say that this point principally explains the reason why Professor Fleming failed to get his signals when transmitting Hertzian waves through a bare wire submerged in water. He no doubt transmitted the waves at a very rapid rate with Hertz apparatus; but had he transmitted a slow current and split that current up at the receiving end, he could have very effectively operated either a telephone or a coherer by putting that coherer as a shunt for an electro-magnetic coil, and in series in either case with a rapid make-and-break applied at the receiving end. I think Mr. Evershed mentioned last time that when he was putting a current of 1 ampere through the primary circuit on the Lavernock-Flat Holm system he produced a difference of potential in the receiving circuit sufficient to make a telephone howl, but that the said telephone did not howl, because when the frequency was so raised as to make the telephone sound at all with a practicable note, the absorption (or whatever else the phenomena may be which acts to reduce the received current) became so great that the telephone did not get enough current to operate it. I say, Then let the telephone howl! or, of course, with the alternatives of reducing power, or of operating a much longer distance with the same power. My patent which Professor Fleming referred to (30123 of 1896) just brings out that point. If Mr. Preece will take his rapid make-and-break out of the primary circuit on the Lavernock and Flat Holm system and put it into the receiving Mr. Brown.

Mr. Brown. circuit—provided that the make-and-break does not produce or induce any current in the receiving circuit from its own action—I think he will find that he will get very much louder signals by earth conduction than he is now getting by induction with a rapid make-and-break in the primary circuit. If earth currents or the like give trouble, they can be neutralised by a small-capacity condenser in series with the telephone and silent make-and-break. I also wish to point out that the use of condensers to time circuits is also distinctly described in the same patent. I will read just one line: "A condenser is inserted in the circuit "at the receiving station, and its capacity adjusted so as to tune "or synchronise the time rate of the circuit to that of the "transmitting circuit." I find that this produces a very much greater effect than simply sending currents on the plain circuit without any capacity in it.

Also I should like to say one or two words with regard to the supposed method of selecting signals by means of synchronised circuits. From my experience I have found that this in practice cannot be done. You can increase the sensitiveness of the receiver by putting a capacity in the circuit, but you cannot so tune the circuit to a distant circuit as to cut out any intermediate current or impulse that may be generated in the district. A coherer will be affected by every current or impulse that comes along, provided that it is strong enough, and you cannot keep out foreign impulses from such circuits. Occasionally they come, whether they are wanted or not, and one has to put up with them. The only way, apparently, is to allow them all to come, but then to cut out those you do not want. In other words, while it is impossible so to attune two circuits as to prevent the receiving circuit from getting extraneous impulses, it is easily possible, by putting a tuned electro-magnetic vibrator in the *local* battery circuit that is operated at the receiving station, to cause that tuned vibrator to be actuated by impulses received from your own similarly tuned transmitter at a distant station, and not to be actuated by impulses affecting the receiver in any other rate, as when other signals are being transmitted in the vicinity.

nson. Mr. C. A. Stevenson [*communicated*]: Dr. Lodge's paper is

an important contribution to the subject of telegraphy by induction, especially as it deals with a simple method, namely, that of using a number of turns of wire in the form of a horizontal coil—a system which I first proposed and recommended for telegraphy through space.* On account of the simplicity of the coil system, it is probable that it will prove superior to the Lodge-Marconi system; and on account of its compactness it will, for most purposes, prove superior to the parallel-wire system so long advocated by Mr. Preece.† Mr. Stevenson.

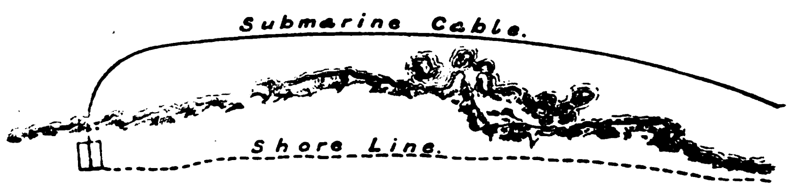
Dr. Lodge's main alteration on what I proposed is the addition of condensers, thereby getting results greater than he got by coils alone. It is the natural thing to try condensers; but, on my trying them, they did not, even with high frequency, give the advantage I had anticipated—probably due to want of proper tuning. I am glad, however, that Dr. Lodge has succeeded, thus going a step in advance of me. Dr. Lodge has not given us any of his experience, if he has any, of making the coils with soft iron cores. There can be no doubt of the enhancing effect of the core up to distances of 200 feet; a large electro-magnet, properly designed, with a suitable core, being a powerful instrument for the purpose.

It was not on account of the cost of carrying the induction farther that I did not carry the coil system farther, as Dr. Lodge suggests, but simply because there was no immediate call for doing so. A particular case had to be met at North Unst—a gap of half a mile had to be bridged—and the experiment I made at Edinburgh was intended to, and did, demonstrate the practicability of the method on a large and useful scale to the Commissioners of Northern Lighthouses; the Commissioners having been so impressed with the experiments I had previously shown them on a smaller scale, even through stone and mortar, that they sanctioned the cost of the experiments on the larger (which was full size) scale, viz., 2½ miles off. Financial reasons alone prevented the erection of this system to connect Muckle Flugga with the lighthouse station. It is well to remember that

* March 24th, 1892. See *Engineer*, vol. lxxiii.; *Royal Society of Edinburgh*, January 30th, 1893, and March 19th, 1894.

† See "Submarine Telegraph," by Bright (1898), p. 186.

Mr. Stevenson. in the experiments at Edinburgh a very small number of cells were purposely used; and it is well to compare the feeble currents then used with those used by Dr. Lodge, with their respective results. Theory and the author's formulæ give one the impression at first sight that a single wire is always best, the simple experiment of getting a greater effect at a distance by uncoiling a wire while a constant current is passing, supporting this impression; but the formulæ, if they are to be practical, ought to take into account a limited area for practical work, and a practical resistance and E.M.F., &c., and then the fact is disclosed that the coiling of wires (whether condensers be used with the coils or not) becomes an advantage for most practical work which the engineer will be called upon to deal with; and the dependence on mass of copper and the independence of number of turns ceases to be practical.



I cannot—nor apparently does Dr. Lodge—agree with Mr. Evershed that the coils should be identical in size and shape. Far from it; each case must be treated for size and configuration by itself. For instance, take the case of Muckle Flugga: my design was for a line two miles in length, with a coil at the end enclosing a larger area than the one on the rock, which was opened out to the maximum possible. Again, take the case of Sule Skerry and the Flannan Islands, on the north-west of Scotland, where telegraphy by induction would be of great value: it is impossible to make the coils of large diameter on these rocks, but the coil on shore should be of large dimensions; indeed, a single long wire with the ends earthed well away from the line would be, perhaps, the best arrangement. And again, for guarding a dangerous coast, a similar wire of many miles in length would be suitable for communicating its warning signal to vessels on board of which

were detectors, necessarily of small dimensions. This guarding of a dangerous stretch of coast appears to me to be the most important application of telegraphy by induction, and one to which I hope Dr. Lodge may give his valuable attention. There are two main systems, both of which I have tried. First, there is the principle of laying down a submarine cable along the line of coast (see diagram). Mr. Stevenson.

In this case the currents set up in the cable have to be sent only through the sheet of water to the vessel—say 20 fathoms, or 120 feet, or, if an electro-magnet be let down from the ship, only 4 or 5 fathoms. There are, however, certain objections to this system; for instance, the first cost of cable and maintenance of it are important considerations.

In the second system the erection of a pole line on shore, either along the stretch of coast, or in the form of a coil on a peninsula. The main difference from the last case is that the currents set up in the land line have to be sent several miles out to the ship in place of only a few yards; but the submarine cable is done away with, which is an important consideration. I have tried this system with two miles of pole line and a coil about a quarter of a mile off the line with perfect and never-failing success, and I see no reason why this system should not have been erected ere this. The cost is by no means prohibitive, and the boon it would be to vessels of the Navy and mercantile marine attempting to make, say, the south-west of Ireland would justify the expenditure, as our sound fog signals even of the best type guard but a small area—an area of six miles diameter at most—and that with no great certainty, owing to atmospheric causes. It is thus impossible for one sound signal to guard more than six miles of coast, and that imperfectly; whereas the submarine cable system or the shore line system could guard a long stretch, irrespective of atmospheric conditions. The influence of electric wiring on ships fitted with electric light fittings can easily be guarded against. The question is a national one, and I trust our Government will at least test the system, and not let Italy or any other nation step in in front of us.

Mr. Stevenson.

With special reference to Mr. Evershed's paper,* the system actually used at the North Sand lightship was quite useless as a magnetic induction apparatus, as, owing to the arrangement, there were *practically* no magnetic currents set up in the circuit. To ascribe failure to the screening effect of the water or sheathing is an error, as I have shown by trial on board ship on the Firth of Forth that any such loss is not a practical one.† I may mention that for anyone to suppose that a system which works in air must necessarily work if laid down in the sea is a mistake, as special arrangements are requisite for sea work.

Mr Evershed compares the method of building up the circuits on the parallel-wire system with the coil method proposed by me of having coils with their planes lying in the same plane, turning Mr. Preece's two vertical circuits into horizontal circuits, stating that for equal areas there is a loss of mutual induction, but rightly mentioning that "such "horizontal circuits possess compensating advantages which may "easily outweigh their drawbacks;" and this I think is specially the case with communication between ship and ship, or a lighthouse and the shore, where compactness is an essential; indeed, in my opinion, where inductive telegraphy is necessary or expedient, compactness at one end is an essential which entirely precludes the adoption of the parallel-wire system.

The vibrator indicator devised by Mr. Evershed in 1892 for Morse code signalling, and adapted in 1895 for induction, is an ingenious apparatus, and he deserves credit for it if it works well, as it is said to do on the installation in the British Channel.

I have made numerous trials of the parallel-wire system since the year 1891, and I have found—and other observers seem also to have found—that it is not practical to work the parallel-wire system more than three or four times the base, whereas by coils I have found it is possible to work many times the breadth of base. Thus, at the experiments in 1892, at the Isle of May lighthouse, I signalled 360 times the diameter of an electro-magnet or base

* The coil system was proposed by me in January, 1894, for lightships.

† *Nature*, December 31st, 1896; *Proceedings Royal Society of Edinburgh*, vol. xx., p. 200. There is no screening effect through brine.

with a De Meritens magneto-electric machine; again, at Murrayfield, I signalled four times the base with five dry cells; and I have in Edinburgh a coil with iron core 17 inches diameter, which, with one cell, can easily signal 25 times its diameter. Such effects are absolutely impossible with parallel wires. With reference to Mr. Preece's paper, we have in it mention that previous to 1885 he had made many laboratory experiments with coils of a number of turns, but these experiments were never published. These laboratory experiments of Mr. Preece's are to me new and interesting, emphasising as they do that the mere use of a number of turns will not give success, but that the coil system requires to be designed skilfully to ensure success. The experiments at Newcastle in 1885 were not made with a number of turns of wire. The first successful application of coils to distant signalling with a number of turns was made by me at Edinburgh, and described to the Royal Society of Edinburgh in 1894. These experiments, for the success of which I was solely responsible, were initiated by me and designed by me; and if Mr. Preece granted the use of his poles and his wire, and the advantage of the assistance of the Post Office staff, with a view to proving that my views were erroneous, all I have to say is that he was mistaken, for the experiments demonstrated the truth of the only view I had, viz., the applicability of coils to signal to North Unst Lighthouse. Mr. Preece's paper all through is against the coil system, but I shall leave my 1894 paper as answer. Mr. Stevenson.

Mr. CHARLES BRIGHT, F.R.S.E. [*communicated*]: My remarks have to do with the discussion which occurred on December 22nd. As regards the papers of Dr. Lodge and Mr. Evershed, I can only say that they call for very careful study, whilst Mr. Preece's record of some of the Post Office work must necessarily be one of great practical interest. Mr. Bright.

I am glad that General Webber has brought forward the claims of our esteemed Past-President, the late Mr. Willoughby Smith, as one of the most active pioneers in so-called wireless telegraphy. From the historical point of view, it may, however, be of some interest to trace the origin still further. As a result of somewhat close investigation, I find that the first really

Mr. Bright practical suggestion in the direction of inductive telegraphy occurred so far back as 1849. This emanated from Mr. J. W. Wilkins, a telegraph engineer of the earliest days. Mr. Wilkins proposed to establish electric communication between England and France—even before we were in any sort of communication with the Continent—by a system altogether innocent of cables. This method he described fairly fully in the course of a communication to the *Mining Journal* for March 28th of that year; and I venture to submit a copy, thinking that it might form an interesting appendix to the *Journal*. In reading this it must be remembered that 50 years have elapsed since its original appearance, and that the terms of to-day are a little more explicit than those then in vogue. For further particulars, I would draw attention to an article in the current number of the *Edinburgh Review*.

We must also bear in mind the work of Mr. W. F. Melhuish, a member of this Institution, who gave us an important paper in 1890, describing his wholly successful experiments in induction signalling across Indian rivers, following those of Mr. Johnston. In these Major Cardew's vibratory sounder was made use of.

I will pass on to the remarks of Mr. A. R. Sennett. With this gentleman I cordially agree in calling attention to the extreme importance of inductive telegraphy experiments in their practical application to the great national need of a complete and satisfactory system of telegraphic communication with lightships and lighthouses. Mr. Sennett refers in some detail to the method suggested by Mr. James Wimshurst, F.R.S., for putting a lightship into telegraphic touch with the shore, and it would certainly seem as though the Wimshurst system was worthy of a full trial, if it has not had it already. It should, however, be noted that though Mr. Wimshurst's device successfully overcomes one source of trouble in the ordinary continuous-cable system—that of kinks in the cable with the swing of the lightship—it does *not* overcome a still more serious difficulty—that of constant wear and tear on the bottom, due to rise and fall.

Another device, having similar properties, is the ingenious type of veering cable designed by Mr. F. C. Crawford, which is built

up in such a way that it is very difficult to make a kink in it. Mr. Bright. The main characteristic of its construction is that each sheathing wire is covered with india-rubber and tape; and, finally, there is an outer serving of india-rubber, which, besides ensuring for the cable the necessary degree of flexibility, also prevents the sheathing wires from slipping over one another when the cable is bent.

With regard to the system of Mr. Charles Stevenson, F.R.S.E., of the Northern Lighthouse Board (to which Mr. Sennett refers), though not adopted practically in England, it is but fair to state that it has now received a practical test of over two years in the United States, the lighthouse authorities there reporting it to be a complete success.* It was thought by some that an iron ship would damp the effect, but a trial with a large iron steamer on the sea shows that the damping, if any, was quite inconsiderable.

So far, my remarks have been directed more especially to lightship communication, but I will now venture to touch on the question of telegraphy between rock lighthouses and the adjacent mainland. The practical difficulties here are less formidable; and it is just a question whether the ordinary direct-cable system could not be turned to good account—provided that a complete mastery of the requirements be arrived at and provided for. If communication is to be successfully established in this way, there can be little doubt that the cable must be secured to the rock, if possible, and effectually embedded in it to a distance of some five fathoms below low-water mark, to save it from the force of the sea. Where the latter is impossible, a specially designed cable becomes necessary—*i.e.*, an ordinary type of telegraph cable (of quite moderate weight) ensheathed in a heavy, but flexible, chain armour. Such a cable, being fairly flexible, may be zigzagged about among the irregularities of the bottom, which may then be converted from a source of danger into an absolute protection. On the other hand, on a bottom free from irregularities the great weight of the chain will prevent any serious movement. Divers should be employed to see that the cable is properly laid,

* Report of the United States Lighthouse Board, 1895

Mr. Bright. and once this is done no further trouble should be experienced. As a further security, it has been proposed by Mr. M. H. Gray to employ two or more branch cables approaching the lighthouse on different sides. He would form a junction of the ends, with the main cable, by means of a heavy cast-iron joint box, placed at a position under water well clear from the action of the sea. The advantage gained by this would be the unlikelihood of two or more cables, well removed from each other, breaking at the same time.

Whilst fully appreciating Mr. Sennett's further remarks on the vital import of isochronism in connection with all instruments—electrical or otherwise—to be influenced by æther, or sound, waves, I should much like to know at what distances he was enabled to communicate effective signals by means of sound through water.

WILKINS'S "WIRELESS" TELEGRAPHY.

Copy of Letter appearing in "Mining Journal," March 28, 1849.

[*Submitted by* Mr. Charles Bright, F.R.S.E.]

"TELEGRAPH COMMUNICATION BETWEEN ENGLAND AND FRANCE.

"*To the Editor, &c.*

"Sir,—Allow me, through the medium of your valuable journal, to draw attention to a theory upon which a telegraphic communication may be made between England and France without wires; and as this mode of telegraphy seems to be the ultimatum of all wishes upon the subject, I do not hesitate to lay before your readers, for investigation, experiment, or actual use, this, my view of the subject.

"I take for certain (as experiments I have made have shown me) that when the positive and negative poles of a battery are dipped into or connected with any conductory medium, the electricity around the positive pole is positive, being diffused in radial lines, and the part around the negative pole is negative in radial lines converging towards it, supplying the electricity requisite for the decomposition of the substances composing the battery. This understood, it is evident that when a positive radial line sets out from the junction of the battery with the earth it makes its way to, or is attached by the nearest negative portion of *earth*, at last meeting the negative pole of the battery restoring the equilibrium. From this it appears that the first portion of electricity will pass in a straight line between the two poles, being the shortest between them, and the rays will then form curves between the

poles until, by reason of increasing distance, they are no longer influenced by one another, without a better medium of conduction be interposed in their circuit. Mr. Bright.

"It is natural to suppose that all parts of the earth are not of one uniform density; if so, then some parts are positive, and others negative. Then from this it is easy to see that some of the electricity flowing from the positive pole is the means of restoring equilibrium to negative portions of the earth—not necessarily rendered so by the negative pole of the battery; and also positive portions, for the same reason, rendered neutral or negative (see note No. 1 below).

"These rays of electricity may be collected in a certain quantity between the point whence they start, and where they are rendered neutral, and by the interposition of a metallic medium that shall offer less resistance than the water or earth —obviously the nearer the battery the greater the chance of collecting them. I do not anticipate the distance of 20 miles is at all too much (with the means we can use to compensate it) to collect a sufficient quantity of current to be useful for telegraphic purposes. Still, the quantity would be small, and with the present telegraphic instruments would not be detected at all. The current in the wire (of the instrument used) must be detected—not by its amount, but that it exists in *any quantity*, however small. If, then, electricity can be collected in France, simultaneously with a discharge from a battery in England, all that is required is, to find out what to do with it, so that it shall indicate its presence.

"I will now lay before you the arrangement I propose for carrying out this design.

"*No.* 1.—Upon one shore I propose to have a battery that shall discharge its electricity into the earth or sea, having a distance of some 5, 10, or perhaps 20 miles—as the case may be—between the poles.

"*No.* 2.—Let a similar length of wire be erected on the opposite coast, as near to, and as parallel with it as possible; having its ends dipping into the sea or earth.

"*No.* 3.—Within the above circuit have an instrument consisting of about 10 or 20, or more, square or round coils of finest wire, of best conductibility, suspended on points or otherwise, being part of circuit No. 2. Suspend this coil before or between the poles of an electro, or permanent magnet or magnets, and in either case any current passing through the coil will be indicated by its moving or shifting position (see note No. 2 below). *This* would then constitute the telegraph. It now would only depend upon the distance between the poles of the battery, in one case, and the ends of the circuit wire in the other, together with the lightness of the coils of wire used—having reference to their number—and the power of the magnet or magnets used to deflect it; although that would be easy of adjustment, and when once done, are certain to be of continuance—at all events, much more so than a submerged wire across the Channel.

"I hope some one will take up this suggestion, and carry it out practically, to a greater extent than my limited experiments have enabled me: for, of its truth, for long, as well as short distances, I am satisfied; and want of means only prevents me carrying it out at once. I venture to say what I have, on an experience in electricity of 10 years, and a practical acquaintance with electric telegraphs of near five years.

(Signed) "J. W. Wilkins.

Mr. Bright. "*No.* 1 *Note.*—The present electric telegraph is affected by what is *commonly* termed 'Auroras'—or a current of electricity passing along the wires—of course, entering, and passing through the coils used, thereby deflecting the needle. While inspector of telegraphy on the lines between Rugby and Newcastle, I have known that for hours at a time the telegraphs on particular sections of the road have been rendered useless from this deflection, often moving the needle from the perpendicular to either side, and back again, in a few minutes, giving evidence of different currents flowing in the same direction, or the same currents in different directions. As the telegraph wires are terminated at each end with a plate in the earth, these wires are thus portions of the earth itself, and currents flowing from place to place pass along them.

"*No.* 2 *Note.*—The coils under these circumstances would, if between the poles of the magnet, move from any position and lie parallel to the poles, and, if in front of the poles of the magnet, would move from side to side in a plane parallel to the ends of them.

"I claim these movements as design or invention of my own, as telegraphs, working with the *least* amount of electricity and resistance; and the movement depends not so much upon the current passing, as on the power of the magnet or magnets used."

Mr. Adams. Mr. A. J. S. ADAMS [*communicated*]: I had hoped—as an old inquirer into the mysteries of induction—to have spoken in discussion upon the interesting subject of wireless telegraphy, opened up by the papers of Dr. Oliver Lodge, and of Messrs. S. Evershed and W. H. Preece; but, as time at our meetings is at a premium, perhaps I may be permitted a communication.

My first point is that the world is anxious to know if, and when, wires are to be dispensed with in practical general telegraphy; and the second is, that the answer lies within a nut-shell, and has been rightly pronounced by Mr. Evershed as £ s. d.

The question may be put thus: If a disconnected telephone be held to the ear near a telegraph wire of any description—coated or uncoated by insulation—all that goes on upon that wire will be distinctly apparent in the telephone, by induction. From this it would almost seem that we have but to increase the circumference of the wire, and the electrical disturbances upon it, in order to widen indefinitely the distance over which those disturbances could be detected. And, roughly, this would be so, except for at least two far-reaching agencies of intervention—one, the sure and speedy transformation of inductive energy into heat,

on the way; the other, interference by forces similar in character to itself; and it is the fact that the subjugation of both will require an increase of metal and power in rapid ratio, which renders cost the prime factor. Nor is mere sensibility of receiving apparatus calculated to reduce the difficulty. Mr. Adams.

It has been already pointed out that the value of absorption has yet to be deduced. I am inclined to think its value will be a serious hindrance in the case of electro or magneto forces. But, be that as it may, there can be little question as to the seriousness of the effects of stray frequencies with earth conduction.

In illustration of this latter disturbance I would point to the destruction of the Greenwich Royal Observatory earth-current records by the inductive oscillations of the South London Electric Railway, some 3 miles distant; and to the further fact that, whereas the Greenwich circuits are some 2½ miles from earth to earth, I am able to trace, and to record by photographic means, the same disturbances in my garden at New Cross, about a mile and a half from the nearest point of that railway, with earths only 1 foot apart. With earths some 100 feet apart I have even recorded disturbances so far afield as Brockenhurst, in the New Forest; and indeed find that England, at least, is permeated by vibrations of one sort and another.

In view of these agencies, I think that Dr. Oliver Lodge's remark to the effect that where a wire may be extended between any two points for telegraphic purposes it should be done, fairly answers the question, so far as concerns the efforts of electrostatic, or magnetic, forces; and that the day of wireless telegraphy, in a wide sense, with those forces is a long way off. Where wireless telegraphy is a necessity, and cost is a secondary consideration, I am inclined to favour insulated circuits and low frequency.

Dr. Oliver Lodge: Mr. Granville asked about the case where I had said—apparently in my paper—that experiment was absurd. If I used those words, I wish to withdraw them. Experiment is never absurd. I only meant that I did not *expect* the earth to have any effect when one of the coils was perfectly insulated. He thinks that, even though only one of the coils is earthed, yet Dr. Lodge.

Dr. Lodge. earth conduction may possibly have some effect. I will not deny it. I do not know. I was very much interested in his admirable description, and the views that he gave us, of the Fastnet Rock. The Fastnet Rock and lighthouse have appealed to my imagination several times. I think it is a most interesting case; and some of the lighthouses in the North of Scotland are also most interesting, though they are dangerous and difficult places to go to. I do not want to go there particularly myself, yet I would very much like, if possible, to be of assistance in getting them connected up for signalling purposes. What the best system is remains to be seen. Each system probably has its due and proper place.

Major Brett says that he is sorry I have forsaken the Hertzian system, but I do not consider that I have at all forsaken the Hertzian wave system. I have experiments going on pretty frequently in connection with coherers, though not on a large scale. The reason I have not referred to it in this paper is because I was on a different subject; and, moreover, I had been told that Mr. Marconi was going to read a paper before this Institution later in the present session. Hence, I thought that any remarks about the Hertzian method had better not come now. Besides, Mr. Marconi has so much greater facilities than I have, that he has rather taken it out of my hands. He can make experiments now on a large scale, and does, and is doing, evidently, very good work. I am glad to hear from Major Brett about that work. I do not know anything about it at first hand myself. I seem to have said that the Lavernock case was the only one in practical work; I meant regular or commercial or remunerative work, as opposed to experimental stations; not, indeed, ordinary commercial work, but the dependable work required by military authorities at forts. I do not think that the Hertzian plan is yet used for such a purpose. With regard to the earth connection being of no use for the receiving system, and therefore the coherer being affected without the impulses coming through the earth, I should be the last to deny that a coherer-circuit can be affected directly by Hertzian waves. Certainly it can be affected direct by Hertzian waves. I know it can also be affected by

impulses which come from the earth. I was not sure which had the most effect in those experiments that we read about in the daily papers. Major Brett says that if you remove the earth wire the thing is improved. Then I do not see why you have an earth wire. But anyhow it is an interesting experiment, and perhaps more experiments in that direction will now be tried. Dr. Lodge.

Mr. Sinclair said instructively that earth-conduction was locally very various, and that it was more difficult to get earths for small distances than big distances. I do not think it is generally known to telegraphists—I have thought not—that the resistance of the earth is independent of distance, depending only on the size of the earth-plates and what they are bedded in. If you take two small electrodes and put them in the skin of a sphere, then, whether you put them close together or any distance apart, or right away at the Antipodes, the resistance is the same theoretically. Hence, of course, when you have a line 1,000 miles long, the earth resistance is small in proportion; whereas, if the line is only a few miles, the earth resistance feels big; but it may be the very same earth resistance all the time, if the circumstances of the plates and the embedding of them are the same, and if you do not bring them too near together so that the size of the plates is an important fraction of their distance apart. With small plates at a reasonable distance apart the resistance is independent of the distance. To diminish the resistance you must use bigger plates.

Mr. Evershed.
Mr. EVERSHED, in reply, said: The hour is so late that I must confine myself to a series of emphatic statements somewhat after the manner of Dr. Lodge. Dr. Lodge began by expressing some surprise that in making one correction I had not gone on to make a second. The reason was that I had no second correction to make. Dr. Lodge objects to the statement made in my paper that the power available in the secondary circuit when working a telephone at 400 periods per second is over 600 times as great as that available for the relay which I showed at the last meeting working at a frequency of 16 periods per second. If that statement were not qualified it would be absurd; but if you look at equation (4) in my paper, you will find it distinctly stated that the equation refers to that condition in which the E.M.F. and

Mr. Evershed.

the current are brought into step—that is to say, they are in the same phase. My paper was not concerned with the manner in which the current and E.M.F. were to be brought into step. Dr. Lodge himself has brought forward one means of doing it by condensers. There is another method which I believe may be brought into use, but it is still in embryo, and I will not refer to it now. Let me make it quite clear that that statement with regard to the power does not relate to the Lavernock circuit in any way whatever. It is not in that part of the paper which relates to the Lavernock and Flat Holm experiments. It is simply a plain statement of the truth that when you have either no self-induction, or you have neutralised any self-induction there may be and brought the E.M.F. into the same phase as the current, the available power (with the same impressed E.M.F. in the two circuits) will be 625 times as great at 400 periods per second as it is at 16 periods. I am very glad that Dr. Lodge has given us some particulars of the power used in his telephone. Unfortunately, he has only given us the $C^2 R$ losses, or, rather, he has given us the impressed E.M.F. multiplied by what he thinks is the current going through his telephone; but he calculates that current by omitting possibly the most important factor, viz., the back E.M.F. due to the motion of the diaphragm. He says that the mechanical work done in beating the air by means of the diaphragm and overcoming the viscosity of the diaphragm, and so on, is negligible compared with $C^2 R$. Very likely that may be so. If it is, I can only say Dr. Lodge has not taken the best advantage of his improved telephone. What he has to do is to raise the E.M.F. of the telephone either by rewinding it or by raising its field until he makes its back E.M.F. equal to one-half the impressed E.M.F. He will then get the maximum "activity" (as defined by Lord Kelvin) out of his telephone. At present he is not doing himself justice.

Dr. Lodge asked several questions about the North Sand Head lightship. I may, in reply, say that the vessel is built of iron; and I therefore arranged the ship coil outside the ship, in order to avoid, as far as possible, the screening effect of the conductivity of the iron hull, and secure what little advantage there might be

in having an iron core within the coil. The cable was, necessarily, armoured. I felt perfectly confident that the armouring of the cable must necessarily cut off all induction at a frequency of 400 per second. I measured the resistance of the armouring, and calculated the damping effect of it, and I found that at 16 periods per second (the frequency at which I worked my relay) the absorption was 10 per cent. I did not mind about 10 per cent. At the last meeting I gave you the total absorption; and of that total absorption, at 16 periods a second, only 10 per cent. was due to the armouring on the cable. Mr. Granville has shown us to-night a photograph of a lighthouse. Well, a lightship is very much like a lighthouse, with the rock foundation left out; and you will readily understand that, if you were to lay an unarmoured cable on the bottom of the sea, it would not last a week in such a place as the Channel, where the scour is very great; so we were obliged to adopt a very disadvantageous course, and endeavour to signal with armoured cable. Mr. Evershed.

Dr. Lodge referred to some experiments he made on absorption. I have made a great many experiments of that kind myself, and I must confess they are entirely fallacious. When you reduce the scale of your experiments (reduce the size of your circuits)—as Dr. Lodge will readily admit when it is pointed out to him—you must at the same time decrease the specific resistance of your absorbent medium. If it is desired to ascertain the absorbent effect of a sheet of sea-water of specific resistance ρ_1, by means of small-scale experiments with a model coil and model conducting sheet $\frac{1}{n}$th full size, the material chosen to represent the sea must have a specific resistance

$$\rho_2 = \frac{1}{n^2} \rho_1.$$

For example, I made a model of the light-vessel communication, in which the ring of cable to be laid on the bottom of the sea was represented by a coil of wire about 1 foot in diameter, and the absorbing sheet used to imitate the effect of the 60-feet depth of sea-water required to be about $\frac{3}{8}$ths of an inch thick; that is to say, the model was 1-1,900th of full size. Hence $n = 1{,}900$, and

Mr. Evershed. the specific resistance of sea-water being about 20 ohms (per c.c.), it follows from the above equation that the conducting sheet used in the model must have a specific resistance

$$\rho_2 = \frac{20}{1{,}900^2} = 5{\cdot}5 \text{ microhms.}$$

I was able to obtain a large sheet (about 8 feet × 3 feet × $\frac{3}{8}$ inch) of rolled zinc of almost exactly this resistance—by careful measurement its specific resistance was found to be 5·7 microhms—and the observed absorption at 16 $\sim$ per second was 38 per cent Hence I should expect to find that a sheet of sea-water 1,900 times as thick as the zinc sheet would absorb the same proportion. Now I have calculated the absorption for that sheet of zinc by Mr. Whitehead's formula—which only, by the way, professes to be approximate—and it amounts to 27 per cent. That appears to me to settle the question raised by Dr. Lodge as to whether Mr. Whitehead was right and Maxwell wrong, or *vice versâ*. Because, if I understood Dr. Lodge aright, the discrepancy was in the manner in which $2\,\pi$ F entered into the equations. Mr. Whitehead's formula is,

$$\text{H with absorbent medium} = \text{H without such medium} \times e^{-\left(\frac{2\,\pi\,\mu\,p}{\rho}\right)^{\frac{1}{2}} t};$$

and it is clear that, if we put p in the denominator instead of the numerator (as I understand Maxwell tells us to do), we shall only get a result that bears no relation whatever to my observed absorption; whereas Mr. Whitehead's formula does, at any rate, give a result of the right order of magnitude.

Mr. Sennett made some very interesting remarks. They did not relate very much to induction telegraphy; but he mentioned Mr. Wimshurst's ingenious scheme for communicating with a lightship by means of an induction coil, the primary and secondary coils being on the two halves of a swivel. Mr. Sennett wondered why such a thing had never been tried. I can only refer you once more to Mr. Granville's realistic photograph of the Fastnet lighthouse; that gives you the clue to the whole matter. No electrical apparatus of any kind whatever that has to be put into the sea in a place like the Channel or the Fastnet Rock would last for more than a few days.

I will just refer to one other matter. Dr. Lodge, since the last

meeting, has apparently abandoned horizontal coils in favour of coils in a vertical position. A system of vertical coils on ships is one which ought to be worked out, but the ingenious manner in which Dr. Lodge suggested that a ship would be able to ascertain her position is one which is rather liable to break down at the critical moment. Two ships having this apparatus on board would be able to hear each other as long as they were not in danger of running into one another, but the moment they pointed towards each other their means of communication would cease; so that when you did not hear a signal you might expect a collision. Mr. Evershed.

Major Brett advocates Hertzian telegraphy. Certainly the simplicity and portability of the apparatus for Hertzian telegraphy are an immense advantage. What the cost of signalling by Hertzian waves may be no one has ever told us. I was hoping that Major Brett would have told us what the power used was, but that appears to be the last thing experimenters ever think about.

Mr. Brown referred to the experiments which I made on leakage at Lavernock, but I think he misunderstood the point. He appeared to imagine that it was absorption which was causing the difference in the leakage, or difference in the received current, between no periods per second—a continuous current, that is to to say—and 16 periods per second. That is not so. It was the difference in the true leakage current, and the extraordinary thing was that the leakage should fall off so enormously between a continuous current and a current alternating at a frequency of 16 periods per second. My point was this: If leakage falls off so enormously between continuous current and 16 periods, it may fall off to such a very large extent when you get to 400 periods per second that the resulting signals received in the telephone at that frequency may be due entirely to induction.

The CHAIRMAN: We have had an exceedingly interesting discussion, and your cheers have forestalled what I was about to say, namely, that we should give our hearty thanks to the readers of these three papers, Dr. Lodge, Mr. Evershed, and last, but not least, to our Past-President Mr. Preece. Our thanks are certainly due to all three of them. Professor Thompson.

Adjourned to January 26th.

SPECIAL RESOLUTION

OF

THE INSTITUTION OF ELECTRICAL ENGINEERS.

Passed 3*rd November*, 1898. *Confirmed* 18*th November*, 1898.

At a Special General Meeting of the Members of the above-named Institution, duly convened, and held at Victoria Mansions, 28, Victoria Street, London, S.W., on Thursday, the 3rd day of November, 1898, the subjoined Resolution was duly passed; and at a subsequent Special General Meeting of the Members of the said Institution, also duly convened, and held at the same place on Friday, the 18th day of November, 1898, the said Special Resolution was duly confirmed:—

"That the Articles of Association contained in the "printed document submitted to the Meeting, and, for "the purpose of identification, subscribed by the "Chairman thereof, be, and the same are hereby, "approved; and that such Articles of Association be. "and they are hereby, adopted as the Articles of "Association of the Institution; and that Article Number "One do come into operation forthwith, and that the "remaining Articles, numbered Two to Eighty, both "inclusive, with the Schedules thereto, do come into "operation on and from the First day of January, 1899."

By order of the Council,

W. G. McMILLAN,

Secretary.

ARTICLES OF ASSOCIATION OF THE
INSTITUTION OF ELECTRICAL ENGINEERS.

1. Save as hereinafter expressed to the contrary, the Articles of Association of the Institution of Electrical Engineers, as the same now exist, will remain in force up to, and including, the 31st day of December, 1898. Provided that the existing Articles* relating to the election of Council and Officers, viz.: Articles 38 to 43, both inclusive, and Article 55, are hereby cancelled; and provided further that all existing Officers of the Institution shall remain in office until the Annual General Meeting to be held in the year 1899; and provided also that the Annual Statement of Accounts under existing Article numbered 50 may be made up to any day subsequent to the 30th day of September, 1898, as the Council may prescribe.

2. On and after the 1st day of January, 1898, the said existing Articles shall cease to have any prospective operation, save for the purpose of enforcing any then outstanding obligations, or exercising any rights and remedies which the Institution shall then be entitled to enforce or exercise; and on and after the 1st day of January, 1899, the following Articles, with the Schedules and Footnotes thereto, shall be substituted for the existing Articles, numbered 1 to 71, both inclusive, as the same have been amended from time to time, and for the Schedules and Footnotes to those Articles, except so far as the same are hereinbefore cancelled or modified.

Subject as provided in Article 6, the terms used in these Articles are intended to have the same meanings as they have when used in the Companies Acts for the time being in force; and words implying the singular number are intended to include the plural number, and *vice versà*.

3. For the purposes of Registration the number of Members of the Institution is unlimited.

* The expression "existing Articles," and any equivalent expression in the present Articles, means the Articles adopted on the Incorporation of the "Society of Telegraph-Engineers and Electricians," as altered from time to time by Special Resolution.

MEMBERSHIP.

4. The Institution shall consist of Honorary Members, Members, Associate Members, Foreign Members, Associates, and Students.

5. On and after the 1st day of January, 1899, then existing Honorary Members shall continue to be Honorary Members, then existing Members shall continue to be Members, then existing Associates shall continue to be Associates, then existing Foreign Members shall continue to be Foreign Members, then existing Students shall continue to be Students, subject to the obligations attaching to such various classes, and there shall be a new class of Members, to be styled "Associate Members."

The different classes of Members referred to as existing on the said 1st day of January, 1899, and such other persons as shall be admitted, in accordance with these Articles, and none others, shall be or become members of the Institution (either as Honorary Members, Members, Associate Members, Foreign Members, Associates, or Students, as the case may be), and be entered on the Register as such.

6. No Honorary Member, Associate Member, Foreign Member, Associate, or Student shall, by reason of being legally a member of the Institution, within the meaning of the Joint-Stock Companies Acts, be entitled to any privileges other than those which, by these Articles, attach to the specific class of Members of the Institution to which he belongs, and wherever the term "Member" is hereinafter used without qualification, it shall be taken to exclude Honorary Members, Associate Members, Foreign Members, Associates, and Students.

7. The Institution may admit such other persons as may be hereafter qualified and elected in that behalf as Honorary Members, Members, Associate Members, Foreign Members. Associates, and Students respectively, but such persons shall sign the form G in that behalf contained in the Schedule hereto, or such form to the like effect as may from time to time be authorised by the Council.

8. The rights and privileges of every Honorary Member, Member, Associate Member, Foreign Member, Associate, and Student shall be personal to himself, and shall not be transferable or transmissible by his own act, or by operation of law.

ABBREVIATED TITLES.

9. The authorised abbreviations indicating the class in the Institution to which any Honorary Member, Member, Associate Member, Associate, or Student belongs shall be as follows:—For an Honorary Member, *Hon.* M.I.E.E.; for a Member, M.I.E.E.; for an Associate Member, A.M.I.E.E.; for an Associate, A.I.E.E.; and for a Student, *Student* I.E.E.

DIPLOMAS.

10. Subject to such regulations as the Council may from time to time prescribe, the Council may issue to any Member or Associate Member a certificate showing the class to which he belongs. Every such certificate shall be according to the form F in the Schedule, or such modification thereof as may from time to time be approved by the Council, and shall remain the property of, and shall on demand be returned to, the Institution.

QUALIFICATION OF MEMBERS, ETC.

11. HONORARY MEMBERS.—Honorary Members shall be distinguished persons who are intimately connected with Electrical Science or Electrical Engineering, and whom the Institution especially desires to honour for exceptionally important services in connection therewith.

12. MEMBERS.—Every new Member (*i.e.*, Members not on the Register as Members on the 31st December, 1898) shall, whether admitted by election or transfer, be at least 25 years of age, and come within one of the following descriptions:—

(*a*) He shall have been educated as an Electrical Engineer or Electrician in a manner which shall satisfy the Council, and either (1) shall have had

subsequent employment in the application of electricity for at least five years in situations of superior responsibility, and shall be actually engaged in such a situation at the time of his application for election or transfer, or (2) shall be in practice, and shall have practised on his own account in the profession of an Electrical Engineer or Electrician for at least five years, and shall have acquired sufficient eminence in the same; or

(*b*) being, either in a position of superior responsibility, or in practice on his own account, in the profession of an Electrical Engineer or Electrician, he shall have held such responsible position or positions, or shall have so practised on his own account that he has acquired sufficient eminence in the said profession, during a period or periods which, singly or in the aggregate, shall amount to seven years; or

(*c*) being an Associate Member, he shall have gained the senior premium in any year for a paper read at an Ordinary General Meeting of the Institution: or

(*d*) he shall be so prominently associated with the objects of the Institution that the Council consider his admission to Membership would conduce to its interests.

13. Associate Members.—Every Associate Member shall be either an Electrical Engineer or Electrician, and shall be at least 25 years of age, and, whether admitted by election or by transfer, shall come within one of the following descriptions:—

(*a*) He shall have been on the Register as an Associate on the 31st of December, 1898; or

(*b*) he shall have been educated as an Electrical Engineer or Electrician in a manner which shall satisfy the Council, and either (1) he shall have had subsequent employment for at least two years in a responsible situation as an Electrical Engineer or Electrician, and shall be actually engaged in such

a situation at the time of his application for election or transfer; (2) he shall have been engaged for at least five years in one of the branches of Electrical Engineering, and shall be actually so engaged at the time of his application, and shall afford satisfactory proof to the Council of his fitness for election; or (3), being an Associate, he shall have gained a premium in any year for a paper read at an Ordinary General Meeting of the Institution.

14. FOREIGN MEMBERS.—Foreign Members shall be *foreigners* residing abroad who are eminent in Electrical Science or Electrical Engineering, and whose admission to the privileges of Membership the Council consider would conduce to the interests of the Institution.

15. ASSOCIATES.—Associates shall be persons, more than 21 years of age, who are interested in, or connected with, Electrical Science or Engineering, or who are so associated with the application of Electricity that the Council consider their admission as Associates would conduce to the interests of the Institution.

16. STUDENTS.—Students shall be persons of any age serving pupilage to an Electrical Engineer or Electrician, or who are studying Electrical Science at one of the Universities, Public Colleges, Technical Institutions, or Government Schools, or who otherwise satisfy the Council that there are special circumstances which, in the opinion of the Council, entitle them to admission. No person shall remain in the Class of Students for more than three years, unless, at the end of that period, he shall not have attained the age of 22, in which case he shall cease to be a Student on attaining the said age.

ELECTION OF MEMBERS, ETC.

17. HONORARY MEMBERS shall be elected by the Council, and notice of such election shall be given at the next General Meeting of the Institution, but not more than one Honorary Member shall be elected in any one year.

18. Except as hereinafter provided, every candidate for election into the Institution, otherwise than as an Honorary Member or a Student, shall be duly proposed and seconded, in writing and from personal knowledge, by a Member; and his candidature shall be further supported, in writing, as follows:—

If he be proposed for election to the class of Members or of Foreign Members, by three Members or by two Members and two Associate Members; or,

if he be proposed for election to the class of Associate Members, by two Members or by one Member and two Associate Members; or,

if he be proposed for election to the class of Associates, by two Members or by three Associate Members or Associates.

The Secretary shall thereupon submit the application of the candidate to the Council to be considered, and if it be approved by them, it shall be brought before the next General Meeting of the Institution for approval. But in the event of a candidate resident abroad not being personally known to a sufficient number of Members, Associate Members, or Associates, to enable him to satisfy the foregoing conditions of proposal, if such candidate be nominated by the Local Honorary Secretary of the Country or Colony in which he resides, and if sufficient evidence be produced to satisfy the Council as to the fitness of such candidate for election to any class, the Council may propose his election to such class, and no further support will then be necessary; but the proposal form of the said candidate shall be signed by the Chairman of the meeting of Council at which his candidature was accepted, and his application shall be brought before the next General Meeting of the Institution for approval.

Every candidate for election into the Institution as a Student shall be duly proposed, in writing and from personal knowledge, by one Member or Associate Member. The Secretary shall thereupon submit his application to the Council, and if it be approved by them, it shall be brought before the next General Meeting of the Institution for approval.

19. Ballots shall take place at the Ordinary General Meetings of the Institution, but, except at the last meeting of the session, no candidate for admission shall be balloted for at the same meeting at which his application is announced. Members, Associate Members, and Associates, only, have the right of voting for admission of candidates.

20. The proportion of votes necessary for the election of any candidate shall be at least two-thirds of the total number of votes given.

21. After the Ballot the Secretary shall inform the candidate of the result thereof upon the form C, but no record shall be taken in the Minutes of the non-election of any person balloted for.

22. In the case of the non-election of a candidate, a second Ballot shall be granted, if demanded, by a written notice addressed and sent to the Secretary within twenty-eight days by any five persons having a right to vote, and such Ballot shall take place at the next General Meeting at which candidates are balloted for.

23. The Council shall decide upon the application for transferring any candidate from one class to another, but, except as hereinafter provided, every candidate for transfer to any class shall be duly nominated for such transfer, in writing and from personal knowledge, by two Members; and his candidature shall be supported, in writing, by as many Members, Associate Members, or Associates as would have been required by Article 18 had he been a candidate for direct election to the class to which he seeks to be transferred. [Excepting that an Associate who shall have been on the Register as an Associate on the 31st December, 1898, and who can prove to the satisfaction of the Council that he is duly qualified according to the preamble of Article 13, may be transferred to the class of Associate Members on receipt of his personal application alone, in writing.] But in the event of a candidate for transfer resident abroad not being personally known to a sufficient number of Members, Associate Members, or Associates, to enable

him to satisfy the prescribed conditions of nomination, if such candidate can produce sufficient evidence to satisfy the Council as to his fitness for admission to the class to which he seeks to be transferred, the Council may accept, without further support, the nomination of the Local Honorary Secretary of the Country or Colony in which such candidate resides.

24. Except as otherwise provided in Articles 18 and 23, the proposal for admission to the Institution, otherwise than as an Honorary Member, or for transferring any person from one class to another class, shall be according to forms A, AA, or B in the Schedule, or such modification thereof as may from time to time be approved by the Council. These forms, being subscribed by the number of Members, Associate Members, and Associates prescribed in Articles 18 and 23, and delivered to the Secretary, shall be submitted to the Council for consideration.

CONTRIBUTION OF MEMBERS, Etc., TO THE INSTITUTION.

25. Honorary Members shall not be required to pay any contributions.

26. Every Member shall pay to the Institution an entrance fee of Three Guineas on his election. Every Associate Member shall pay an entrance fee of Two Guineas. Every Foreign Member and every Associate shall pay an entrance fee of One Guinea (21s.).

Every Student transferred to the class of Associates shall pay an entrance fee of One Guinea. Every Associate transferred to the class of Associate Members, if he has paid an entrance fee of One Pound Ten Shillings on admission to the class of Associates, shall pay a further entrance fee of Twelve Shillings; or, if he has paid an entrance fee of One Guinea on admission to the class of Associates, he shall pay a further entrance fee of One Guinea. Every Associate Member transferred to the class of Members shall pay a further entrance fee of One Guinea.

27. Except as hereinafter provided, every Member elected before the 31st December, 1891, shall contribute annually to the Institution the sum of Two Guineas; or, if elected after that date and before the 31st December, 1898, the sum of Three Pounds; or, if elected after the last-named date, the sum of Three Guineas.

Every Associate Member shall contribute annually the sum of Two Guineas.

Every Foreign Member elected before the 31st December, 1898, shall contribute annually the sum of One Pound; or, if elected after the said date, the sum of One Guinea.

Every Associate elected before the 31st December, 1891, shall contribute annually the sum of One Guinea; or, if elected after that date and before the 31st December, 1898, the sum of One Pound Ten Shillings; or, if after the last-named date, the sum of One Guinea and a Half.

Every Student elected before the 31st December, 1898, shall contribute annually the sum of Half a Guinea; or, if elected after the said date, the sum of One Guinea.

Any Member residing abroad,* or absent from the United Kingdom* of Great Britain and Ireland, the Isle of Man, and the Channel Islands, for nine months in any year, and giving previous notice in writing to the Secretary of his intended absence, shall, during the period of his absence, if elected a Member before the 31st December, 1898, contribute annually the sum of One Pound, or, if elected after the said date, the sum of Two Guineas. Any Associate Member so residing or absent abroad, and giving previous notice of his absence as above required, shall, during the period of his absence, contribute annually the sum of One Guinea. Any Associate so residing or absent abroad, and giving previous notice of his absence, as above required, shall, during the period of his absence, if elected an Associate before the 31st December, 1898, contribute

* Wherever in these Articles of Association the term "United Kingdom" is hereinafter used, it is to be understood as including the United Kingdom of Great Britain and Ireland, the Isle of Man, and the Channel Islands, and the term "abroad" is to be understood as including any place situate beyond these limits.

annually the sum of One Pound, or, if elected after the said date, the sum of One Guinea.

28. Any Member, Associate Member, Foreign Member, or Associate may, after the 31st December, 1898, compound for his annual subscription by the payment to the Institution in one sum of Forty Guineas. Provided that if any such Member, Associate Member, Foreign Member, or Associate has, as a Member, Associate Member, Foreign Member, or Associate, paid to the Institution or its predecessors* more than ten annual subscriptions, such sum of Forty Guineas shall be reduced by the deduction of One Guinea for every year after the tenth year during which he shall have paid such annual subscription.

Any Foreign Member, or any Member who, as residing abroad, shall have already compounded by payment of Ten Pounds, shall, if he come to reside in the United Kingdom, and if elected before the 31st December, 1891, pay, either an additional composition of Eleven Pounds, or an annual subscription of One Guinea. If elected after the said date, and before the 31st December, 1898, he shall pay either an additional composition of Fifteen Pounds or an annual subscription of Two Pounds. Any Associate elected after the 31st December, 1891, and before the 31st December, 1898, who, as residing abroad, shall have already compounded by the payment of Ten Pounds, shall, if he come to reside in the United Kingdom pay, either an additional composition of Two Pounds Ten Shillings, or an annual subscription of Ten Shillings. Provided that any existing Member, Foreign Member, or Associate may, until the date of the Annual General Meeting in 1899, compound upon the terms specified in Article 25 of the existing Articles of Association.

All such compositions shall be invested, and the interest alone shall be appropriated to the current expenditure of the Institution.

* Wherever in these Articles of Association the word "predecessors" is used, it is to be understood to mean, and to include, the Society called by the name of the Society of Telegraph-Engineers and that called by the name of the Society of Telegraph-Engineers and Electricians.

29. A Foreign Member who has not compounded shall, if he come to reside in the United Kingdom, pay an annual subscription of Two Guineas if he was elected before the 31st December, 1891, or of Three Pounds if he was elected after the said date and before the 31st December, 1898, or of Three Guineas if elected after the last-named date.

30. An Associate resident in the United Kingdom who has compounded by payment to the Institution or its predecessors of Ten Pounds or of Twelve Pounds Ten Shillings shall, if transferred to the class of Members before the 31st December, 1891, pay an annual subscription of One Guinea; if transferred after the said date and before the 31st December, 1898, he shall pay an annual subscription of Two Pounds; or, if transferred after the last-named date, either directly to the said class of Members or after passing through the class of Associate Members, he shall pay an annual subscription of Two Guineas; if transferred to the class of Associate Members, he shall pay an annual subscription of One Guinea.

An Associate resident abroad or absent from the United Kingdom for nine months in any year, and giving previous notice in writing to the Secretary of his intended absence, shall, during the period of his absence, if he has compounded as an Associate by payment of Ten Pounds or of Twelve Pounds Ten Shillings, and if he is transferred to the class of Members, either directly or through the class of Associate Members, after the 31st December, 1898, pay an annual subscription of One Guinea.

Provided that any such Associate may compound for his annual subscription by payment of an additional composition equal to the difference between the composition he has already paid and the composition that he would have to pay if he had not compounded as an Associate.

A Student, or an Associate, Foreign Member, or Associate Member, who has not compounded, shall, if transferred to a higher class, pay the same annual subscription as if he had been elected to such higher class on the day upon which he was transferred thereto.

31. Any Member, Associate, or Foreign Member who has compounded by payment to the predecessors of the Institution, shall have the same rights and privileges as if he had compounded by payment to the Institution.

32. All Members, Associate Members, Foreign Members, Associates, and Students, hereafter elected, shall pay the annual subscription for the year in which they are elected, without reference to the period of the year at which their election takes place; but they shall be entitled to receive a copy of all numbers of the Journal containing the proceedings of that year, and other publications of the Institution which may have been issued during that year.

33. Every Member, Associate Member, Associate, or Student absent for nine months in the year, and every Foreign Member, shall be liable, if residing where the postal rate is excessive, to pay, in addition to his annual subscription, a sum to be fixed by the Council, not exceeding five shillings per annum, to defray the expense of posting the Journal and other publications.

34. The annual subscriptions shall be payable in advance on the 1st of January in each year.

35. Every individual admitted into the Institution, whether as Member, Associate Member, Foreign Member, Associate, or Student, shall be considered as belonging thereto, and, as such, liable to the payment of his annual subscriptions and other payments, until his name shall have been removed by the Institution from its Register, or until he shall have signified to the Secretary, in writing, his desire to resign, having previously paid all arrears.

36. No Member, Associate Member, Foreign Member, Associate, or Student whose contribution is six months in arrear shall be entitled to attend or take part in the meetings of the Institution, nor to receive the Institution's printed papers, nor shall any such Member, Associate Member, or Associate be entitled to vote. Any Member, Associate Member, Foreign Member, Associate, or Student, whose contribution is two years in arrear, shall be deemed to have forfeited his claim to

membership, and his name may be removed from the Register by order of the Council; but he shall nevertheless continue liable to pay the arrears of subscription due at the time of his name being so removed.

37. The Council may, at their discretion, reduce or remit the annual subscription, or the arrears of annual subscription, of any Member, Associate Member, Foreign Member, or Associate, who shall have been unable to continue the payment of the annual subscription prescribed by these Articles.

EXPULSION.

38. In case the expulsion of any individual shall be judged expedient by ten or more Members, Associate Members, or Associates, and they think fit to draw up and sign a proposal requiring such expulsion, the same being delivered to the Secretary, shall be by him laid before the Council for consideration. If the Council, after due inquiry, do not find reason to concur in the proposal, no entry thereof shall be made in the Minutes, nor shall any public discussion thereon be permitted; but, if the Council do find good reason for the proposed expulsion, they shall direct the Secretary to address a letter, according to the form D in the Schedule, to the person proposed to be expelled, advising him to withdraw from the Institution. If that advice be followed, no entry on the Minutes, nor any public discussion on the subject, shall be permitted; but if that advice be not followed, nor a satisfactory explanation given, the Council shall call a Special General Meeting of Members, Associate Members, and Associates for the purpose of deciding on the question of expulsion; and if a majority of the persons present at such Special General Meeting, provided the number so present be not less than twenty, vote that such individual be expelled, the Chairman of that Meeting shall declare the same accordingly, and the Secretary shall communicate the same to the individual according to the form E in the Schedule.

OFFICERS.

39. The Officers of the Institution shall be a President, four Vice-Presidents, fifteen Ordinary Members of Council, three

Associate Members of Council, to be chosen from the classes of Associate Members and Associates, the Chairman or President of each Local Section of the Institution as hereinafter referred to, two Auditors of Accounts, an Honorary Treasurer, an Honorary Solicitor, an Editor of the Publications of the Institution, a Secretary, and a Librarian.

40. All the Officers, except Auditors, Solicitors, Secretary, and Librarian, shall be elected from the class of Members, Associate Members, or Associates, and all offices shall be honorary, except those of Editor of the Publications of the Institution, Secretary, and Librarian.

ELECTION OF COUNCIL AND OFFICERS.

41. The Council shall consist of the President, all past Presidents (including past Presidents of the predecessors of the Institution), four Vice-Presidents, fifteen Members, three Associate Members or Associates, the Chairman or President of each Local Section of the Institution, the Honorary Treasurer; and all these (except the three Associate Members or Associates) shall be chosen from the class of Members.

42. The following Members of Council—namely, the President, one Vice-President, the Honorary Treasurer, five Members, and one Associate Member or Associate—shall be elected annually by ballot. The President, one Vice-President, the Honorary Treasurer, five Members, and one Associate Member or Associate, and every Chairman or President of a Local Section shall retire annually, and, save as hereinafter provided, be immediately eligible for re-election. Provided that no President, Vice-President, Member of Council, Associate Member of Council, or Chairman or President of a Local Section shall hold office in the same capacity for more than three years in succession, that is to say, three periods between successive Annual General Meetings. Provided also that every past President and every Chairman or President of a Local Section shall be eligible for election as an Ordinary Member of Council, but that whilst holding office as an Ordinary Member he shall cease to be an *ex-officio* Member.

43. The Auditors shall be elected annually by ballot, but shall be eligible for re-election on the expiration of their year of office.

44. Casual vacancies in the Council during the year may be filled up by the Council, and the name of each Member, Associate Member, or Associate selected shall be announced at the next Meeting of the Institution, but the Member, Associate Member, or Associate so chosen shall retain his office so long only as the vacating Member of Council would in ordinary course have retained the same.

45. The Council shall, previous to the Annual General Meeting in each year, prepare a list of Members whom they propose as suitable for the offices of President, Vice-Presidents, and Treasurer for the ensuing year. The list shall also contain the names of a sufficient number of Members and Associate Members or Associates whom the Council nominate as fitted to become Members of Council, to fill the vacancies in the Council other than those caused by the retirement of the Chairmen or Presidents of Local Sections. At an Ordinary General Meeting held not less than twenty-eight days before the Annual General Meeting, the Chairman shall announce the candidates so nominated. Any two Members, supported by eight other Members, may thereupon nominate in writing any duly qualified person to fill any such vacancy by forwarding such nomination, together with the written consent of such person to accept office if elected, to the Secretary within seven days after such meeting. Thereupon, if any other candidate or candidates has or have been duly nominated in addition to those proposed by the Council, a ballot list, containing the names of all persons duly nominated to fill such vacancies on the Council, stating which persons are nominated by the Council and giving the names of the two Members by whom every other person is nominated, shall be forwarded to the Members, Associate Members, and Associates of the Institution not less than seven days before the Annual General Meeting; and each Member, Associate Member, or Associate shall be at liberty to make a selection from such list, provided the number of names so selected shall not exceed in

any case the number requisite to fill the vacancies; but, if no other candidate or candidates has or have been so nominated, those proposed by the Council shall be considered duly elected. Ballot papers shall be so marked and recorded as may be from time to time determined by the Council.

46. All other Officers shall be elected by the Council, and notice of such election shall be given to the Institution at its next Meeting.

47. The Salaries to be given to the Officers of the Institution whose offices are not honorary shall be fixed by the Council.

PROCEEDINGS, POWERS, AND DUTIES OF THE COUNCIL.

48. The Council shall direct and manage the property and affairs of the Institution, and may effect any loan necessary in the judgment of the Council for meeting the current expenses of the Institution. The Council may, with the authority of a resolution of the Members, Associate Members, and Associates in General Meeting, borrow moneys for other purposes of the Institution on the security of the property of the Institution. The Council shall prescribe such rules and regulations in reference to the Ronalds Library, and the inspection thereof, as to them may seem reasonable, and generally they shall do everything and execute all such instruments as may be necessary in the judgment of the Council for giving full and complete effect to the Trust Deed affecting the Ronalds Library.

49. The Council shall meet as often as the business of the Institution may require; and at every Meeting five shall constitute a quorum. The Council may appoint Committees chosen from their own body, and Committees for special purposes consisting of Members of Council and Members, Associate Members, or Associates of the Institution and others, with such powers as the Council may prescribe. In the absence of the President, the senior Vice-President, or, if none be present, the senior Member of Council present, shall take the Chair at Council Meetings, and at all Meetings, whether Ordinary, Extraordinary, or Special.

50. Questions shall be decided at any meeting of the Council by the votes of the Members of Council present, each of whom shall, save as hereinafter mentioned, have one vote; provided that, in the event of a ballot being demanded by at least three of such Members at any meeting, if there be more than four past Presidents at such meeting, the vote of no past President save those of the immediate past President and the four senior past Presidents present and voting shall be counted in such ballot; but the Chairman shall have a casting vote. At the desire, expressed in writing, of any two Members present, the determination of any subject may be postponed to the succeeding Meeting of the Council, but not beyond that Meeting unless required by a majority of such Meeting.

51. The Council shall (in accordance with the provisions of the Memorandum of Association) at all times cause to be kept, in appropriate books, proper and sufficient accounts, in all necessary detail, of the Capital, Funds, Receipts, and Expenditure of the Institution, so that the true financial state and condition of the Institution may be at all times exhibited by such accounts.

52. The financial year of the Institution shall end on the 31st December in each year; and a statement of the funds of the Institution, and of the receipts and expenditure during such financial year, shall be made, under the direction of the Council, each year, and, after having been verified and signed by the Auditors and approved by the Council, shall be laid before the Annual General Meeting next following.

53. It shall be the duty of the Council to adopt all due means for the advancement of the Institution; to provide for properly conducting its business in all cases of emergency; and to arrange for the publication, in such a way as they may deem advisable, of the papers read at Meetings of the Institution, and of such papers read at meetings of Local Sections as may be selected by the Council for the purpose, and of such documents as may be calculated to advance Electrical knowledge.

54. The Council, when they may consider it expedient to

propose the enactment of any new Article, or the alteration or repeal of any existing one, shall summon the necessary Special General Meetings of Members to decide the same; and the Council are at all times bound to summon such a Special General Meeting on a requisition, in writing, of any ten Members, Associate Members, or Associates, specifying the particular new Article, or the alteration of an existing one, which they recommend.

SESSIONS AND MEETINGS.

55. The Sessions of the Institution shall commence on or about the 1st November of each year, and terminate on or about the 1st June of the following year. During these months the Meetings of the Institution will be held at such places and on such evenings as the Council may appoint, and notice of the dates so appointed will be given to Members, Associate Members, and Associates by the Secretary before the commencement of each Session.

56. The General Meetings of the Institution shall be held as follows:—1st. Special General Meetings of Members only, for the purpose of altering the Articles of Association. 2nd. The Annual General Meeting. 3rd. Ordinary General Meetings. 4th. Extraordinary General Meetings. 5th. Special General Meetings of Members, Associate Members, and Associates.

57. The Annual General Meeting of the Institution shall be the last Meeting of the Session, to receive and deliberate upon the Accounts and upon the Report of the Council on the state of the Institution, and to elect the Officers for the ensuing twelvemonth. At the Annual General Meeting any business may be transacted of which notice, in writing, shall have been given to the Secretary at least fourteen days before such Meeting.

58. No new questions shall be introduced, or motion be made, at an Ordinary General Meeting later than one hour and a half after the time fixed for the commencement of such Meeting.

59. The business of the Ordinary General Meetings of the Institution shall be conducted as nearly as possible in the following order:—

1. The Minutes of the preceding Meeting to be read and confirmed, and signed by the Chairman.
2. Business arising out of the Minutes so read.
3. Candidates for admission to be announced.
4. Transfers to a higher class to be announced.
5. Presents to be announced and acknowledged.
6. Communications from the Council to be brought forward.
7. New communications to be read.
8. Questions for discussion to be brought forward.
9. Candidates for admission to be balloted for.
10. Other business (if any).

60. Every Member, Associate Member, and Associate shall have the privilege of introducing one visitor, to be present at an Ordinary General Meeting of the Institution, on writing his name in a book provided for that purpose, or sending with him a card signed with his name, according to a form provided.

61. No question shall be discussed, or motion be made, at the Ordinary General Meetings relative to the direction and management of the concerns of the Institution.

62. A Special General Meeting of Members, Associate Members, and Associates may be called at any time by the Council for a specific purpose relative to the direction and management of the concerns of the Institution, and the Council are at all times bound to call such a meeting on a requisition, in writing, of ten Members, Associate Members, or Associates, specifying the nature of the business to be transacted. But such Special General Meeting of Members, Associate Members, and Associates shall have no power to make, alter, revoke, or dispense with any Article whatsoever, such power being vested only in a Special General Meeting of Members.

63. Members, Associate Members, and Associates shall have five days' notice of the time appointed by the Council for such Special General Meeting of Members, Associate Members, and Associates; and the notice shall specify the nature of the business to be transacted, and no other business shall be transacted at that Meeting. All Members, Associate Members, and Associates, except those disqualified under Article 36, shall have a right to attend and vote.

64. No one entered on the Register of Members other than Members, Associate Members, or Associates shall have the right to vote at any Meeting of the Institution.

LOCAL SECTIONS.

65. The Council may, at their discretion, upon receipt of a request to that effect from a sufficient number of Honorary Members, Members, Associate Members, Foreign Members, or Associates, resident in any district, create a Local Section of the Institution in such district, and they shall also have power to dissolve such Section at any time after it has been formed.

66. Each Local Section shall be constituted, and its affairs shall be carried on, in accordance with the rules and regulations to be laid down from time to time by the Council, and it shall elect annually for its Chairman or President a Member of the Institution, who, during his period of office as Chairman or President of the Section, shall be *ex-officio* a Member of Council.

67. The Council may, at their discretion, upon receipt of a request to that effect from any Local Society with objects kindred to those of the Institution, arrange for the union, alliance, or incorporation of such Society with the Institution; and may also if they think fit remit or reduce the entrance fees of the Members of such Society at the time of union, alliance, or incorporation.

PROPERTY AND FUNDS.

68. The property and funds of the Institution, and generally all its personal estate, and all its real estate (if any), may be sold

or disposed of by or according to the order and direction of the Council, sanctioned by a Special General Meeting of Members, Associate Members, and Associates.

69. Any donation in aid of the funds of the Institution may be accepted by the Council and Treasurer.

70. All the moneys of the Institution in excess of such current balance in the hands of the Treasurer as the Council shall from time to time authorise or require the Treasurer to keep in hand to meet the current expenses of the Institution, shall be invested in any of the Public Funds or Government Securities, or with the Commissioners for the Reduction of the National Debt, Debentures, or Debenture Stock, or Preference Shares or Stock of any British Railway Company paying a Dividend on its Ordinary Share Capital, the Stocks, Shares, or Debentures of any East Indian or Colonial Railway or other Company receiving or entitled to a percentage or fixed Annual Dividend from the Indian or Colonial Governments, or in the public Stocks or Funds of any Colony or Dependency of the United Kingdom, or on Security of Rates of any Municipal Corporation, or upon Real Securities, or in the purchase of Freehold or Leasehold Hereditaments in England, or in any mode in which Trustees are or shall be by law, in absence of special direction, authorised to invest trust moneys under their control.

71. The receipt in writing of the Treasurer, or of such other Officer or Officers of the Institution as may be authorised by the Council to receive moneys on account of the Institution, for any moneys payable to them on account of the Institution, or which shall in any manner become payable to or for the purposes of the Institution, shall be an effective and complete discharge for any such moneys.

72. All moneys, funds, and securities belonging to the Institution, in the hands or under the control of Trustees, if any, or the Treasurer or Bankers of the Institution, or for which the Treasurer or the Bankers may be accountable, may be withdrawn therefrom, either pursuant to a Resolution of the Council

or of a General Meeting, and in such manner as by any such Resolution shall be from time to time directed.

73. Every paper presented to the Institution, and accepted for reading or for publication in the Journal, and the copyright thereof, shall be the property of the Institution, unless there shall have been some previous arrangement to the contrary. But the Council, in such cases as they may think fit, shall have power to release or surrender their rights in respect of any such paper or the copyright thereof.

74. Each Member of the Council shall be accountable in respect of his own acts only, and shall not be accountable for any acts done or authorised to which he shall not have expressly assented. And no Member of the Council shall incur any personal liability in respect of any loss or damage incurred through any act, matter, or thing done, authorised, or suffered by him, being done in good faith for the benefit of the Institution, although in excess of his legal power.

75. The Members of the Council shall be indemnified out of the funds and property of the Institution from and against all costs, charges, damages, and expenses whatsoever which they or any of them shall sustain by reason of their respectively accepting office or acting in execution of the duties or powers imposed upon or given them by the Articles of Association.

COMMON SEAL.

76. The Council may provide a Common Seal of the Institution, and make rules for the safe custody of the same, and for the use thereof, and it shall never be used except by the authority of the Council previously given, and in the presence of two Members of the Council at the least, who shall sign every instrument to which the seal is affixed, and every such instrument shall be countersigned by the Secretary, or some other person appointed by the Council.

77. The Institution, acting by the Council, may exercise all the powers given by the Companies' Seals Act, 1864.

NOTICES.

78. A notice may be served by the Council or Secretary of the Institution upon any Honorary Member, Member, Associate Member, Foreign Member, Associate, or Student, either personally or by sending it through the post in a prepaid letter addressed to such Honorary Member, Member, Associate Member, Foreign Member, Associate, or Student, at his address, as registered in the books of the Institution.

79. Any notice, if served by post, shall be deemed to have been served at the time when the letter containing the same would be delivered in the ordinary course of the post; and in proving such service it shall be sufficient to prove that the letter containing the notice was properly addressed and put into the Post Office.

80. No Honorary Member, Member, Associate Member, Foreign Member, Associate, or Student, not having a registered address within the United Kingdom, shall be entitled to any notice; and all proceedings may be had and taken without notice to such Honorary Member, Member, Associate Member, Foreign Member, Associate, or Student, in the same manner as if he had had due notice.

SCHEDULE.

A

The Institution of Electrical Engineers.

A. B of

being years of age, and desirous of admission into THE INSTITUTION OF ELECTRICAL ENGINEERS, we recommend him, from personal knowledge, as a person in every respect worthy of that distinction.

[*Here specify distinctly the qualifications of the Candidate.*]

On the above grounds, we beg leave to propose him to the Council as a proper person to be admitted into the Institution, and in case of election he authorises the Secretary to place his name on the Register of members.

Signed, (C. D.) M.I.E.E., PROPOSER.
,, (E. F.) M.I.E.E., SECONDER.

Dated this day of 1 .

We, the undersigned, concur in the above recommendation, being convinced that A. B. is in every respect a proper person to be admitted into the Institution.

Signed

The Council, having considered the above recommendation, present A. B. to be balloted for as

of THE INSTITUTION OF ELECTRICAL ENGINEERS.

Chairman.

Dated day of 1 .

A A

The Institution of Electrical Engineers.

A B of

, who 22 years of age on the

day of 1 , being desirous of admission into THE INSTITUTION OF ELECTRICAL ENGINEERS as a Student, I recommend him, from personal knowledge, as a person in every respect worthy of that distinction.

[*Here specify distinctly the qualifications of the Candidate.*]

On the above grounds, I beg leave to propose him to the Council as a proper person to be admitted into the Institution, and in case of election he authorises the Secretary to place his name on the Register of Students.

M.I.E.E.
Signed, (C. D.) or
A.M.I.E.E.

Dated this day of 1 .

The Council, having considered the above recommendation, present A. B. to be balloted for as a Student of THE INSTITUTION OF ELECTRICAL ENGINEERS.

Chairman.

Dated day of 1 .

B

The Institution of Electrical Engineers.

We, whose names are hereunto subscribed, from personal knowledge of the Candidate, submit to the Council of THE INSTITUTION OF ELECTRICAL ENGINEERS the propriety of transferring

A ______________ B ______________

of ______________ from the class of ______________

in which he is now registered, to the class of ______________

because

[*Here specify distinctly the qualifications of the Candidate.*]

Signed this ______ day of ______________ 1 ___ .

(C. D.) M.I.E.E. (E. F.) M.I.E.E.

We, the undersigned, concur in the above recommendation, being convinced that A. B. is in every respect a proper person to be admitted into the class of ______________ .

Signed ______________

The Council of THE INSTITUTION OF ELECTRICAL ENGINEERS, meeting on the ______ day of ______________ 1 ___ , transfer ______________

from the class of ______________ to the class of ______________

Signed ______________

Chairman.

C

The Institution of Electrical Engineers.

SIR,

I have the honour to inform you that on the ______ day of ______ you were elected ______ of THE INSTITUTION OF ELECTRICAL ENGINEERS, and I beg to transmit to you a copy of the Memorandum and Articles of Association of the Institution.

According to the Articles of the Institution, you are required, as ______, to sign the enclosed form and to pay the sum of ______, being Entrance Fee £______, and Annual Subscription £______, within one month of your receipt of this notice, otherwise your election will be void. These conditions being complied with, you will be considered as admitted into the Institution; and any publications or notices to which you are entitled will be forwarded according to your directions.

I beg to direct your attention to Articles Nos. 28, 34, 35, and 36, and to point out that all annual subscriptions become due on the 1st of January in each year.

I am, Sir, etc.

D

The Institution of Electrical Engineers.

SIR,

I am directed by the Council of THE INSTITUTION OF ELECTRICAL ENGINEERS to inform you that, upon mature consideration of a proposal which has been laid before them relative to you, they will call a Special General Meeting thereon, as is provided for by Article 38 of the Articles of Association of this Institution. But previous to taking such a step it becomes the duty of the Council to advise you to withdraw from the Institution, which can be done on your complying with Article 35.

I am, Sir, etc.

E

The Institution of Electrical Engineers.

SIR,

It is my duty to inform you that, by a Resolution of a Special General Meeting of Members, Associate Members, and Associates of THE INSTITUTION OF ELECTRICAL ENGINEERS, which was held on the ________ day of ________________ 1____, according to the provisions of Article 38 of the Articles of Association, you were declared to be no longer belonging to the Institution.

I am, Sir, etc.

F

The Institution of Electrical Engineers.

Founded 1871. Incorporated 1883.

This is to certify that ________________ was admitted a ____________ of THE INSTITUTION OF ELECTRICAL ENGINEERS.

Witness our hands and seal at Westminster this ________ day of ____________ 1 .

L.S.

________________ *President.*

________________ *Secretary.*

G

The Institution of Electrical Engineers.

I, the undersigned, having been elected a ____________ of the above Institution, hereby request you to register me as a ________ of the said Institution.

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